CHAPTER SEVEN
Philosophical Assessment of General Equilibrium Models

In assessing the models we have just studied, we face many questions. Theories and models serve many different purposes. A model may efficiently guide research, although no theoretical hypotheses employing it are of predictive or explanatory value. Like Ptolemy's account of planetary motion, a theory may make accurate predictions without actually explaining anything. My main concern here is whether general equilibrium models enable economists to explain adequately the principal phenomena of capital and interest. Those who deny that explanation is an important aim of economics or of science generally will find this question misguided. But it remains a question that economists and laymen ask. I shall implicitly defend its importance.

To assess the explanatory power of general equilibrium theories, we need to know what criteria scientific explanations must satisfy. Unfortunately there is no well-supported consensus among philosophers of science concerning these criteria. I cannot thus simply list a number of conditions which general equilibrium theories must satisfy and then check whether the theories do satisfy them. We must proceed cautiously and sensitively. General equilibrium theories apparently fail to satisfy criteria some philosophers have argued that explanations must satisfy. Some of these failures reveal the inadequacy or misapplication of the philosophical criteria. I shall argue, however, in §4 that the simplifications general equilibrium theories employ cannot legitimately be used in explanations. For this reason general equilibrium models cannot yet provide the basis for an explanatory theory of capital and interest.

1. Explanation

The much abused starting point for modern philosophical discussions of explanation is Carl Hempel’s deductive-nomological model (1965:331–496). According to Hempel, a necessary condition for giving a scientific explanation is deducing a description of the event or state of affairs to be explained from a set of true statements, some of which are lawlike. Schematically we have:

laws
true statements specifying the circumstances
statement of what is to be explained

where the line represents a deductive inference. There is, according to Hempel, a second kind of explanation, an “inductive-statistical” explanation, which one employs when the laws are not deterministic. In statistical explanation one does not deduce what one wants to explain. Instead, one only shows it to be highly probable. For simplification, I shall focus on the deductive-nomological model. Most of my discussion, however, applies (with some rephrasing), to both models. That one deduces in a nonstatistical explanation the description of the event or state of affairs to be explained from a set of true statements, some of which are lawlike, is only a necessary condition. Sufficient conditions are not well understood and are sensitive to the particular context. Hempel is, of course, not just proposing a “model” in the sense discussed in chapter 3.

The deductive-nomological model has been much criticized. Indeed, one familiar with these criticisms might wonder why I focus on deductive-nomological explanation. I have two reasons. First many of the major criticisms of this model challenge its demand that scientific explanations be explanatory arguments (Scriven 1959, 1963; Salmon 1971). These criticisms are not germane here, since purported explanations of phenomena of capital and interest that employ general equilibrium models are arguments. As a set of necessary conditions on explanatory arguments, the deductive-nomological model has faced fewer criticisms and has fewer rivals. My major reason for focusing on the deductive-nomological model is that I know of no better analysis of explanatory arguments.¹ The grounds for this assessment of Hempel’s model of statistical explanation are serious and Salmon’s alternative is attractive. I shall, however, discuss neither these criticisms nor Salmon’s model. Since apparent explanations employing general equilibrium models are arguments (proofs) and since the statements in such explanations are not statistical in form, it is simplest to focus on nonstatistical explanation and the deductive-nomological model. Ignoring statistical explanations and their many difficulties will create strains in my exposition, particularly since I shall argue in §3 that ultimately one must regard many economic explanations as statistical. The only alternative would be an elaborate and largely irrelevant discussion of statistical explanation. Purported explanations of phenomena of capital and interest that rely on general equilibrium theories are inadequate on any reasonable model of explanation I know of. This chapter employs the simplest model of explanatory arguments.

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pel's analysis will not be clear until §6. Despite the difficulties I shall discuss in §2 and §4, I shall defend the deductive-nomological model as an analysis of explanatory arguments and shall use it to assess general equilibrium accounts of capital, interest, and exchange values.

Talk of scientific models does not fit gracefully into Hempel's analysis of explanations. I have argued (chapter 3) that we should regard models as sets of assumptions that define a new predicate. Theoretical hypotheses assert that the new predicate is true of something. From those hypotheses we may derive the closure of the assumptions of the model. The derived set of statements is a theory. Models thus contribute nothing to explanation until theoretical hypotheses are offered. The laws which are needed in a deductive-nomological explanation will be closures of assumptions of a model. The detailed specification of the relevant circumstances will usually not be derivable from a theoretical hypothesis, yet closures of assumptions of models need not always be lawlike. Models may contain only lawlike assumptions, like the model of a classical particle system, or they may also contain simplifying assumptions. If one were to assert that some actual economy is in intertemporal general equilibrium, one would not only be making lawlike claims. One would also be claiming the commodities are infinitely divisible, that agents have perfect information about the economic future, and so forth. Let \( S_1, \ldots, S_n \) be the statements in a deductive-nomological explanation, not including the statement of what is to be explained. Let \( S_1, \ldots, S_j \) be the laws and \( S_{j+1}, \ldots, S_n \) be statements in the explanation implied by theoretical hypotheses. I am claiming that \( 1 \leq j \leq k \leq n \).

If explanatory arguments must be deductive-nomological (or inductive-statistical), must we not conclude immediately that general equilibrium theories cannot be used to explain the phenomena of capital and interest, or, for that matter, anything else? In Hempel's models, all of the statements must be true. Suppose one were to take the general equilibrium model of chapter 5, add the theoretical hypothesis that the economy of the United States in 1978 and 1979 was an equilibrium economy of the sort defined by the model, fill in initial conditions somehow, and deduce a real rate of interest which happens by some miracle to agree with the actual one-period real rate of interest. Surely one would not have provided a deductive-nomological explanation of the rate of interest. The theoretical hypothesis in this case is obviously false. Moreover, the difficulties are not confined to the particular case. All current general equilibrium theories contain statements that appear to be false. Some, like the model in chapter 5, will falsely assert that all individuals have perfect information concerning the prices and availability of commodities and concerning the production possibilities. Many will falsely assert that the preferences of all consumers are transitive.

To conclude, without further analysis, that general equilibrium models can have no role in explanations would be to misapply pedantically the deductive-nomological model. Employing that model in this finicky way, one can criticize virtually all explanatory arguments scientists offer. Either the deductive-nomological model is much too demanding and needs replacing, or one must find a more flexible way to apply it.

Consider what happens when natural scientists attempt to deal with complicated everyday phenomena. Take the trite example of the path of a leaf's fall. In what sense can it be explained by physicists? Precise deductive-nomological explanation seems out of the question. Scientists cannot get exact information concerning all relevant initial conditions and cannot do all the complicated calculations that would be necessary if they did have the pertinent data. If the only stumbling blocks were these problems of knowing the initial conditions and of calculation, physicists would have what I shall call "an explanation-in-principle" of the leaf's path. Scientists would know all the relevant laws and causal factors. Even though they could not now and never could explain the path in detail, there would be nothing mysterious about the trajectories of falling leaves and nothing general or theoretical to learn about them. Explanations in principle appear to be genuine explanations.

Scientists justify their explanations in principle by developing theories they test in simpler circumstances. In the case of the falling leaf, one tests the theoretical hypothesis that the model of gravitation applies to ordinary bodies falling in evacuated chambers or that it applies to the motion of the planets. Theories of fluids and of resistance are also developed and tested with respect to simpler situations. It is difficult to know whether one has taken account of all the laws which bear on a leaf's tossing and gliding. Perhaps telekinesis is a significant phenomenon with leaves. Scientists have good reason to believe they know all the relevant laws when, with simplifications concerning initial conditions, they can make roughly correct predictions concerning falling leaves and are able to cite the factors responsible for any appreciable errors.\(^2\) Philosophers must grant that such explanations in principle are truly explanations. If the deductive-nomological model does not

\(^2\) Achieving a theory of falling leaves in this way is employing what J. S. Mill called the deductive method (1843, bk. III, ch. 11; bk. VI, ch. 9). Mill believed that economic laws are established inductively by psychology and the natural sciences and that economists then develop economic theory deductively (1843, bk. VI, ch. 9; 35; Cairns 1888:710).
permit this recognition, it is inadequate. The best evidence for or against a model of explanation is the congruence between the model and the achievements of scientists.3

Even explanation-in-principle is a tall order. I doubt that physicists can now explain in principle the path of a falling leaf. Economists certainly cannot explain in principle local or overall characteristics of real economies. Not only are the purported laws of economics difficult to confirm—perhaps because of the difficulties of setting up simplified experimental situations—but theorists know these “laws” are inadequate. Economists are ignorant of many relevant laws. They leave out of account significant causal factors. At best theorists believe that they have got what, following J. S. Mill, I shall call an “inexact science”—an account of the principal causal factors involved.

Inexact sciences appear to be explanatory. We believe that physicists can explain the paths of falling leaves even if they cannot give a full explanation in principle. The theory of gravitation explains some of the characteristics of tides, even though theorists remain ignorant to what Mill calls the “minor causes” (1843, bk. VI, ch. III, §1). We need to understand what such “inexact explanation-in-principle” is and when scientists are justified in believing that they have given one. I shall argue that, appearances to the contrary, one may regard many such explanations as deductive-nomological. I shall, moreover, show that even when one appreciates such inexact explanation-in-principle, one is still forced to conclude that current general equilibrium models do not enable economists to explain the main phenomena concerning capital and interest.

2. Inexact Laws

Economists have long recognized that they are unable to give precise explanations and that the general statements they rely on are, if interpreted naively, false. In defense of these general statements (and thus of the explanations which employ them), most economists would offer one or both of the following claims:

1) The lawlike assumptions of the basic equilibrium model are not precisely true of actual people and technologies, but in the relevant circumstances they are sufficiently correct and the failures (with certain exceptions) sufficiently random and insignificant that one may nevertheless rely

3 Within this sentence lurk puzzles which I discuss in the postscript.

These claims, as I have stated them, are vague and ambiguous. Various economists and philosophers have attempted to provide a firmer defense for the basic “laws” of equilibrium theory by refining these claims or by substituting what they regarded as philosophically more sophisticated views. Before discussing the views of others or the claim that economics is concerned with idealizations, we must analyze the inexactness of equilibrium theories. Not only, as we have suggested, may lawlike generalizations in explanatory theories be inexact or incomplete, but also many economists already believe that economic theory should be understood as somehow inexact or close to the truth.

In considering the inexactness of various economic theories, I am not merely calling attention to the fact that economists are able to make only inaccurate or imprecise predictions. They may be unable to make accurate predictions or to explain in detail merely because of difficulties in specifying the initial conditions or because of limitations in their mathematical powers. Not only are the implications of economic theories inaccurate or imprecise, but the basic generalizations, the nine “laws” of equilibrium theory, themselves appear to be inexact.

There are several different ways in which one might attempt to analyze inexact laws. Sometimes generalizations are inexact because they are approximate. They are not true as stated, but they can be made true merely by specifying a margin of error in a certain domain. Kepler’s Laws are in this sense approximate. Within a certain percentage of the calculated angular velocities or periods of revolution, these laws appear to be true. By “blurring” what the laws assert (and thus imply) one gets exceptionless true generalizations. It seems unpromising, however, to interpret basic economic generalizations as approximate laws principally in this sense. Suppose in a recession it maximizes profit for several companies each to lay off 1000 workers. If no company laid off fewer than 800 or more than 1200, then the evidence would confirm (within a margin of error of 20%) that firms attempt to maximize profits. Economic behavior is, however, more complicated. One can reduce, but not eliminate, the disconfirmations of economic generalizations by specifying a margin of error. Some
firms feel responsible for their employees and accept losses rather than lay them off. Other sorts of inexactness are involved.

Second, one might regard the lawlike claims of inexact sciences as probabilistic or statistical claims. Such an interpretation is implicit in the claim economists often make that only the aggregate consequences of the basic "laws" are significant (Hicks 1946:11). Yet this interpretation is problematical. To regard all empirical laws as probabilistic, as McClelland does (1975, ch. 1), seems to confuse the results of testing with what laws assert. The basic "laws" of equilibrium theory do not appear to involve elements of chance or randomness; they merely appear to have counterexamples. To construe all generalizations that face counterexamples as probabilistic is merely to rechristen them. Although I shall argue in the next section that we must regard some of the basic "laws" of equilibrium theory as statistical, this conclusion is not a happy one. It is best, for the reasons given, to attempt to construe the inexactness of economic "laws" in some other way. I shall make that attempt, but it does not fully succeed.

Perhaps one should deny that the basic generalizations of equilibrium theory are laws. They are rough generalizations which in certain applications work well enough. They are useful oversimplifications. Calling the inexact "laws" of equilibrium theory rough generalizations is appealing, but it is not informative until we know what a rough generalization is. If rough generalizations are to be analyzed in the various ways in which I am interpreting inexact "laws," "rough generalization" turns out to be only another term for "inexact law." What I have in mind in suggesting that one might attempt to analyze inexactness in terms of rough generalizations is that one might try to understand an inexact claim as something quite different from a law. Perhaps philosophers need to recognize that scientists sometimes employ in place of laws in their explanations a different kind of assertion, which may not be true.

This suggestion is problematic. One can appreciate that a false statement may help one to predict and control a certain range of phenomena, but how can a false statement explain anything or help one to understand how things truly are? What special features distinguish rough generalizations from simple falsehoods and permit one to use them in giving explanations? A philosopher might suggest, for example, that a rough generalization has explanatory worth only if it possesses a certain reliability, does not appear accidental, and enables scientists to systematize phenomena. These are plausible conditions. A generalization which satisfies them no longer appears, however, decisively unlike a law. No one has yet offered a cogent model of explanatory arguments that employ rough generalizations which is clearly different from established models of explanations employing laws. Until we have such a model, we are driven to interpret these inexact generalizations as laws of some sort. To say that inexactness is a matter of roughness is not to offer an incorrect analysis; it is not to offer any analysis at all.

Inexactness is, I believe, often not a matter of approximation or of statistics. Inexact laws are instead often qualified with implicit ceteris paribus (other things being equal) clauses. Scientists may assert some lawlike statement only with the proviso that other things are equal or that there are no unspecified interferences. Approximate claims may involve ceteris paribus qualifications in addition to a margin of error or a statistical restriction. Rosenberg has recently defended ceteris paribus qualifications in economics and in sciences generally (1976b:134–38). I agree with him that ceteris paribus qualifications may be legitimate. We shall see, however, that the inexactness of equilibrium theory goes beyond implicit qualification with ceteris paribus clauses.

A law which contains a ceteris paribus clause need not be inexact if the clause can be replaced by precise qualifications. Such replacement is not, however, possible in any inexact science. In this regard equilibrium theory is typical of inexact sciences. In asserting that, ceteris paribus, the preferences of consumers are transitive, economists are saying that in the absence of "interferences" or if the "interfering factors" are held constant, consumer preferences are transitive. Economists can enumerate some of the interfering factors from which they abstract. One should, for example, assume that the consumer's tastes do not change. One should assume that the consumer's memory is unimpaired. Theorists cannot, however, list all the possible interferences and replace the ceteris paribus clause with a precise qualification.

Is it sensible to regard statements so vaguely qualified as laws? (See Hutchison 1938:40f). Statements with such qualifications are dubious candidates for laws. It is not the case that ceteris paribus, we are all immortal; or ceteris paribus, that ravens are pink. Not all appeals to ceteris paribus qualifications to explain away apparent disconfirmations are legitimate or perhaps even make sense. One who regards the laws of inexact sciences as vaguely qualified claims must make clear what they assert and must distinguish legitimate from illegitimate uses of ineliminable ceteris paribus clauses. What do sentences with such clauses say? When, if ever, can one justifiably regard them as laws? John Stuart Mill's discussion of inexact sciences is suggestive here.
According to Mill, in an inexact science

the only laws as yet accurately ascertained are those of the causes which affect the phenomenon in all cases, and in considerable degree; while others which affect it in some cases only, or, if in all, only in a slight degree, have not been sufficiently ascertained and studied to enable us to lay down their laws, still less to deduce the completed law of the phenomenon, by compounding the effects of the greater with those of the minor causes. (1843, bk. VI, ch. 3, §1).

The example Mill gives is the science of tides. Physicists know the laws of the greater causes, the gravitational pull of the sun and the moon, but are ignorant of the laws of the minor causes like the configuration of the ocean bottom. The model Mill has in mind when he speaks of “compounding the effects” of causes is the vector addition of forces in mechanics. The notion that economists know and employ only the “laws” of the “greater causes” seems compelling. The “other things” which theorists hold equal are the lesser causes.

The intuitive picture is that many causes influence economic phenomena. Economists focus on a few factors which they believe to be major or distinctive causes. Since economists consider only some of the causes, their generalizations need ceteris paribus qualifications; otherwise the omitted causes would sometimes lead to disconfirmations. The claims of economics are true only under special (and not fully specified) conditions. One can regard economic generalizations with their qualifications as laws whenever one has reason to believe that these “laws” truly capture independently functioning or “greater” causes within some domain.

We need more than such an intuitive picture to assess general equilibrium theories of capital and interest intelligently. What precisely is a ceteris paribus clause? How are sentences with such clauses to be interpreted? How can they be true?

The same sentence can say different things in different contexts. Following Stalnaker (1972:380–97), let us distinguish the meaning of a sentence, the context-invariant interpretation, from the content of a sentence (or the proposition expressed by a sentence) which may change from context to context. “I hate my economics class” has a single meaning, but its content varies depending on who utters it when. Stalnaker suggests that one should regard the meaning of a sentence as a function from contexts (which can be characterized as sets of a certain kind) to contents or propositions (which can also be given a set-theoretic characterization). The meaning of a sentence determines a content in a given context.

Adapting this terminology, I suggest that ceteris paribus clauses have one meaning, “other things being equal,” which in different contexts picks out different predicates (not propositions). It is the context, the economist’s background understanding, that fixes what the “other things” are and what it is for them to be “equal.” Although the phrase, “ceteris paribus”, has an invariant meaning, its content, the predicate it picks out, varies greatly from context to context.

The phrase “ceteris paribus” does not determine a predicate in every context. Sometimes in uttering a sentence containing such a clause one fails to express a proposition. An example was my writing above that ceteris paribus, all ravens are pink. The phrase “ceteris paribus” may pick out no predicate here. The predicates ceteris paribus clauses pick out in different uses vary greatly in clarity and precision. Coulomb’s law, for example, says that in the absence of other forces, or holding other forces equal, any two like charged bodies will repel one another with a force directly proportional to the product of their charges and inversely proportional to the distance between them.

The phrases “in the absence of other forces” or “holding other forces equal” are refined ceteris paribus clauses. They have a more precise meaning than do the words, “ceteris paribus” in an assertion like “Heavy bodies will, ceteris paribus, fall when dropped.” The qualification on Coulomb’s law picks out relatively precise (though always open-ended) predicates. The law of diminishing returns with its “holding other inputs constant” clause is similar, although it is usually interpreted as carrying a vague qualification as well. Even the predicates determined by these “refined” ceteris paribus clauses do not have precise extensions. Although there are formal difficulties with vague predicates, such predicates abound in both science and ordinary language. We cannot now do without them.

What proposition does a qualified law express? Suppose the law is formulated as “Ceteris paribus everything which is an F is a G.” (Not all the “laws” of equilibrium theory have such a simple form, but I shall ignore these complications.) Consider first the unqualified generalization, “Everything which is an F is a G.” Modern logicians interpret sentences with this form to mean that there is nothing in the extension of the predicate F which is not in the extension of the predicate G. The extension of a predicate is the set of all things of which the predicate is true. Provided that the extensions are non-empty, one

* Sentences with ceteris paribus clauses may be false. In the particular case (and context) I believe that the clause does not pick out any predicate and that no proposition is expressed.
can represent what "Everything which is an $F$ is a $G$" asserts in the accompanying set diagram (fig. 7.1).

The box denotes the domain or universe. The interior of the larger circle represents the set of all things which are $G$. The interior of the smaller circle represents the set of all things which are $F$. Since the latter is contained in the former, we can see that all $F$'s are $G$'s. Notice that there is nothing in the diagram which distinguishes generalizations like "All humans are mortal" from generalizations like "All red roses are red" or "All coins in my pocket are nickels."

In the case of qualified generalizations like "Ceteris paribus everything which is an $F$ is a $G$," some things which belong to the extension of $F$ do not belong to the extension of $G$—otherwise one would not need the qualification. In my view "Ceteris paribus everything which is an $F$ is a $G$" is a true universal statement if and only if the ceteris paribus clause picks out a predicate, $C$, and everything which is both $C$ and $F$ is $G$ (fig. 7.2).

Considering only the interior of circle $C$, one sees that all of region $F$ contained there is also contained in region $G$. In offering a qualified generalization, one is only asserting that, once the qualifications are met, all of region $F$ lies within region $G$. The predicate $C$ belongs in the antecedent of the law. I have drawn $C$ with dotted lines only to suggest that we do not know precisely what the definite extension of the ceteris paribus predicate is, and not to suggest that it does not have one.

Thus, in my view, to believe that, ceteris paribus all consumer's preferences are transitive is to believe that anything which satisfies the ceteris paribus condition and is a consumer has transitive preferences. One need not be disturbed by intransitive preferences caused by, for example, changes in tastes, because such counterexamples to the unqualified generalization lie outside circle $C$. In my analysis sentences qualified with ceteris paribus clauses are sometimes genuine laws. A sentence with the form "Ceteris paribus everything which is an $F$ is a $G$" is a (nonstatistical) law, just in case the ceteris paribus clause determines in the given context a predicate, $C$, and everything which is $C$ and $F$ is also $G$.

When someone asserts "Ceteris paribus things which are $F$ are also $G$," he or she does not always intend to assert that everything which is $F$ and $C$ is also $G$. When people believe that, other things being equal, aspirin cures headaches, they need not believe that there is any set of conditions they can add to the taking of aspirin which are sufficient for headache cures. They might believe simply that the frequency of cures is high among people who take aspirin and satisfy the ceteris paribus clause. That is, in terms of the conditions of figure 7.2 one might believe that inside circle $C$ a small portion of region $F$ does not lie within region $G$. Imagine shifting circle $C$ slightly to the left.

Indeed sometimes scientists may believe that the true law will not involve the predicates $F$ or $G$ at all. One can grant that "Ceteris paribus (most) things which are $F$ are $G$" has some explanatory and predictive force, yet still expect to supersede this generalization in the course of further inquiry. The true law which lies behind the generalization that aspirins cure headaches probably involves chemical or neurological predicates. With various qualifications ($C$), however, the generalization is not without explanatory and predictive power. Scientists may hope through further study to discover precise chemical or neurological laws, but that hope gives one no reason to reject generalizations like "Aspirins cure headaches." Even if such hopes are fulfilled, the inexact generalization may be useful for various practical purposes. Such generalizations may still on my analysis be laws.
Influence and alter the total effect, but they do not affect its “operation.” When one has such “mechanical phenomena” the causal factor captured in the qualified law is responsible for a “tendency” in the phenomena which is present whenever the causal factor is. One can then compound these tendencies or these effects of the independent causes. In classical physics Newton’s law of gravitation or Coulomb’s law of electrostatic attraction or repulsion always specifies one component of the force on any body. Scientists may make mistaken predictions because they failed to consider other forces, but such mistakes show only that other things were not equal. They do not disconfirm the laws.

When one is not dealing with causal factors which are in this way independent or when one simply does not know whether or how various causal factors will interact, one may still use laws qualified with ceteris paribus clauses. Qualified laws dealing with nonmechanical phenomena will, however, be more provisional and will have a much more restricted scope. They may apply only when there are no interfering factors. Even if the basic generalizations of equilibrium theory are qualified laws, they will not help one to understand real economies with their inevitable interferences and complications, unless economic phenomena are mechanical phenomena. Mill simply asserts that economic phenomena are mechanical, that the basic economic causal factors continue to act as component forces in the total complicated effect (1843, bk, VI, ch. 7, §1). Such a supposition is implicit in most applications of economic models. Its only justification is the success of such applications.

Is my suggestion that we regard inexact or rough generalizations as qualified laws sensible? Since scientists do not know which predicate a ceteris paribus clause picks out, why regard it as picking out any predicate at all? Why regard generalizations like the basic “laws” of equilibrium theory as true or false? One can recognize that they may guide research and help economists to interpret data without regarding them as lawlike assertions and assessing their truth. If a theorist believes that in a certain domain the interferences inadequately denoted by the implicit ceteris paribus clause are absent, he or she can regard the generalization (in that domain) as a “virtual” law (Morgenbesser 1956, chs. 1, 2). But the theorist need not regard the unrestricted generalization as true or false. To regard inexact “laws” as thus schematic is appealing. It emphasizes the elusiveness of ceteris paribus clauses, which I have perhaps understated and emphasizes that scientists regard inexact “laws” differently when they use them to give explanations than when they rely on them in doing research.
These qualifications about regarding inexact generalizations as laws do not, however, challenge the position I have defended. There is nothing in my account which demands that one judge all sentences which contain *ceteris paribus* clauses to be true or false. Sometimes one finds that in certain domains the justification conditions (to be discussed below) are satisfied, while in others they are not. In such cases it may be best to regard the unrestricted “law” as schematic rather than as false. If forced to judge the truth of the general lawlike statement that, *ceteris paribus*, all agents choose that option which brings them the largest bundle of commodities, one would, I think, have to decide that it is false. In restricted domains like those consisting of market behavior, one might be justified in regarding versions of it as laws. It is best in this case not to regard the completely general claim as fully specified and not to judge its truth. In completely general form, non-satisfaction should be regarded as an assumption only. Theorists do, on the other hand, use basic economic “laws” to try to explain economic phenomena. In doing so they are no longer regarding these sentences as mere assumptions, but as expressing some truth, however rough it may be. Otherwise, their attempt to explain would be incomprehensible. In discussing laws and explanation here and in §6 below, I am concerned with applications of models, with qualified generalizations which in specified domains can be regarded as laws.

Countenancing qualified laws forthrightly, one is no longer forced to make invidious comparisons between the natural sciences which possess laws and provide adequate explanations and the social sciences which possess at best virtual laws and whose explanations are unsatisfactory. We have instead graduations of inexactness. Scientists strive for exactness, but possessing, as they often do, only qualified generalizations, they nevertheless have achieved some knowledge of a particular subject matter and are able to explain some of the phenomena in the domain.

Generalizations whose scope is not restricted are often best regarded as assumptions in models, not as assertions which are true or false. In making this last assertion I am not denying that one important goal of science is to explain nor that purely theoretical assertions can be true or false. I am only pointing out that sometimes it is unhelpful to assess the truth of unrestricted theoretical sentences—that they are sometimes better regarded as schemata than as statements. I am also not asserting that one must assess each applied theory separately and cannot reasonably accept the claims of some applied theories after testing others in related domains.

It is not enough to provide an interpretation of qualified generalizations which enable us to understand how they can be laws. We must discover when one has reason to believe that a qualified generalization is a law. Standard theories of confirmation provide little guidance, since scientists do not know precisely what the extension of the *ceteris paribus* predicate is (what the size and location of circle C is). When is one justified in regarding a statement with a *ceteris paribus* clause as a law?

I would like to suggest four necessary conditions. First the statement must be lawlike. It must be the sort of statement which, if true, would be a law. Philosopher do not agree on any analysis of lawlikeness, but scientists and laymen are able to distinguish lawlike from non-lawlike claims. People recognize a difference between accidental generalizations like “There are no poodles in Nebraska” and lawlike generalizations like “Copper conducts electricity.” No matter how lawlike statements are to be analyzed, they are not accidental and they support counterfactual or subjunctive claims.

Second, the qualified statement must be reliable. In some class of cases, after ignoring the *ceteris paribus* clause or allowing for specific interferences, the scientist should rarely need to explain away apparent disconfirmations. The class of cases considered must be specified in some independent way. “All roses are yellow” turns out to be highly reliable if tested in the class of yellow roses or in the class of roses my mother likes best. The reliability condition is a statistical condition. One takes samples from the independently specified domain of interest. A generalization like “Everything which is an F is a G” is reliable only if (after making allowances for specific interferences) almost all Fs sampled are Gs.

Third, one does not have good reason to regard a qualified claim as a law unless it is refinable. If scientists add specific qualifications, the generalization should become more reliable or reliable in a larger domain. Theorists may not be interested in actually refining the generalization. The uncomplicated original claim may be more convenient. Refinability only demands that scientists be able to make the generalization more reliable. Note that the refinability condition does not demand that theorists can completely replace the *ceteris paribus* qualification with specific provisos.

Finally, no one is justified in regarding a statement with a *ceteris paribus* clause as a law unless it is excusable. Intuitively, one should...

* In speaking of “greater causes” in the quotation above, Mill has in mind principally the case in which one can simply ignore the interferences and still have a reliable generalization. Sometimes, however, scientists need to and are able to make specific allowances for known interferences.
not invoke the *ceteris paribus* clause blindly. One should know which are the important interferences and should be able in most cases to justify relying on the *ceteris paribus* clause as an excuse. The excusability condition demands that, with only rare exceptions, which are puzzles demanding further research, scientists be able to point out the interferences and explain away failures of the unqualified generalization.\(^7\) The excusability condition differs from both the reliability and refi-nability conditions, because it does not demand good statistical results. Unlike the refi-nability condition it is also unconcerned with amending generalizations. Instead, the excusability condition demands that after scientists have done their tests and have identified those cases in which the generalization is not reliable, they be able to cite the interfering factor in all except possibly a few anomalous cases. It should not seem a miracle that the generalization ‘works’ sometimes and fails others.

One may regard a generalization that would face disconfirmation, if it were not qualified, as a law only if it is lawlike, reliable, refi-nable, and excusable. These conditions are schematic, since I am offering no account of lawlikeness, of theory assessment in general, or of how to specify in an ‘independent’ way a domain of application. Since some degree of inexactness infects general statements in all sciences, it might be better to incorporate such justification conditions into a general account of theory assessment. Since I know of no good general philosophical model of theory assessment, I cannot offer such an integrated account.

The lawlikeness, reliability, refi-nability, and excusability conditions themselves help to assess general equilibrium theories of capital, interest, and exchange values. They are, I believe, plausible and sensible. Not only are they a reasonable formulation of the implicit criteria by which scientists and laymen assess the legitimacy of invoking *ceteris paribus* clauses to explain away apparent disconfirmation; they are rational as well. Since one does not know precisely which predicate *C* the *ceteris paribus* clause expresses, one cannot test directly the explicit lawlike statement, ‘‘Everything which is *C* and *F* is *G.‘’ One should not believe that there is such a law until one finds a class of cases in which there is a reliable connection between *F* and *G*. If scientists could not explain away cases in which something is both *F* and not *G* they would not believe that they have a law. If scientists cannot find any qualifications which would lead them to find a more reliable connection between *F* and *G* they would once again deny that they have a law. Unless these four conditions are met, one cannot reasonably regard vaguely qualified generalizations as laws. Notice that these conditions are necessary, not sufficient. I am not asserting that one is necessarily justified in regarding any statement which satisfies them as a qualified law.

3. Inexact Laws in Economics

Economists have long recognized that their claims must be qualified. It should therefore seem natural to them to conceive of rough or inexact lawlike statements as qualified with *ceteris paribus* clauses. To assert that people’s preferences are transitive is to make a qualified claim. This assertion is not falsified by a change in tastes; such changes are ruled out by an implicit *ceteris paribus* clause. For the same reason, this assertion is not disconfirmed by panic behavior or by the efforts of an experimental subject to make a fool out of economists. Economists describe how agents behave against a fixed background or in the absence of various complications. Equilibrium models are intended to analyze the distinctive “causes” that operate in economic life, although these are modified and sometimes counteracted by other factors. Apparent failures are not falsifications, since economists have the implicit qualifications to invoke as excuses. *Ceteris paribus* qualifications will, in my view, never be eliminable in economics. Social phenomena are so interdependent and subject to so many influences, that it is futile to hope that economics can ever be an exact science. If one included all the “lesser” causes of economic phenomena, economics would merge with the other social sciences.

*Ceteris paribus* qualifications are powerful excuses, which must not be invoked too often; nor may they be invoked blindly. Is one justified in regarding the nine basic generalizations of equilibrium theory as qualified laws? Although they appear to be lawlike and refi-nable, they may not satisfy the reliability and the excusability conditions.

In sufficiently restricted domains generalizations of equilibrium theories are reliable: In agriculture there are diminishing returns to larger applications of fertilizers; people are for the most part less excited with their second television than with their first. It would be foolish to deny that the generalizations of equilibrium theory are ever reliable. But what can we say of general equilibrium theories of capital and interest? If we had to directly assess theoretical hypotheses which employ intertemporal general equilibrium models to derive conclusions concerning capital and interest, we would be forced to negative conclu-
interferences in such cases might be. They expect their generalizations to fail from time to time. With effort and ingenuity interfering factors might always be identified, but economists do not make such efforts. They do not make such efforts because they do not regard apparent disconfirmations as the result of specific interferences. The “laws” of equilibrium theory do not satisfy the excusability condition.

Since most of the lawlike claims of equilibrium theorists do not satisfy the excusability condition, one cannot regard them as qualified universal laws. Perhaps the basic assertions of equilibrium theory should be regarded as both implicitly qualified, and statistical. Perhaps one should regard economists as asserting only that, ceteris paribus consumer preferences are usually transitive.

Is such an interpretation justifiable? Economists provide no statistics to support their basic generalizations. All we are told is that, with qualifications, the “laws” are reliable in some domains. If economists can say no more, one might as well concede that these generalizations are false. Until economists specify what statistical claims they are making or philosophers show how false generalizations can be explanatory, one seems driven to conclude that the basic “laws” of equilibrium theory have little explanatory power. I find this conclusion unpalatable. In at least some applications equilibrium models seem to have explanatory worth. I see no philosophical way to support this appearance. My own attempts to develop a philosophical model of explanation employing false generalizations (Hausman, 1979a) have not been convincing. As I argued above in discussing roughness, I do not find the project promising. It thus seems to me that the responsibility lies with economists to develop those basic assertions which do not satisfy the excusability condition as explicitly statistical qualified laws.

In my view all the basic generalizations of equilibrium theory should be regarded as qualified with ceteris paribus clauses. In some cases the qualified generalizations satisfy the four justification conditions. One can, I think, regard diminishing returns to a variable input and diminishing marginal utility as qualified laws. The other basic general statements of equilibrium theory cannot be regarded as qualified universal laws. Perhaps one can regard them as qualified statistical laws. Since the statistics are unstated, one should not be satisfied with these generalizations, although we need not deny that they have some value.

The problems with justifying economic “laws” are serious and unresolved. At least the “laws” of general equilibrium theories of capital and interest face no further problems than do the “laws” of more familiar and less abstract equilibrium theories. If the only dubious

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8 The generalization that equilibrium is reached is not reliable. I also have my doubts about the generalizations that businessmen attempt to maximize profits or that there are constant returns to scale.
feature of general equilibrium theories of capital and interest was that they employed the fundamental "laws," I am not sure whether one might regard these theories as explanatory. At least one would have no grounds to compare such theories unfavorably with microeconomic theories of consumer choice or of the effects of rationing. We cannot, however, make even this relative assessment of general equilibrium theories of capital and interest until we consider the other assumptions in general equilibrium models.

As I mentioned before, many economists agree that their "laws" are rough and at best "close to the truth." In the above analysis I have made this view precise and have considered to what extent it may be regarded as a defense of the explanatory worth of equilibrium theory. To clarify the view of economic generalizations I have developed, I shall briefly show how it differs from some influential recent accounts with which it might be confused.

Philosophers and economists have attempted to analyze the inexactness of economic generalizations in many different ways. Fritz Machlup denies that these generalizations are false. He seeks in two different ways to show how they are insulated from apparent disconfirmations. Sometimes he denies that these generalizations say what they appear to (1960:559, 577–79). According to Machlup, they do not describe how consumers or businessmen behave. Instead they are theoretical statements about theoretical entities misleadingly called "consumers" or "businessmen." These terms are in fact only interpreted partially by the econometrician who tests significant implications of the theory as a whole. One has reason to believe that the general statements of an equilibrium theory are true if the theory is well-confirmed. This view of Machlup's is implausible (see Rosenberg 1976b:139–52). Not only does it misapply the efforts of twentieth-century philosophers of science to understand how sentences apparently referring to unobservable items can be testable, but econometric testing has, in fact, provided little confirmation for equilibrium theories. The view I have defended adds ceteris paribus qualifications to the general statements of equilibrium theory and considers some of them to be statistical claims, but otherwise interprets them literally.

Machlup sometimes denies that the general statements of equilibrium theories are either true or false (1955:9–11). This view also seems both unreasonable and unsuccessful as a defense of the purported laws of equilibrium theory against the objection that they are false. If one is sensibly to deny that theoretical statements are true or false, as the "noncognitivist" instrumentalist does (Morgenbesser 1969:202), one must be able to distinguish terms denoting observational entities from theoretical terms (here equated with terms denoting nonobservational entities). Otherwise one has no basis to distinguish observational sentences which can be true or false from theoretical sentences which supposedly cannot be regarded as true or false. But if terms like "consumer" or "firm" or "price" are supposed to lie on the theoretical side of the divide, what (except for "here blush now," or other such phenomenalist granting) is supposed to lie on the observational side? Furthermore, regardless of its shortcomings, it is hard to see how such an instrumentalist position helps. If one conceives of the purported laws of equilibrium theory as rules for making inferences or as instruments for making predictions, one still faces the problem that the rules or instruments are often unreliable.

In his extremely influential essay "The Methodology of Positive Economics" (1953:3–42), Milton Friedman offers a quite different justification of the basic generalizations of equilibrium theory. Friedman concedes that the general statements of equilibrium theory are false (or inapplicable because their antecedents are not true of any real economic situation). At least this is how I understand his view of them as one kind of "unrealistic assumption." Friedman simply and boldly denies that their falsity matters. If a theory is well-confirmed (is a good "predictor") in the class of cases in which economists are interested, it is a good theory; otherwise not (1953:14). Even assertions as abruptly counterfactual as the attribution of consciousness to tree leaves are perfectly acceptable in theories of the distribution of leaves on the branches of trees. All that matters is how successfully the actual distribution of leaves is "predicted" (1953:19–20).

Friedman's position is a special kind of instrumentalism, which must be distinguished from the noncognitivist kind that Machlup has sometimes espoused. Friedman does not deny that theoretical statements are true or false. In fact the distinction between theoretical and observation statements is irrelevant to his views. What he does deny is that the truth of any fundamental statement ("assumption" in Friedman's terminology) matters. All that counts is whether the theory makes correct predictions concerning the limited phenomena of interest. While many philosophers would agree that we should value a theory which "works" even for a very limited range of phenomena, few would be willing to discount completely the importance of the truth and falsity of consequences that happen not to be of practical concern (Bronfenbrenner 1966:12).

Friedman's methodological article has nevertheless been popular and influential among economists. Friedman has been so influential partly because he has been misunderstood. Economists often read him
as contending merely that one should assess theories by testing their implications. This view is, of course, no defense at all against the criticism that the basic generalizations of equilibrium theory are false and thus have some disconfirmed consequences. What is more responsible for the popularity of “The Methodology of Positive Economics” than such misunderstanding is that Friedman recognizes that the generalizations of equilibrium theory are false as stated, yet defends them anyway. Economists do not believe that businessmen always seek to maximize their profits. To say that businessmen behave this way is to oversimplify reality. The simplification is useful. It so happens that models which employ this and other falsehoods are reliable and useful tools for understanding some economic problems. Friedman defends the usefulness and legitimacy of such models.

Yet the defense is extravagant. According to Friedman, when an implication of a theory is false, one should conclude only that the theory does not apply to the particular phenomenon. Friedman concedes that scientists prefer theories which apply broadly, when theories with such a broad scope are as simple and easy to use as theories which only apply to restricted domains. Whether a given model suits an economist’s specific predictive purpose(s) remains, however, the crucial question for Friedman. The truth of the “assumptions” does not matter. We can, of course, agree that a theory full of apparently false assertions may be good enough for certain limited predictive purposes. Such a theory can, however, hardly meet demands for explanation and scientific understanding. The truth of generalizations must count.

Friedman’s views have also appealed to economists, because he rejects, as I did, a global or an absolute demand for truth. If one finds that individuals do not behave as self-interested utility maximizers in dealing with their children, one has discovered no major flaw in economic theory. It would be nice to have a general theory of human behavior, but there is nothing illegitimate or misconceived in trying to account for only some ranges of human behavior, for considering only the “major causes” in certain circumstances. The limited generalization that in their market behavior people are utility maximizers will serve just as well. Indeed, such limitations are implicit in the way economists employ their fundamental “laws.” Friedman, however, again goes too far. He believes that even within the limited range of phenomena which economists study, they should be concerned only about whether the particular predictions they make are true. Whether other implications of the theory are true is of no importance. Not only can economists be unconcerned about the discovery that, when on vacation, entrepreneurs do not act like profit maximizers, but, according to Friedman, economists can discount as irrelevant all investigations into the motivation of businessmen. If one has any interest in understanding economies, this view is unacceptable. Some investigations of motivations may be of little interest: they may reveal only that other things are not always equal or that actual behavior does not match any simple motivational generalization perfectly. Theorists can offer some explicit qualifications to take these results into account. Economists can, however, hardly announce, as one does (153:22, 31), that investigations into how entrepreneurs behave are irrelevant to assessing theories which make assertions about how entrepreneurs behave.

Inexactness is not a mortal sin. Economists can reasonably qualify their generalizations and limit the domain to which they are supposed to apply. The proof of the whole pudding is ultimately in the application and testing. Such is one moral of this chapter. On these points Friedman is right and economists are wise to follow him. But Friedman does not stop with these truths, and his methodological writings become, I believe, apologetics. If accepted, Friedman’s views would insulate neoclassical theory from legitimate criticism. The difficulties with the basic “laws” of equilibrium theory are serious. One can reasonably question their explanatory worth.

I have argued that it is legitimate to regard a generalization which is not true without vague ceteris paribus qualification as an explanatory law, but only if the generalization is lawlike, reliable, refinable and excusable. It seems to me that generalizations in the sciences nearly always carry such qualifications. The “laws” of equilibrium theory are disquieting, because their inexactness is neither a simple matter of implicit qualification, statistics, nor margin of error. Whether these “laws” can justifiably be regarded as laws in any given application is an open empirical question. The many confirmations of microeconomic equilibrium theories give one some indirect reason to accept the “laws” of general equilibrium theories of interest and prices. We cannot, however, yet offer even a relative assessment of general equilibrium models and theories. We must first consider the other assumptions of general equilibrium models, and we must consider whether the emphasis on economic models should in any way affect our assessment.


In assessing explanatory arguments, “laws,” simplifications, and specifications must be carefully distinguished. Economists often think
of the basic statements in their models as "assumptions" only. Many are unaccustomed to distinguishing between lawlike and simplifying assumptions. Some are uncomfortable speaking of "laws" at all, because they recognize that the "laws" of economics are qualified, statistical, and limited in scope. In discussing equilibrium theory and equilibrium models in chapter 6, I introduced and defended the distinction between lawlike and simplifying assumptions. The "laws" I listed, unlike such simplifications as perfect information, can be regarded as discovered by or asserted by economic theorists. Finding disconfirmation of the "laws," once these are qualified and limited in scope, creates a problem for the economic theorist. Finding that some commodities, like a Mercedes, come in large indivisible units reveals at most limits to the applicability of economic models.

In assessing possible explanatory arguments in economics, we need to refine and clarify the distinctions among the kinds of statements employed. My distinction between lawlike and simplifying assumptions (chapter 6) is not adequate, since in explanations scientists employ not only theoretical hypotheses, but other specifications and simplifications as well. Since there is among philosophers no standard systematic treatment of the differences among kinds of statements employed in explanatory arguments, it seems least confusing to employ simplification and specification as new technical terms. The distinction between them corresponds only very roughly to the more usual contrast philosophers draw between statements of initial conditions and auxiliary assumptions. The term specification avoids the misleading temporal reference of "initial conditions." I count as a specification any non-lawlike claim employed in explanation (or prediction or testing) which one has good reason to believe is true. I count as a simplification any non-lawlike claim employed in explanation (or prediction or testing) which one has no reason to believe is true (and which one may often have reason to believe is false). The assertion that other things are equal, that there are no unspecified interferences, is always at least a tacit premise in an explanatory argument. I place the ceteris paribus stipulation in a category by itself and regard it as neither a specification nor a simplification. The distinction between specifications and simplifications is entirely epistemic. One has good evidence for specifications. One does not have good evidence for simplifications. In fact

scientists are often quite confident that their simplifications are false. Those claims which philosophers often call "statements of initial conditions" will usually be what I call "specifications." Those claims which philosophers call "auxiliary assumptions" or "auxiliary hypotheses" (which are not assertions that there are no unspecified interferences and which are not lawlike) will usually be what I call "simplifications." Many auxiliary assumptions are not what I call "simplifications," but are instead lawlike claims which are independent of the particular theory which carries the major explanatory burden. In explaining the motion of a planet, for example, one may rely on the laws of optics as auxiliary assumptions which justify using data obtained by means of a telescope. Such auxiliary assumptions are not simplifications.

I have distinguished between simplifications and specifications because only the latter seem at first glance permissible in deductive-nomological explanations. As true statements, specifications are (along with laws) just the sort of statements which Hempel expects to find in scientific explanations. Simplifications, on the other hand, often state falsehoods, even impossibilities. Even in those cases in which they may be true, one has no reason to believe they are true and thus no reason, on Hempel's model, to accept purported explanations which employ them. Yet simplifications seem largely unavoidable in explanatory arguments. Human abilities to specify initial conditions (like the precise degree of divisibility of each commodity) are limited. Human mathematical abilities to employ such specifications, even if they are available, are also limited. Economists, like scientists generally, compensate for these limitations by substituting convenient simplifications for unavailable or inconvenient specifications. In the derivation of the ideal gas law, physicists use as a premise the claim that gas molecules are point particles. In relying on simplifications to derive important results, economists are proceeding in the way in which all scientists must.

Simplifications which appear extreme, even outrageous, are common in highly respected science. Idealizations, which I shall discuss in the next section, are usually such extreme simplifications. The prevalence of simplifications, however, neither proves that they are desirable nor justifies relying on particular simplifications in particular cases. Simplifications may be legitimately included in explanatory arguments only if they satisfy certain conditions. Friedman has the barest account of these conditions. For him there is only one. A simplification is legitimate if and only if the predictions one is interested in are correct (1953:p.17). For Friedman, a model of free fall in a vacuum would be

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9 Milton Friedman and others have sometimes called antecedents of conditional statements and even predicates "assumptions" as well. For a taxonomic guide to the controversy concerning unrealistic assumptions in economics see Brunner (1969:501–25) and Hausman (1978:202–6).
an adequate model of the fall of a steel ball in a vat of molasses in a strong magnetic field if the distance the ball falls is proportional to the time squared. Friedman is right that one necessary condition is a confirmation condition, but wrong to believe that it is sufficient. In my view a simplification is legitimate—one is justified in employing it in an explanatory argument—only if

1. **Confirmation condition.** One needs the simplification not only to derive the statement of what is to be explained, but to derive other testable consequences, most of which are confirmed.

2. **No-accident condition.** One can understand why, even though one has no reason to believe the simplification is true, one can use it in explanations and predictions and meet the confirmation condition.

3. **Sensitivity condition.** If one replaces the simplification with a specification or with another simplification which is more realistic or a better approximation, one is able to explain more phenomena or is able to explain the given phenomena under a more refined description or within a smaller margin of error.

4. **Convergence condition.** In those circumstances in which the simplification is a better approximation, one is able to explain the phenomena under a more refined description or within a smaller margin of error.

Like the lawlikeness, reliability, refinability, and excusability conditions discussed above, these four conditions are justification conditions. The conditions discussed earlier must be satisfied before one can be justified in regarding a statement as an inexact law. The four conditions just listed must be satisfied before one is justified in employing a simplification in an explanation—that is, before one is justified in regarding a simplification as "legitimate." The conditions in the two sets are related to one another, although the correspondence is rough.

There is nothing mysterious in either the relations or the lack of precise correspondence. An explanatory argument, like a law, exhibits some nonaccidental connection between phenomena. Indeed, if one rewrites a valid explanatory argument as a conditional statement with the nonlawlike constituents of the explanation in the antecedent and the description of what is to be explained in the consequent, one can regard that statement as a highly specific law. If such a conditional statement can be regarded as a law, the simplifications it contains must be legitimate. The confirmation and no-accident conditions are thus variants of the reliability and lawlikeness condition, once we correct for the fact that in an explanation one is concerned with a single phe-nomenon or with a much more limited range of phenomena than in enunciating a law. The lawlike statement into which an explanatory argument can be translated will be refinable only if the simplifications in the explanation satisfy the sensitivity condition. The lawlike statement into which one can convert an explanatory argument will be excusable only if the simplifications in the explanation satisfy the convergence condition.

These justification criteria are implicit in the assessments scientists and laymen make of explanatory arguments. The derivation of the ideal gas law from kinetic theory, for example, satisfies these conditions. The simplifications employed are legitimate. The ideal gas law is testable. For certain gases and for certain ranges of temperatures and pressure it is well confirmed. Indeed, one can use the ideal gas law to make reliable gas thermometers. The no-accident condition is also satisfied, since scientists have independent reasons to believe that gases are made up of small particles which in some cases exert only very weak attractive forces on one another. Without any reason to believe in the existence of such particles or to accept the laws of motion these particles supposedly follow, the derivation of the ideal gas law from kinetic theory would not be explanatory. The convergence condition is also satisfied. Scientists know that gas molecules are not point particles and that molecules attract one another. Scientists have independent ways to estimate the size of the particles and the strength of the attractive forces. They find that the ideal gas law is more accurate for gases with smaller molecules that exert weaker attractive forces on one another. Finally the sensitivity condition is satisfied. When one takes into account the size of gas molecules and the attractive forces between them, one is able to derive a more accurate law relating volume, temperature, and pressure of gases.

But why do scientists tacitly demand that the simplifications used in explanations satisfy the confirmation, no-accident, sensitivity, and convergence conditions? I have suggested that unless a simplification satisfies these conditions, one should not judge it to be legitimate. But what is it for a simplification to be legitimate? What property must it have in order to be (not merely to be considered as) part of an explanation? For the presence of what property is one testing by checking whether the simplification satisfies the four justification conditions? Not only is this question itself important, but we must answer it to evaluate the reasonableness of my proposed criteria of justification. Regardless of how prevalent or plausible those criteria are, we cannot rationally decide whether scientists should employ them until we know what scientists are testing for.
A simplification is legitimate, I think, if and only if it is approximately true. Simplifications are approximately true only if either they are true within a margin of error, or they are statistical or probabilistic statements which are true or true only within a margin of error. In an explanatory argument one is showing the connection between the factors cited in the argument and the phenomena to be explained. The factors one cites must actually be present and must actually be responsible for the phenomena; otherwise, one has not explained anything. If the simplifications one employs were not close to the truth, one would not be showing a real connection and would not be explaining.

The four justification conditions are in fact reasonable criteria for judging whether a claim used in an explanatory argument is true within a margin of error. The confirmation condition tests whether things are as if the simplification were true. The no-accident condition demands that to the best of one’s knowledge there actually be a connection between what the simplifications assert and the phenomena being explained—which could not be the case if there were no truth to the simplifications. The convergence condition looks for more accurate consequences in those cases in which the error in the simplification is smaller. The sensitivity condition does the same by revising the simplification itself rather than by considering more restricted cases. Scientists thus do, I suggest, demand that a simplification employed in an explanatory argument be approximately true. They check to see whether a simplification is thus legitimate by seeing whether it satisfies the confirmation, no-accident, convergence, and sensitivity conditions.

If we assess attempts to employ current general equilibrium models to explain the phenomena concerning capital, interest, and exchange, we are forced to negative conclusions. When the simplifying assumptions those models include are closed and taken as statements, they do not meet all of the above four conditions. The models contain illegitimate simplifying assumptions. Theoretical hypotheses employing intertemporal general equilibrium models or temporary equilibrium models cannot be confirmed.

There is thus no good way to tell whether the simplifications in such general equilibrium theories satisfy the sensitivity or convergence conditions. One can, however, argue that applications of intertemporal equilibrium models like the one in chapter 5 will contain simplifications that fail to satisfy the convergence condition. Suppose one could somehow measure on a single scale the accuracy and extent of an individual’s information about current and future economic data. Can one say that as individuals move up the scale (within the attainable range) that the values of the economic variables approach those calculable from intertemporal equilibrium theory? There are good grounds for doubt. Better knowledge within the attainable range might lead to disastrous decisions. Lipsey and Lancaster’s work on the theory of the second best (1956–57: 11–32) establishes an analogous result. These considerations do not prove that simplifications like perfect information will not satisfy the convergence condition, but they give one good reason to doubt. More practical general equilibrium theories, like input-output analyses, are not entirely without explanatory merit. There is, however, a tradeoff. As general equilibrium theories come closer to being explanatory, they come to have fewer implications concerning capital and interest. Recall that making even the small move from intertemporal equilibrium models to temporary equilibrium models means that one can no longer (in theory) say anything about the real rate of return on investments.

Can anybody devise a general equilibrium model that is both explanatory and that has significant implications concerning capital and interest? I do not know. I have argued only that current abstract general equilibrium models, which contemporary economists regard as fundamental for the understanding of economics, are not explanatory, while more practical general equilibrium models do not answer any of the interesting questions concerning capital and interest. Who knows what theorists may be able to accomplish in the future? At present, however, neoclassical economists cannot justifiably claim to be able to explain the major phenomena of capital and interest.

From this conclusion one might be tempted to jump to the more radical conclusion that abstract general equilibrium models are worthless. That jump would be unjustified. In § 1 I shall argue that such models are extremely valuable. Once one ceases to regard them as explanatory, one can see much more clearly what their role and importance is. Before turning to these issues, however, we need to consider some questions concerning economic theorizing which the discussion has so far left hanging. We have not yet considered carefully the nature or significance of idealizations in economics nor the importance of the economist’s reliance on models.

10 This claim is probably too restrictive. Simplifications may perhaps be legitimate if they are replaceable in an explanatory argument by a true statement, even though scientists do not yet know how to carry out the replacement. There may be other possibilities as well. Confining my discussion to the simplest kind of legitimacy does not, I believe, bias my conclusions.
5. Models and Idealizations in Economics

Many economists, as I have noted, regard their models and theories as idealizations. It is not always clear, however, whether the view that economic theories involve idealizations is regarded as opposed to or as complementing the view that economic theories are approximations. Economists have generally been unclear about what idealizations are, what role they are supposed to play, and what significance their presence has. Until recently, economists have not had much help on these questions from philosophers. Even a philosopher as eminent and sensible as Ernest Nagel conflates idealizations and theoretical terms (1961:160; 1971:49). After interpreting talk of idealizations in economics as sympathetically as I can, I shall argue that it has little to contribute to understanding the apparent inexactness of economic theories and is of no help in defending their explanatory worth.

An ideal entity or property is one which one knows cannot be real. The knowledge that there are no perfectly rigid bodies or point particles is supplied by scientific theories (Shapere 1969:131-49). No scientific theory reveals that there are no complete futures markets or that individuals do not have perfect information concerning the economic future. It would, however, be a mistake to deny, as Alexander Rosenberg does (1976b:133), that these claims are idealizations. Well-developed scientific theories are not the only sources of knowledge. Notice that none of the fundamental generalizations discussed in chapter 6 themselves appear to be ideal. People’s preferences are not always transitive, but during any given time interval, they might be. Entrepreneurs do not always attempt to maximize profits, but they might.

Economics employs simplifications that are idealizations. In this it resembles the natural sciences. But what can we conclude from the resemblance? Unconditional ideal assertions are always false. Why should one regard the fact that a theory makes ideal claims as a defense rather than as a criticism? Even if the “laws” of equilibrium theory were ideal, which they are not, merely calling attention to that fact would not provide a clear interpretation of them. Nor do we learn much about simplifications when we are told, correctly, that some of them are idealizations.

In fact it seems to me that although economists often use the words “ideal” or “idealization,” they are not really interested in idealizations, at least as I defined them above. Instead they wish to interpret their models as making unrealistic claims (hence the use of the term “ideal”) about how things would be, were various complications absent. I shall call this view of economic models the “modal model” view, since it interprets models as making modal claims about how things would be. According to the modal model view, sentences like “Entrepreneurs attempt to maximize profits” do not merely define predicates in models and make inexact claims in theories. Rather they state truths about certain possible economies. The assumptions of economic models not only appear to concern highly simplified and in some cases ideal circumstances, but they are in fact about unreal entities and properties.

The modal model view does not necessarily demand different justification conditions than those discussed in the previous two sections. The modal model view, as baldly stated above, says nothing about justification. The theorist who regards the assumptions of economic models as counterfactual is questioning instead whether one should interpret the purported laws of economics as making qualified claims about actual economies. The modal model view is in some ways stronger and in other ways weaker than the qualified empirical assertion view. If one grants the scientific legitimacy of talking about how things would be, one will be able to deduce counterfactual claims from qualified non-counterfactual claims. If it is the case that, given certain qualifications, businessmen attempt to maximize profits, then it is the case that they would do so were the qualifications unnecessary. The converse does not always hold. From the claim that businessmen would all attempt to maximize profits in certain hypothetical circumstances, it may not follow that one can make any qualified claims about what businessmen actually do. The modal model view thus appears (once we grant the legitimacy of talk of possible economies) more circumspect than the qualified assertion view. Yet the modal model view is, of course, committed to a more ambitious metaphysics. Someone who regards the purported laws of economics as making qualified and statistical claims may never need to refer to any hypothetical circumstances.

Many economists have regarded their theories as making claims about how things would be were various complications absent. I do not see any way of arguing that this view of economic models and theories is incorrect. I have, however, two quibbles. First I am disposed toward metaphysical modesty. If one can thoroughly and sensibly understand economic theory without making reference to any merely possible economies, so much the better. Second I think that the modal model view tempts economists to take applied theories seriously even when the claims of such theories do not satisfy the eight justification conditions discussed in §2 and §4. In cases in which there is no way to apply or test the equilibrium models in question, economists still insist that the models provide a guide to which concepts are most
important and a significant framework for making comparisons or contrasts (see, for example, Bliss 1975:301 or Weber 1949:90–91).

The modal model view leads one to take assumptions of models themselves seriously and to blur the distinction between assumptions and statements. Nothing real belongs to the exact extension of predicates defined by many economic models. The modal modelist, unhappy with empty or fuzzy extensions finds possible economies or economic sectors which the predicates denote. This “finding” is not automatic. Not all models define predicates which denote possible economies. Definitions must satisfy some conditions before one can see in them accounts of possible economies. These conditions may be identical to those discussed in the last two sections. Such talk of possible economies is, in my view, of value only to the extent that it helps one understand (real) economies. Taking the assumptions of economic models themselves seriously as providing information about possible economies, theorists are distracted from assessing the various resultant theories and are more likely to be sloppy about questions of justification.

If I am right that the modal model view tempts economists to take the claims of their models themselves seriously and to be sloppy about justification, then it certainly does matter whether one adopts this view or the view I have defended which regards economic theories as inexact. Many economists tend, I think, rather lightly to regard comparisons and contrasts of real and hypothetical economies as significant. Were they to heed the justification conditions discussed in §2 and §4 (or to propose rational alternatives), their manipulations of models might be more informative and less questionable. Those assertions of economics which help explain economic phenomena provide information about actual economies.

6. Inexact Explanation in Principle

A great many actual scientific explanations are inexact explanations in principle. In the nonstatistical case, one explains an event or state of affairs by deducing a description of it from a set of statements which include qualified laws, specifications, legitimate simplifications, and the assertion that other things are equal, that there are no unspecified interferences. In the statistical version the set of statements must make the description of what is to be explained highly probable. Explanations need not include statements from all four categories, although they must always contain laws.

If the above account of inexact explanation in principle is correct, we can see that such explanations are never literally deductive-nomological explanations or inductive-statistical explanations, since the simplifications they contain are by definition false. This conclusion is, however, pedantic. If we allow a little rephrasing, we can regard many inexact explanations in principle as deductive-nomological or inductive-statistical. Qualified laws on my analysis are genuine laws. Specifications are true statements. Since simplifications are approximately true, one can rephrase them as less specific but literally true assertions. (In doing so the description under which one explains the phenomenon also becomes less specific.) The stipulation that other things are equal is vague, but it may nevertheless be true. Inexact explanations in principle, charitably interpreted, often satisfy Hempel’s conditions (pace Rescher 1970:176).

Two factors led me previously to challenge the deductive-nomological model (Hausman 1981). In part I failed to distinguish carefully between the conditions an argument must satisfy it is to be an explanation and those it must satisfy before one can regard it as an explanation. After discovering the difficulties involved in knowing that the statements in explanations are true and in some cases laws, I looked to justification conditions for an alternative model of explanation that could be more easily used to evaluate purported explanations. The justification conditions were also attractive as an alternative model, since I recognized (as has Cartwright 1980) that the statements in scientific explanations are often not literally true.

It now seems to me a mistake to challenge the deductive-nomological model of explanatory arguments on either of these grounds. Scientists demand that the statements in their explanatory arguments be true. Without truth, there is no explanation and no rationale for the justification conditions I have presented. We can better study the criteria

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11 They need not always do so. Sometimes the simplifications are not approximately true. Instead one believes that they are replaceable in the explanation by true statements, which scientists cannot yet provide. The explanatory argument thus contains false statements which cannot now be replaced with true ones. Such arguments are thus not deductive-nomological or inductive-statistical explanations. We have two choices. We can continue to regard such arguments as explanatory and change Hempel’s models, or we can conclude in these cases that one does not yet have an adequate explanation. I see nothing to be gained in changing the model of explanation. We can account for the apparent explanatory power of inexact explanations in principle which are neither deductive-nomological nor inductive-statistical by pointing out how close such inexact explanations in principle come to satisfying the conditions of Hempel’s models. Despite the many complications involved in actual scientific explanations, the deductive-nomological model seems to provide reasonable necessary conditions for nonstatistical explanatory arguments.
scientists employ in assessing explanations if we distinguish these criteria from the necessary conditions something must satisfy in order to be an explanation. The two need not always be different. Sometimes it is helpful directly to ask, for example, whether the statements in a purported explanation are true. Yet, as we have seen, scientists must often settle for statements that are not, as stated, true, but that have some truth in them. Doing so does not force one to reject the deductive-nomological model of explanation, but only to recognize that literally false statements may, with reinterpretation, express truths. In assessing an inexact explanation in principle, one cannot judge directly whether it consists of a set of true statements, some of them laws, which entail a description of the phenomenon to be explained. Instead one must consider whether its lawlike statements satisfy the reliability, refinability, and excusability conditions and whether its simplifications satisfy the confirmation, no-accident, sensitivity, and convergence conditions.

Applying these conditions, we reach the same judgment concerning general equilibrium explanations of the phenomena of capital and interest as does the naive critic who notes that such explanations contain apparently false statements. Yet our judgment has not been summary or naive. I have not merely applied abstract philosophical dicta, but have considered directly whether the peculiarities of general equilibrium models conflict with the demands of explanation. For the "laws" of equilibrium theory, I could make at least a partial apology. General equilibrium explanations of the phenomena of capital and interest are no worse off on account of the laws they employ than are many persuasive microeconomic explanations. For the simplifications employed, on the other hand, I could make no apology at all. General equilibrium models do not enable economists to explain the phenomena of capital, interest, or exchange values.

7. The Merits of General Equilibrium Models

If we must conclude, as I have argued, that abstract general equilibrium theories do not explain the characteristics of real economies, what worth do they have? Is the work of the abstract general equilibrium theorists more than some pretty mathematics? Can abstract general equilibrium models be of value if they say nothing about real economies?

These are difficult questions upon which leading theorists disagree. Some, like Debreu (1959:ix), mistakenly believe that general equilib-rium models can be used to explain prices. Malinvaud (1972:242) argues that intertemporal models like the one presented in chapter 5 "must be considered to be aimed at the analysis of one aspect of reality, namely that concerning the intervention of prices and interest rates."

My argument here shows that no current general equilibrium models achieve that aim. C. J. Bliss is more cautious:

Of course, that model does not serve to represent reality and that is not its purpose. Where the simple model of an intertemporal economy with all the forward markets functioning can prove useful is as a point of departure, as a guide to which concepts are central and fundamental and which peripheral, and as a reminder that time and capital make an important difference precisely because and only because the system of forward markets... are [sic] not in fact extant. (1975:301)\textsuperscript{12}

Why should one believe that the simple model in question is a good "point of departure" or a good "guide to which concepts are central and fundamental and which peripheral" or a valid "reminder that time and capital make an important difference precisely because" the forward markets postulated in the model do not exist? In my view one has no good reason when one is unable to apply the model (or similar models) and confirm some of its implications. Economic models can only help one understand economies if they meet conditions of §2 and §4. Economists are fond of saying that their models only provide some sort of logic of economic phenomena or that they are merely tools one can use when convenient. These claims have reasonable interpretations, but as I argued in §5, they do not provide any way around the demands of §2 and §4.

There is, however, something more in Bliss’ attitude. General equilibrium models may be of great heuristic value.\textsuperscript{13} One can show that general equilibrium models have been of heuristic value merely by showing that they have in fact helped in developing valuable empirical economic theories. General equilibrium models have in fact been valuable as the source of conceptual and mathematical devices which have been employed in other, less abstract, models. Although some of these devices, such as dated and spatially located commodities,

\textsuperscript{12} Bliss does not always speak so generously of intertemporal models. “If, therefore, we reinterpret the basic equilibrium model of the atemporal economy by supposing that goods have been labelled according to the date at which they become available, . . . we would place an impossible strain upon the assumption of perfect knowledge. Thus the appropriateness for an economy that persists through time of a model that is formally analogous to the perfect knowledge atemporal model is to be doubted” (1975:44).

\textsuperscript{13} Conversations with Edward Green helped me to appreciate this point.
facilitate proving the existence of equilibrium, their heuristic value is independent of the proofs these models provide.

Are general equilibrium models only of heuristic value? Frank Hahn has argued at some length that general equilibrium models, although not explanatory, are of great value. In response to Kornai's extended critique of general equilibrium theory (1971), Hahn emphatically denies that general equilibrium theories are descriptive or explanatory. "It does not occur to him [Kornai] that the most obvious explanation why one studies this theory, which is known to conflict with the facts, is that one is not engaged in description at all" (1973:323). "It is Kornai's besetting sin that he writes as if such a lunatic claim had ever been entertained" (1973:329).

Hahn goes on to defend emphatically the worth of abstract general equilibrium models. First, he argues that such models are reductio ad absurdum arguments against the claim that competitive economies are optimal or efficient:

Someone proposes an explanation of the origin of the earth, say that it was sucked out of the sun. There is no way in which the event itself can now be observed. A theoretical physicist calculates the angular momentum of the earth if the explanation were true. In doing so, he provides a way in which the theory can be falsified. When the claim is made—and the claim is as old as Adam Smith—that a myriad of self-seeking agents left to themselves will lead to a coherent and efficient disposition of economic resources, Arrow and Debreu show what the world would have to look like if the claim is to be true. In doing this they provide the most potent avenue of falsification of the claims. (1973:323-24)

Let us call the old claim of Adam Smith's "the invisible hand postulate." Hahn is suggesting that general equilibrium models "provide the most potent avenue of falsification" for the invisible hand postulate. They show us how infeasible a certain sort of social ideal is.

It seems to me, however, that neither economists nor laymen need general equilibrium models to show that any real (and thus at most semi-competitive) market economy does not regularly achieve full employment or optimal allocation of resources. If general equilibrium models were needed for the purpose, they would not help anyway, since the existence proofs the models present do not show what conditions are necessary, but only what conditions are sufficient for competitive equilibrium.

Hahn also argues that general equilibrium models have a more practical use. He writes, "When it is claimed that foreign aid is unnecessary because only investment profitable to private investors can be benefi-
cial, we know at once that the speaker or writer does not know the findings of GE [general equilibrium theories]. Anyone who has this knowledge will have no difficulty in pointing to those features of the actual situation which are at variance with what would have to be true if such a claim were to be true" (1973:324). General equilibrium models are thus supposed to be a palliative against practical recommendations based on inadequate theory. General equilibrium theories lack predictive power and do not solve practical problems, but they can reveal that conclusions of previous theories depend on implicit assumptions which are not true of actual economic situations. This benefit from general equilibrium models is real, but should not be exaggerated. Earlier economic theory did not show that only investment profitable to private investors can be beneficial. The public demands a great deal of economists. They must often make recommendations which they realize are not based on solid theoretical foundations. It is unclear whether general equilibrium models ought to or will make economists more cautious in their predictions or recommendations.

Neither of these assertions on behalf of general equilibrium models shows that they are of any great worth. One can understand their merits better when one focuses on the relations between general equilibrium models and other neoclassical economic models. Hahn argues that "the student of GE believes that he has a starting point from which it is possible to advance towards a descriptive theory" (1973:324). "General equilibrium theory," according to Hahn should not be identified with theorems in Debreu's Theory of Value. It is instead nothing but the attempt "to study rigorously the interactions of many economic agents" (1973:324). There is no reason why one cannot attempt to study change and development in this way. General equilibrium theory, according to Hahn, involves only a kind of model or a program for developing adequate economic models. It must not be identified with any of the current and inadequate models.

This defense of general equilibrium theory is not fully consistent with the previous two, since equilibrium theories appear to involve the same invisible hand postulate (that equilibrium will be reached) which, according to the first defense, general equilibrium models help to falsify. Nor is this defense, as thus far stated compelling. We need to see more precisely how general equilibrium models help economists advance toward an adequate economic theory.

One can appreciate better what general equilibrium models offer when one accepts my conclusion that general equilibrium models are not themselves the foundation of modern neoclassical economics, but special augmentations of equilibrium theory. Once one realizes that
general equilibrium models are, one can ask "what is such an augmentation for?" Although Arrow and Hahn do not explicitly pose this, the crucial question, they do sketch the answer as follows:

The immediate "common sense" answer to the question "What will an economy motivated by individual greed and controlled by a very large number of different agents look like?" is probably: There will be chaos. That quite a different answer has long been claimed true and has indeed permeated the economic thinking of a large number of people who are in no way economists is itself sufficient grounds for investigating it seriously. The proposition having been put forward and very seriously entertained, it is important to know not only whether it is true, but also whether it could be true. (Arrow and Hahn 1971:vi)

Since the eighteenth century many economists have believed that, given reasonably favorable conditions, self-interested voluntary exchanges lead to coherent and efficient economic organization. Yet the theories which economists have possessed have not enabled them to explain how this order in fact comes about nor even to show how it is possible that such order could come about. Economic theorists might thus reasonably be in doubt concerning both whether their theoretical framework captures the crucial features of the economy and whether it is likely to lead them to an adequate theory. The curiosity that motivates the general equilibrium theorist is thus much like the curiosity that motivates the cosmologist. In developing equilibrium models, will one ever be able to explain how self-interested individual action within certain institutional constraints can lead to coherent economic order? Does one really have a grip on the most important and central economic regularities? Will one ever be able to understand whether the results of individual actions are truly efficient and whether they lead to the achievement of other economic goals?

Clearly there is no way to show in advance that equilibrium models, when refined and developed, will enable theorists to explain (inexact but principle) detailed as well as overall characteristics of economics or of economic events. The successes of partial equilibrium models or of practical general equilibrium models give one some reason for hope. Furthermore, both as a way of working toward such an adequate theory and as a way of testing the promise and power of equilibrium theory, it is sensible and important to investigate the explanatory possibilities of equilibrium models. If one augments the generalizations of equilibrium theory with various simplifications, can one explain the existence of any sort of economic equilibrium, even if it is only imaginary? If theorists can use such augmentations of equilibrium theory to show how economic order can result from independent self-interested individual actions, they have reason to believe that they have captured the fundamental elements of economic life and that the neoclassical approach may succeed.

In proving the existence of equilibria under various conditions, the abstract general equilibrium theorists formulate models one can use to assess the potentiality of equilibrium theory. Such models show that in equilibrium theory is capable of explaining at least some sorts of complicated economic equilibria, and thus they give theorists reason to believe that they are on the track of an adequate general economic theory. Economists thus do have some uses for talk of possibilities (although no need for the more extravagant views of the modal model). This sort of an investigation of the possibility of an explanation needs to be distinguished both from the above discussion of the feasibility of economic equilibria and from philosophical discussions of explaining how possibly. General equilibrium theorists ask whether it is possible to use equilibrium theory to explain complicated economic phenomena.

The abstract general equilibrium theorists have shown that, were the world very much simpler than it is or will ever be, economists could use their "laws" to explain in principle how economies work. Theorists thus have reason to believe that they are on the right track. The existence proofs provide this theoretical reassurance. They are not explanations. These abstract general equilibrium models may also help improve current explanatory theories. By weakening and complicating the stipulations needed to demonstrate the existence of more complex equilibria, theorists can come closer to being able to apply the models to real economies.

There is, however, only so much that mathematical work can do. The fundamental generalizations of equilibrium theory need improvement. Much difficult work remains to be done in characterizing the sort of economic order (and disorder) there actually is that needs explaining. Economists disagree about whether equilibrium models can ever adequately account for certain aggregate economic phenomena. The mathematical efforts of the abstract general equilibrium theorists cannot resolve these problems, although they can help. They can, for example, show economists how to make do with weaker formulations of the "laws" of equilibrium theory and may even suggest replace-

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14 Hempel 1956:128-30; Dray 1957:158f. In explaining how possibly, one is concerned with the case in which something happens contrary to one's expectations which needs explaining away. Economists are not, of course, trying to explain away the existence of some sort of competitive equilibrium.
ments. The existence proofs offer economists some reassurance that equilibrium models will eventually do the trick.

Perhaps equilibrium models will someday enable economists to explain the phenomena of capital and interest. Meanwhile, neoclassical economists remain unable to do so. Perhaps some alternative to equilibrium theory might enable economists to do better. Let us turn to Sraffa’s work and examine the possibility of such an alternative.

CHAPTER EIGHT
Sraffa and Neo-Ricardian Value Theory

In Chapters 2 through 5 we traced neo-classical views of capital and interest and exchange values from their intuitive roots to their precise formulation in abstract general equilibrium models. The final product is logically rigorous and avoids the difficulties faced by capital theories like Clark’s or the Austrians’. Yet, if the arguments and analyses of Chapters 6 and 7 are correct, that theoretical achievement cannot be regarded as an adequate theory of capital and interest or of exchange values. No existing general equilibrium theory explains even the principal phenomena of capital and interest. To reach this conclusion, we required both the economics and the philosophy of the previous chapters. We return now to the classical approach to problems of capital and interest, as revived and modified by Piero Sraffa. Perhaps the refurbished older approach will be better able to account for the relations between capital, interest, and prices. In examining Sraffa’s work we shall also be probing more deeply into the Cambridge criticisms of chapter 4.

Sraffa’s work is intriguing. His economics is strikingly different from neoclassical economics. Juxtaposing Sraffa’s work and Marx’s suggests the outlines of an alternative to the vision of economic life as the reconciling of the plans of self-interested individuals who only interact through exchange. In analyzing Sraffa’s work and reactions to it, we shall uncover further philosophical assumptions underlying the commitment of most economists to general equilibrium models. Sraffa’s achievement is modest, however. His work is interesting not for what it shows, but for the vision of economic theorizing that guides it.

1. Sraffa’s System

Sraffa regards his work as a continuation and a development of the thought of the classical political economists, particularly Ricardo. He