Assessment of Sraffa's System

ing one way in which the acceptance of Sraffa's work along with the rejection of general equilibrium models can have normative implications.

As mentioned in chapter 8, many economists take Sraffa's work to support some strong conclusions. Recall in particular (III) The rate of profit is not causally determined by individual exchanges as constrained by technology and the availability of unproduced factors of production. Sraffa's work does not support III. It says nothing about what determines the distribution of income. In equilibrium models, on the other hand, III is false. III is only defensible if one denies that general equilibrium models can be used to explain the rate of interest. Sraffa's work, on the other hand, is consistent with III. It provides some reason to believe that an alternative to equilibrium theories is possible. Sraffa's work also helps make Marx's theories tenable.

III is a causal claim. How can it be of any normative importance? Consider J. B. Clark's views. He regarded his theory of the distribution of income as crucial to an assessment of the justice of the earning of profits by capitalists. He writes, "If they [the workers] create a small amount of wealth and get the whole of it, they may not seek to revolutionize society; but if it were to appear that they produce an ample amount and get only a part of it, many of them would become revolutionists and all would have the right to do so" (1902:4). Clark seems to be relying on some sort of quasi-Lockean theory of justice that says that individuals have a right to the product of their labor. On such a theory it is important to know whether III is true. Suppose it is false, and that profits are the result of voluntary exchanges related, as general equilibrium theory shows, to the productivity of capital goods and the general impatience of people to consume. According to the theory of justice Clark relies on, workers would then have no right to complain about the existence of profits. If, on the other hand, III is true, one needs some other explanation of what determines the distribution of income. If it is some sort of coercive power that the capitalists possess, then, on Clark's view of justice, workers may have a right to the profits the capitalists are earning. With more sophisticated theories of distributive justice and more specific economic analysis, the relations are no longer this simple; but this example illustrates how the propositions of capital and interest theory may affect our assessment of capitalism.

Chapter Ten

Conclusions

We have considered carefully three theories relating capital and interest to exchange values, and have investigated thoroughly the interconnected economic and philosophical issues the three theories raise. Here now are some general conclusions concerning capital and interest and their relations to exchange values.

My principal conclusion is negative. Economists possess no good theory of capital or interest, or of their relations to equilibrium prices. Certainly, they possess elegant models and are able to prove many theorems. Unfortuntely, these models and theorems do not enable one to explain real phenomena of capital and interest; and I know of no theory undisputed here which avoids the difficulties faced by those I have considered. The kinds of problems which the Austrian theory, general equilibrium theories, and Sraffa are unable to avoid or solve affect all theories of capital and interest and their relations to exchange values that I know of. Economists do not understand the phenomena of capital and interest. They do not understand why the rate of interest is generally positive (and thus how it is that capitalism can work). They do not know how large-scale technological changes will affect wages and interest or how changes in the rate of profits will affect innovation. Of course, economists are not totally ignorant about any of these problems. There is some large-scale historical evidence. Various theoretical tools may be of help in special limiting cases. Yet our ignorance remains vast. Recognizing this fact is not necessarily to condemn the efforts of economists. Even the most brilliant and sensible work does not always succeed. One should honor the efforts of economic theorists not by exaggerating their achievements, but by attempting to understand them correctly and to build upon them.

1. Assessing the Specific Theories

Although the general negative conclusion possesses some drama, the specific points established along the way toward it are more im-
important. The Austrian theory of capital, general equilibrium theories and Sraffa's work are all three unable to answer the questions concerning the phenomena of capital and interest, but each fails in a different way. The problem with the Austrian theory of capital is that it is unfounded. It is not a minor application of the basic equilibrium model, but adds substantive but unsubstantiated claims. Böhm-Bawerk's law of roundaboutness is nicely illustrated by aging wine or by Lange's wood and ax model, but economists have no reason to believe that it is generally reliable. The Cambridge critics' models are no less plausible than are Wicksell's and Lange's. Until theorists possess empirical evidence to show that more roundabout processes are reliably more productive and that the rate of interest is inversely related to the degree of roundaboutness, theorists have no good reason to accept the Austrian theory. Examining general equilibrium models should further incline one to be skeptical of the possibility of representing the role of capital goods in production by some single index like the degree of roundaboutness or the quantity of "waiting."

General equilibrium theories, unlike the Austrian theory, do not add new "laws" to the basic equilibrium model. The extent to which equilibrium models have been successfully applied to various specific economic problems provides general equilibrium theories with a good deal of initial credibility. After all, they are merely a more general application of the same basic model. Yet general equilibrium theories which are sufficiently unrestricted that they have implications concerning the phenomena of capital and interest are currently untestable and thus unconfirmed. The confirmation of the "laws" of such theories in certain restricted domains does not provide sufficient reason to accept these theories as explaining the phenomena of capital and interest.

The inadequacies of Sraffa's work, like those of general equilibrium theories, are methodological. Sraffa's work is not necessarily inapplicable or untestable, but its scope is simply too limited for it to be regarded as a good explanation of the relations between interest and exchange values. Sraffa provides, of course, no theory of capital or interest at all. His work might be combined with theories of capital or interest, but none of the known candidates is well supported. Furthermore, the "law" that Sraffa relies on, that the rate of profit will be equal throughout the economy, although useful and well confirmed in certain limited domains may well not be true of whole modern economies (even with qualifications to some margin of error).

For these rather different reasons, neither Sraffa nor general equilibrium theories nor Austrian theories resolve the puzzles of capital and interest. It thus seems to me unhelpful to think in terms of choosing among these options. Indeed, the three theoretical ventures are not necessarily opposed to one another. One is not in any position now to decide what is the truth. Yet economists must decide how to do their work. Which (if any) of these approaches should theoretical economists devote their efforts to? Which of these approaches should economists employ in attempting to answer pressing practical questions addressed to them by government or industry or labor? All three of the approaches provide tools for solving problems. These tools may be seriously defective, but without alternatives or a demonstration that they do more harm than good, they will be used anyway.

Once one recognizes that neither Sraffa's work nor general equilibrium models provide answers to questions like "How much of the growth of the productivity of labor is due to capital accumulation?" I see no reason why theorists should not attempt to make use of simplified accounts of capital and interest like Clark's or Böhm-Bawerk's. The Cambridge criticisms have not refuted these aggregate capital theories, but at most emphasized the risks of employing them. In employing these theories, not only does one face normal inductive risks, but one is also gambling that Clark's or Böhm-Bawerk's, rather than the Cambridge models apply in the given situation. One should be cautious and skeptical about applying aggregate capital models. It might turn out that one can learn nothing at all from them. Yet at least they are well fashioned tools. Economists do the best they can with them and hope for fruitful results. Should one spurn the use of such tools merely because one has little reason to believe that they will work and few ways of telling if they have? It all depends on how pressing our needs are, how dangerous the mistakes might be and what alternatives are available.

Much the same is true in applying models of the other kinds. In employing Cambridge-style models or less abstract limited general equilibrium models, one again runs many risks. If economists must employ models in unjustified ways, I suppose it is best that they have as broad a range of such models to pick from as is practical. Although current reliance on Cambridge models or on practical constrained general equilibrium models is on a par with reliance on Austrian models, Cambridge and general equilibrium theorists have grander ambitions. The Cambridge theorists hope to develop an interlocking set of models whose applications can be tested and which will provide a general grasp of economic phenomena. The general equilibrium theorists hope to be able to refine their models sufficiently to make good the claims of mainstream theorists to have found the fundamental theory of economics. Austrian theorists might also strive for a distinctive vision of
economics as a whole. Their account of capital and interest, however, is clearly only one way of simplifying the story the general equilibrium theorists hope to tell correctly. If one cannot develop the fundamental theory for which general equilibrium theorists strive, one may never have models any better than those of the Austrians. Yet if theorists cannot fulfill the ambitions of all those who have regarded economics as a separate science, the Austrian theory seems an incongruous stopping point. If theorists cannot employ the basic equilibrium model to understand all the major economic phenomena, surely they should endeavor more diligently to develop nonunified, interdisciplinary, broader-ranging theories of specific economic phenomena. When one shifts from the practical question, "Which approach should I use to answer this specific question," to the theoretical question, "To which approach should I devote my efforts as an economic theorist?" the Austrian theory largely drops out of the running.

2. Strategies of Economic Theorizing and Their Implications

Equilibrium theories possess many virtues. With very few fundamental predicates they enable one to provide remarkably uniform accounts of extremely diverse economic phenomena. In these ways they are simple. The fundamental equilibrium model is extremely flexible, yet simultaneously provides detailed and precise instructions for dealing with new problems. Equilibrium models possess the sort of mathematical sophistication and elegance that are characteristic of advanced theories in the natural sciences. The basic equilibrium model is highly systematized and its scope is supposed to extend to the entire domain. Next to this body of theory Sraffa's work is pale and puny indeed. The most one can say for it is that it offers a new challenge. It is certainly not a well-developed competitor.

The many virtues of equilibrium theories help explain the monotheoretic strategy of mainstream economics. Yet these virtues are certainly in part the result of that strategy. It would be quite surprising if the basic assumptions of equilibrium models could not be elegantly formulated by now. After all, they have been formulated and reformulated by a century of economic theorists. If these assumptions were not, when first specified, fairly flexible, they would not have found their way into the foundations of neoclassical theory. Yet much of the flexibility is due to the efforts of later theorists. Consider, for example, how the device of dated commodities increases the potential applicability of the basic "laws" of equilibrium theory.

The power of equilibrium theorizing is great and has grown. It is easy to give rational reasons for the allegiance of so many economists to the same basic model and the same strategy for solving problems. Once one fully appreciates the distinctiveness of the strategy of mainstream economic theorizing, one can better understand some of the general methodological controversies mentioned in the Introduction and chapter 1. Once one appreciates some of the abuses or exaggerations which result from allegiance to this theoretical strategy, one can see how some of these controversies should be settled.

A. Definition of Economics

As I mentioned in chapter 1, economists have long disputed the definition of economics. Until the last century economics was regarded as the science of the production, exchange, distribution, and consumption of those goods and services that contribute to material well-being. In contrast to this "substantive" definition of economics, most economists now accept a "formal" definition. In Lionel Robbins' classic formulation, economics is "the science which studies human behavior as a relationship between ends and scarce means which have alternative uses." (1932:15). I suggest that this formal definition essentially defines economics as equilibrium theorizing.

To see why, one needs to understand what Robbins and other neoclassical economists mean by "scarcity." Robbins lays great stress on the notion of scarcity:

Scarcity of means to satisfy given ends is an almost ubiquitous condition of human behavior.

Here, then, is the unity of the subject of Economic Science, the forms assumed by human behavior in disposing of scarce means. (1932:15)

On the analytical side Economics proves to be a series of deductions from the fundamental concept of scarcity of time and materials,

... Unless it is made quite clear that in the marginal analysis we possess the basis for a completely unitary Economic Theory, it is safe to say that the inner significance of that analysis has not been recognized at all. (1932:77)

Like Walras, Robbins links the notion of scarcity to that of marginal utility. This is as it should be, since a good or service or resource is scarce in the neoclassical sense of "scarce" if and only if it possesses a marginal utility to somebody. We can thus see that Robbins is defining
economics to be marginal analysis. The "unity" of the subject lies in constrained maximization of utility. In economics, according to Robbins, one seeks and obtains "a completely unitary Economic Theory."

Given that marginal concepts are derivative and that neither marginal utility nor marginal productivity is supposed to have any causal role in a full equilibrium theory, the notion of scarcity ought also to be regarded as secondary or as derivative, at least by equilibrium theorists. In the spirit of Robbins' devotion to neoclassical theory, we should interpret him as defining economics not as marginal utility theory (which is not fundamental), but as equilibrium theory.

Once we have thus clarified the formal definition of economics and have recognized that it is equivalent to deciding by definitional fiat that equilibrium theory is all of economics, it is easy to see how unreasonable that definition is. The debate over the definition of economics has contemporary echo in general philosophy of science. Wolfgang Stuehmeyer maintains that scientific theories determine which phenomena they apply to (1976:93, 176–77). We can see that Robbins agrees. While it is certainly true that the way that one classifies phenomena is strongly influenced by the theories one accepts, both Robbins and Stuehmeyer err in denying that scientists possess any way of determining domains independent of the specific theories they are considering. The substantive definition of economics I gave above is crude, but in its crude way it picks out the phenomena with which economic theories must contend. The vogue enjoyed by the formal definition is merely one of those excesses which results from the strategy of equilibrium theorizing. It may turn out that equilibrium theory is indeed the fundamental theory of all economic phenomena. It is not such a theory by definition.

### B. General Laws

Behind the dispute concerning whether economics possesses general laws lies the same peculiarity of equilibrium theorizing. One plausible way of interpreting both Marx's and Veblen's criticisms of general laws in economics is to regard both as denying that mainstream theory is the basis for a separate science of economics. Marx and Veblen were, of course, objecting to different theories, but, as we have seen, the ambition of dealing with all major economic phenomena in a single unified theory antedates the development of marginal utility theory and explicit general equilibrium theories.

Consider what questions one might be asking when one considers whether economics does, can or should possess general laws. Many theorems of both classical and neoclassical theory are clearly parochial. Outward circumstances, even the very existence of markets, vary from society to society. On the other hand there are obviously some generalizations which are true of all economies. As Marx noted (1973:85) all production requires some sort of instrument of production. The controversy concerning whether economics should seek or currently possesses general laws is best understood as a controversy concerning the proper strategy of economic theorizing. In its quest for a set of assumptions which can be applied to manipulate and analyze all significant economic phenomena, mainstream theorizing bases economics on a set of general laws. While not denying the existence (or even necessarily the utility) of such general laws, both Marxian and institutionalist economists have argued that there is more to be learned by focusing on the distinctive features of the phenomena studied. According to Marx and Veblen, economists can most profitably employ laws which are not general and best explain phenomena by citing factors which are not primitives in some equilibrium theory. Of course, they may be mistaken. For all that I have said, it may be that one will always learn more through developing equilibrium theories. There is, however, no warrant for attempting to rule out a priori piecemeal approaches like the one suggested by Sraffa's work.

How one defines economics and how one regards the laws of economics are thus crucial to how one conceives of the relations between economics and the other social sciences. From the perspective of equilibrium theory, economics stands apart. Lionel Robbins, for example, vehemently denies that economics "rests upon any particular psychological doctrines."

If, therefore, Economics rests upon particular psychological doctrines, there is no task mere ready to hand for the intellectually sterile, than every five years or so to write sharp polemics showing that, since psychology has changed its fashion, Economics needs "rewriting from the foundations upwards"... and the lay public, ever anxious to escape the necessity of recognising the implications of choice in a world of scarcity, has allowed itself to be bamboozled into believing that matters, which are in fact as little dependent on the truth of fashionable psychology as the multiplication table, are still open questions on which the enlightened man, who, of course, is nothing if not a psychologist, must be willing to suspend judgment. (1932:84)

At first glance this scathing attack on those who regard economics as resting on psychological foundations might appear puzzling. After all,
the basic laws of utility theory are, in a perfectly ordinary sense of the term, psychological. The puzzlement fades, however, when we recognize that Robbins is here espousing the vision of economics as a separate science. Economists do not seek more refined psychological laws which will, for example, help explain panic behavior in certain markets. Such an investigation, in Robbins’ view, is scarcely a part of economics. Economists have their basic model. Their tasks are to refine and apply it. Economics is complete unto itself. Although some Marxian theorists even more vehemently deny the importance of psychology, Marxian, institutionalist, and Sraffian theorists have no such prior commitment to rejecting the findings and tools of the other social sciences.

C. Methodological individualism.

It is often said that equilibrium theory is individualistic, that it is not sufficiently holistic. The charge is ambiguous. Equilibrium theory is anti-holist, since it treats economic phenomena as isolable, at least for purposes of analysis and control, from more general social influences. It is also individualistic, since the bulk of its fundamental generalizations concern psychological features of individuals. Yet in one important regard it is holist. Although one may disregard certain interconnections among economic phenomena for practical purposes, true equilibrium is general equilibrium, a simultaneous and mutual determination of properties of the whole economy.

A certain measure of individualism seems to me sane and sensible. Virtually all significant social scientists, including in practice Marx and Durkheim, insist that the “laws” one invokes in explaining social phenomena concern features of individuals or themselves be explainable in terms of other individualistic laws. Such a view is not reductionist, in at least one sense of “reductionism.” One is only insisting that the laws be individualistic or psychological. The specifications and simplifications may mention groups, institutions, etc. Except in so far as one is criticizing neoclassical economics for its overly restrictive theoretical strategy, I do not think it is reasonable to object to its methodological individualism.

1 See O’Neill (1973), Ryan (1973), and Morgenbesser (1967a) for some of the main contributions to the extensive and often confused debate concerning methodological individualism.

D. Naturalism:

All of the economic theories I have considered are in most ways naturalistic. The debate over social scientific naturalism which has occupied philosophers is extremely messy, because there are a number of different questions involved. Social scientific naturalists argue that the social sciences are in certain important respects similar to or at one with the natural sciences. Antinaturalists argue that the social and the natural sciences differ. Until one is more specific about the comparisons and contrasts involved, one knows little about the debate (see Morgenbesser 1970). Naturalists have argued that, at a certain level of generality, the goals, conceptual structures, and methods of social sciences are the same as those of the natural sciences. They have also sometimes argued that the social sciences can ultimately be reduced to the natural sciences. Anti-naturalists have argued for differences along one of these dimensions.

I began my investigation of theories of capital and interest and their relations to exchange values presupposing that the general goals, methods, and logical structure of economic theories were the same as those of the natural sciences. Whenever I found economic theories lacking, I needed to consider whether the naturalistic presumption with which I began had led me to apply an incorrect standard. I found little reason to reject that presumption.

One well-known complication demands some attention. The concepts economists employ differ from those of the natural sciences in one noteworthy way. When economists explain the behavior of an individual, they do so in terms of the agent’s beliefs and desires. Desires are, of course, dressed up as utility functions, while beliefs are made transparent by the assumption of perfect information. Both belief and desire are intentional concepts. As many have noted (Quine 1960, chs. 4, 6; Chisholm 1957, ch. 11), verbs like “believes” or “desires” possess logical peculiarities. These logical peculiarities are weeded out of

2 Let me note three of these peculiarities (Chisholm 1957:170–71). From a non-intentional sentence like (1) Samuelson teaches at MIT one can infer that both Samuelson and MIT exist. From (2) Consumers prefer noncarcinogenic artificial sweeteners one cannot infer that there are any noncarcinogenic artificial sweeteners. From intentional sentences, one cannot infer whether the referents of the terms exist.

Second, from an ordinary compound sentence which contains a propositional clause, one can infer whether the proposition embedded in the clause is true or false. From (3) The big cars American manufacturers built sold well one can infer (4) American manufacturers built big cars. But (5) Ford Motor Company hoped the Edsel would sell well is entirely consistent with the sad truth (6) The Edsel did not sell well.

Finally, the truth or falsity of nonintentional sentences is unaffected when one sub-
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Economists sometimes argue that the “laws” of utility theory ((1)-(5) ch. 6, 82) and possibly the claim that entrepreneurs are profit maximizers are not generalizations about the behavior of people. Instead, they prescribe how rational agents behave or how rational economic agents behave. What I have identified as fundamental “laws” of equilibrium theory are often regarded as definitions of a rational agent or of a rational economic agent. Moreover, economists and philosophers have urged that since these statements articulate what rationality is, one has good reason to accept them (Rosenberg 1976b: 106-7, 129, 137-38). According to these authors, one should not assess such claims the same way one asesses purported laws in the natural sciences.

These views are confused. The basic “laws” of equilibrium theory do not define “rationality.” There is nothing irrational about being satiated or having some increasing marginal utilities or changing one’s tastes. One can, if one wants, treat the basic claims about individuals as defining “a rational economic agent”—that is, as assumptions in a model. But definitions are not enough. Once theoretical hypotheses are offered, closures of the assumptions about individuals are recovered as empirical generalizations. Nor should one or can one apply economic theory only to rational agents. Economists have no specification of what it is to be rational other than that given (inadequately) by the fundamental “laws” of equilibrium theory. To say that equilibrium theory applies only to rational agents is thus to say that it applies only to what it applies to. At best one is rehearsing the excuses one may want to give when the theory fails. Although everyday experience, as well as introspection, provides some evidence for economists’ claims, there is nothing privileged, a priori, or intuitively evident about the basic model they employ. Behavior of the sort assumed in the model is in some ways rational, but that gives one no reason to cling to the model or to believe, without testing, that it applies in any particular domain.

Nowhere in this book have I offered any proof that anti-naturalists are mistaken. I have instead done my best to present a coherent and compelling naturalistic construal of economics. The anti-naturalist is correct in asserting that economists make use of intentional verbs like “believes” and “prefers” and explain individual behavior in terms of the reasons for that behavior. Yet I do not believe that these facts establish an anti-naturalist case. The intentionality of beliefs and desires is neutralized by the assumptions economists make. Explaining individual action in terms of reasons for the actions, especially within

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Economic theory by the assumptions of perfect information and complete and transitive preferences. Those cases in which the intentionality of belief and desire matter violate economists’ ceteris paribus conditions.

The fact remains that economists explain individual behavior in terms of the beliefs and desires of agents. Economists are not greatly concerned with the behavior of the individual; but their theory depends upon individual choice and action. In explaining behavior in terms of beliefs and desires, economists are explaining behavior in terms of the agent’s reasons. In ordinary speech, an agent’s reasons for performing an action may differ from the causes of the action. An agent may cite as his or her reason for closing a window the “fact” that it was drafty. The draft (if there was one—there need not have been) is not necessarily (although it might have been) what causes the agent to close the window. Perhaps the agent was given a post hypnotic suggestion or possessed an unconscious aversion to the smell of the roses outside the window. Can there be scientific laws relating reasons and actions or concerning the components of reasons themselves? Can reasons for actions be causes as well?

These are large questions and have been often discussed. I shall rely on the arguments offered by Davidson (1963), Rosenberg (1976b, ch. 5) and Goldman (1970, ch. 5). Like these authors I believe that reasons can be causes.

I should say a bit more about one argument which many anti-naturalists have found compelling, since in the writings of economists like Lionel Robbins it connects to the monotheoretic strategy of neoclassical economics. Economists sometimes argue that the “laws” of utility theory ((1)-(5) ch. 6, 82) and possibly the claim that entrepreneurs are profit maximizers are not generalizations about the behavior of people. Instead, they prescribe how rational agents behave or how rational economic agents behave. What I have identified as fundamental “laws” of equilibrium theory are often regarded as definitions of a rational agent or of a rational economic agent. Moreover, economists and philosophers have urged that since these statements articulate what rationality is, one has good reason to accept them (Rosenberg 1976b: 106-7, 129, 137-38). According to these authors, one should not assess such claims the same way one asesses purported laws in the natural sciences.

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the confines of perfect information, does not imply that economics cannot share the goals, the methods and the logical structure of natural scientific theories.

Interesting loose ends remain. How serious are the complications of intentionality once one admits uncertainty? Economic theories achieve their straightforward logical structures by using \textit{ceteris paribus} clauses and simplifications to rule out the complexities that beliefs and desires can introduce. If these complications, from which the theories abstract, are not themselves subject to scientific analysis and explanation, can one still claim to have a naturalistic science of economics? The arguments here are quite unclear (see Davidson 1976 and Knight 1956:245–46).

Compared with the best the natural sciences have to offer, theories of capital and interest are inadequate; but that comparison is not the relevant one to make, if one wants to know whether economics is a scientific discipline. The principal difficulties confronting theories of capital, interest, and value are in isolating and testing the regularities they point to. I have given no arguments for social scientific naturalism; but I have found little reason to regard the difficulties that economic theories face as refutations of social scientific naturalism.

3. \textbf{Philosophical Theses and Puzzles}

In the course of this inquiry, philosophical theses, arguments and conclusions have played an important part. I have avoided addressing philosophical issues in a completely general way. Instead I have considered only those issues important for understanding theories of capital and interest and have only attempted to defend the validity of my philosophical conclusions for the particular economic subject matter I have addressed. Yet many of the questions I considered are of general importance and interest. In considering theories of capital and interest, I have addressed six sets of philosophical issues. I shall briefly sketch here for each set the general questions, the suggested solutions and the problems remaining for further investigation.

\textbf{A. Models and Theories (ch 3, §2, §3; ch 6, §2, §3; ch. 7, §1, §5)}

Crucial to any philosophical discussion of economics or of any science is a careful construal of the structure of the discipline’s theories. Economists think and write in terms of models, which consist of assumptions. I attempted to analyze theoretical work concerning capital and interest in these terms. Models in theoretical economics can be construed in the way that Suppes, Sneed, Stegmueller, and Giere have analyzed scientific theories—as predicates or as definitions of predicates. Theoretical hypotheses assert that these predicates are true of something. Theories are sets of related statements. These statements are implied by theoretical hypotheses. The assumptions of models become, when closed or applied, the statements of theories. Special case models are those which are employed to illustrate, teach, develop, or criticize more general models or, when (hypothetically) closed, to confirm more general theories.

This conception of models and theories enables one to analyze the work of economic theorists as simply as any other conception and to employ the terms which economists themselves use. I could have employed other analyses of scientific theories without substantially changing my conclusions (see Hausman 1978). Doing so would, however, have complicated the exposition. One’s choice of a conception of scientific theories should be based on which conception is easiest to use and which enables one to describe the efforts of the given scientists most intelligibly. I have risked confusing my readers by using \textit{“model”} to mean what Suppes, Sneed, Stegmueller, and Giere mean by \textit{“theory”}; and by then using \textit{“theory”} in a more traditional way. Perhaps my terminological change and the compromise that results will be useful in other work in the philosophy of science. I suspect that what is most substantial and interesting about the structure of scientific theories depends heavily on the particular domain.

\textbf{B. Explanatory Arguments (ch 7, §1, §6)}

In assessing economic theories one must face the question of how arguments full of apparently false premises can be explanatory. Although this question is particularly vivid in the case of economics, it is quite general. I argued that one should not attempt to answer it by searching for a new model of scientific explanation. There is nothing to be gained in developing a model of \textit{“messy”} explanation. In fact I endorsed the standard deductive-nomological model of nonstatistical explanatory arguments. In doing so, I did not intend to ignore or conceal the roughness of the fit between the model and the arguments which scientists consider to be explanatory. Once one recognizes that literally false sentences can make true assertions (and that there is a large and diverse category of \textit{“near explanations”}), one can capture
the judgments of scientists concerning which nonstatistical arguments are explanatory without surrendering the deductive-nomological model. Proceeding in this way, one can acknowledge the complexity and messiness of actual scientific explanation without one's own analysis becoming messy or unwieldy.

Many loose ends and open questions remain. Statistical explanations, particularly the sloppy ones given by economists, create additional complications, which I have avoided. Much further work on causality in economics is needed before one can reach a balanced assessment of economic explanations. Questions about the completeness and generality of my analysis of nondeductive explanatory arguments remain as well. Does the analysis apply in other social sciences which lack the mathematical (deductive) structure of economics? Am I right to discount the importance of Verstehen (subjective understanding) in economic explanations?

C. Simplications and Idealizations (ch. 7, §4, §5)

A simplification is a statement in a theory or explanation or other application which a scientist makes use of, although he or she has no reason to believe it is true. Often one has good reason to believe that simplifications are false. In many cases simplifications are idealizations—statements which must be false. The presence of simplifications in apparent explanations is problematic. If one knows the simplification to be false, one knows that the apparent explanation does not, as stated, satisfy the conditions of the deductive-nomological model. If one has no reason to believe the simplification to be true, one has no reason to accept the purported explanation.

There is no mystery about the prevalence of such simplifications in explanatory arguments. Scientists often cannot specify accurately the initial conditions and could not deduce the consequences if they did know all the initial conditions. Substituting simplifications, they offer what I call "explanations in principle." The puzzle comes in understanding how explanations in principle can be explanations. How can one accept an explanation which contains false statements? I have considered one solution to the puzzle. Sometimes, after reinterpreting

1 In other cases, not discussed in the text, there may be no way to interpret the simplification as a true statement. Instead scientists believe it can be replaced by a true statement without appreciably affecting the explanation. In this case, one does not strictly possess an explanation, but one possesses a near-explanation or is close to being able to explain. Such near-explanations are considerable scientific achievements and can greatly help us understand, even though they do not fully satisfy the necessary conditions on scientific explanations.

the simplification as a statistical claim or with a margin of error, one can regard it as a less specific but nevertheless true statement. The argument may then be interpreted as an explanatory argument, although the description of what is to be explained can no longer be deduced or must itself be made less specific.

To conceive of explanations in principle in this way raises questions of justification. When does one possess good reason to believe that simplifications are thus legitimate? I have suggested four necessary conditions, which in outline are:

1. Confirmation condition. By employing the simplification, one can derive testable consequences and confirm many of them.
2. No-accident condition. One can understand why the confirmation condition is satisfied.
3. Sensitivity condition. If one replaces the simplification with a more realistic alternative, one's explanation becomes more accurate.
4. Convergence condition. In those circumstances in which the simplification is a better approximation, one's explanation is more accurate.

These conditions seem consistent with the judgments scientists make. We can also understand why it is rational to demand that a simplification satisfy these conditions before one employs it in an explanation.

On this analysis explanations in principle employ simplifications which are approximate. One is justified in accepting an explanation in principle only if its simplifications satisfy the four conditions. The most important question this analysis raises is one of generality. How well does my schema stand up to the evidence provided by the history of various sciences? Are there other kinds of explanations in principle which I have overlooked? How useful are my justification conditions in understanding and assessing the work of scientists in other domains?

D. Inexact Laws (ch. 7, §2, §3, §5)

Actual explanatory arguments in the sciences are messy not only because they include simplifications. The "laws" such arguments invoke are usually, as stated, not quite true. This problem is particularly striking in the case of economics; the inaccuracies in the basic "laws" are glaring.

There are a variety of ways in which one can regard generalizations that face apparent disconfirmations as laws nevertheless. One might regard them as statistical, rather than as deterministic laws—although without better specification of the statistics involved, this interpretation
seems a mere rechristening. One might regard such generalizations as
approximate laws—laws which are true with a margin of error. Loos-
ening the statement of the generalization, the disconfirmations may
disappear. Third, one might regard the true law as a modal claim. The
law asserts what would happen were certain conditions met. Since
actual circumstances differ from the hypothetical circumstances, things
do not always turn out as a nonmodal reading of the law would require.
The disconfirmations are, however, only apparent. In each case the
antecedent of the law is unsatisfied. Finally, one might regard general-
izations which face disconfirmations as qualified laws. Implicit in the
law is a \textit{ceteris paribus} clause. Apparent disconfirmations of the un-
qualified generalization occur when the \textit{ceteris paribus} clause, which
may be regarded as determining a predicate in the antecedent of the
law, is unsatisfied.

Without any constraints one could use these devices (especially the
last two) to interpret any generalization as a law. Yet merely incanting
the words "\textit{ceteris paribus}" or the words "how things would be, were
..." does not transform a false generalization into a law. If a non-
statistical generalization appears to be false, even given a margin of
error, one cannot justifiably regard it as a law unless certain condi-
tions are met. I have proposed the following four necessary conditions:

(1) Lawlikeness.
(2) Reliability. In some independently specified class of cases with no
qualifications or with specific qualifications only, the generalization
must be disconfirmed only rarely and confirmed often.
(3) Refinability. Adding further qualifications to the generalization
should increase the frequency of confirmations and decrease the
frequency of disconfirmations.
(4) Excusability. Scientists should almost always be able to discover
the interfering factor responsible for an apparent disconfirmation.

I have argued that one is justified in regarding a generalization that
faces apparent counterexamples as a qualified law only if these four
conditions are met. These conditions must, I think, also be met before
one can regard inexact generalizations as modal laws. I have not,
however, considered the justification of modal claims in any detail.
Spurred by metaphysical modesty and trepidations about the tem-
\pation of counterfactual talk, I prefer to interpret economic "\text{laws}"
(and scientific laws generally) as qualified rather than modal claims.

Laws which are statistical, approximate, qualified, or modal can all
be employed in explanatory arguments, although in each case one must
modify the argument slightly. Qualified laws fit into the deductive-
nomological model easily. All one needs is the additional premise that
other things are equal. On my analysis such premises may well be true.
Questions remain concerning when we are justified in believing that
such premises are true. The conditions above provide materials for an
answer.

Loose ends remain. Philosophers have often noted, but have rarely
carefully analyzed the inexactness of scientific laws. Which laws, if
any, are true as stated without qualification or modal reinterpretation?
Is it correct to interpret such phrases as "in the absence of other
forces" as more specific \textit{ceteris paribus} clauses? How does the pres-
ence of \textit{ceteris paribus} clauses limit scientists' ability to deduce new
laws from a number of known laws? Are there kinds of inexactness other
than those I have discussed? Should one attempt to interpret
explanatory rough generalizations, as I did, as laws at all? Might it not
be better to change one's model of explanation to permit explanation
without laws? How adequately do my justification conditions agree
with the judgments good scientists already make? How useful are they
in philosophical investigations of scientific explanations?

\textbf{E. Causal Judgments (ch. 9, §1)}

The deductive-nomological model only provides necessary condi-
tions which explanatory arguments must satisfy. Not all deductive-
nomological arguments are explanatory, nor are all arguments which
satisfy the eight justification conditions of chapter 7 deductive-nomo-
logical or explanatory. When economists deny that the marginal pro-
ductivity of labor explains the wage or that Sraffa's derivation of prices
explains prices, they are not usually denying that the arguments are
deductive-nomological. Instead they question whether the pur-
ported explanation is causal. Economists deny that the marginal product
of labor explains the wage, because they deny that the marginal product
of labor \textit{causes} the wage to be of a certain size. They deny that Sraffa's
derivation of prices from the size and composition of output, the tech-
nology employed, and the rate of profit explains prices, because they
deny that these factors \textit{cause} the prices.

Yet when we look more carefully, we see that economists lack ra-
tional principles upon which to make such judgments. The principles
economists like Bliss or Wold explicitly invoke are inadequate and
implausible. The problem of distinguishing causal relations among
economic variables is largely open. Much further research is needed.
The philosophical question, "What further conditions must arguments
satisfy to be explanatory?" is an important practical question for economists.

**F. Theory, Domain, Method and Strategy (ch. 6; §2, §3; ch. 9; §2, §3; ch. 10, §2)**

In analyzing theories of capital and interest, we faced a number of connected questions concerning the relations between neoclassical theories and the general project of neoclassical theorizing. The issues which arose were confronted by the research leaders like Kuhn (1970), Lakatos (1970), Laudan (1977) and Shapere (1977). I did not, however, attempt to relate the questions considered to such philosophical discussions of larger structures in scientific theorizing. Rather than seeing in economics the general importance of paradigms or research programs or research traditions, I observed a strategy of research and application that grew out of the specific dominant theory. If the program for economic theorizing suggested by Sraffa's work were dominant, one could, no doubt, still identify some sort of paradigm or research program, but the differences between that program and the one which dominates orthodox theory are much more significant and interesting than are the similarities.

Equilibrium theory is a grand theory. Given principles governing individual choices and constraints on production, one seeks those prices which will coordinate and harmonize the plans of individuals. Ideally one solves simultaneously for prices, quantities produced, actual technology employed, incomes, and actual consumption. The theory of how individuals exchange under constraints appears from this point of view virtually to exhaust the subject matter of economics. Economics is redefined as the science which studies the applications of equilibrium theory. The many important questions which equilibrium theory cannot answer are, when possible, regarded as questions for some other discipline. The circle never completely closes, however. Equilibrium theory remains subject to challenges and criticisms since, at least through its policy implications, economics is continually (though quite indiscernably) tested. Applications of equilibrium theory must work. When they fail, excuses, of course, abound. Given the extent of the failures, economists should, however, be eager to explore alternatives.

In fact, despite the limited success of equilibrium theories, there is little support for alternatives and, indeed, great resistance to entertaining any. There are many reasons for such fervent adherence to equilibrium theory, the most important of which is that equilibrium theory provides economists with so much guidance. Regardless of its empirical adequacy, equilibrium theory remains an incredibly flexible tool. Faced with almost any problem recognized as economic (problems of national income and aggregate demand being the main exceptions), equilibrium theory provides virtually mechanical rules for analyzing and solving it. None of the competitors, particularly not the piecemeal theorizing of the Cambridge economists, offers the possibility of such a systematic grasp of the subject matter. Given the enormous complexities involved in any general testing, this essentially heuristic virtue becomes paramount. There are also, to be sure, less rational causes for the allegiance of economists to equilibrium theory. The force of habit is appreciable. Equilibrium theory is more acceptable on ideological grounds to business and government than are most of its competitors. Those who possess great economic power would certainly prefer that everyone conceive of competitive economics as great cooperative ventures. I see no grounds for the allegation that the allegiance of economists to equilibrium theory rests mainly on habit and ideological grounds. Neither do I see any grounds for denying that these nonrational influences are important.

Regardless of its causes, a rich nexus of connections between equilibrium theory, what counts as an "economic" phenomenon, and the method of equilibrium theorizing remains. This nexus is too complex and too dependent on the peculiarities of the given theory to fit into a general philosophical vision of the larger structures of scientific theorizing. The characteristics of the neoclassical enterprise could, however, be highlighted by detailed comparison with such philosophical visions. Detailed comparisons of the strategy of equilibrium theorizing with the development of other theories in the natural and social sciences might also be enlightening. There is room for much further research.

These, then, are the main philosophical issues which I have touched on in this book. None are digressions. To analyze and assess theories of capital and interest these issues had to be faced. Nor should these issues have been faced in a purely philosophical context, with only the results figuring in this inquiry into capital theory. For it is principally in contexts like this inquiry that such issues must be decided.

**4. Ideological Criticisms**

Two kinds of comments can be made about the ideological relevance of theories of capital and interest. In this section I shall outline what
an ideological critique is and discuss ideological criticisms of aspects of equilibrium theorizing. To offer an ideological critique is (1) to show that the assertions in question are false, and moreover, given the evidence available to the author and the author’s methodological commitments, that they ought not to have been made, and (2) to explain the mistake in terms of the author’s social role. There must be a mistake; conscious deception is not ideology.

One can offer ideological criticisms of applications of equilibrium theory and of methodological dicta designed to condemn all alternatives. The following comments of Irving Fisher’s exemplify a common kind of ideological misapplication of equilibrium theory (including here a theory of capital and interest):

It is true, as the socialist maintains, that inequality is due to social arrangements, but these arrangements are not, as he assumes, primarily such as take away the chance to rise in the economic scale; they are, on the contrary, arrangements which facilitate both rising and falling, according to the choices made by the individual. The improvident sink like lead to the bottom. . . . But the great masses, once they get near the bottom, are likely to remain there. Their high rates of impatience manifested through generations have brought many if not most of them to poverty. . . . They are a self-selected group of those impatient by nature or habit or both. They tend to spend rather than to save. (1930:339–40)

These comments are glib and unjustified by the laws of equilibrium theory. They are not the sound conclusions of rigorous argument. That they are false does not require, I think, serious argument; although similar claims emanate from politicians whenever questions of welfare policy are publicly debated. They may also be explained (very roughly) by Fisher’s social role (he was an economist respected by the business community).

Social and natural scientific theories are often misapplied. One has not shown that there is anything wrong with equilibrium theory when one points out that it has been misused (See Sidgwick 1885:30). More seriously, one can criticize the methodological demand, enunciated by Robbins and others, that economics be devoted to the application of equilibrium models. This demand dismisses or regards as dubious theorizing like Sraffa’s. It excludes from economics, or treats as secondary, all questions concerning the dependence of individual motivations or technology on the state and development of the economy. In certain economic theories, such an exclusion is justifiable. To theorize, on the other hand, about large scale economic growth and development by applying an equilibrium model seems unjustifiable. This error might be explained by pointing out that economists have long had the job of portraying capitalist economic organization (although not necessarily all its details) as optimal and in some sense the result of voluntary individual choices or of regrettable perturbations. Mainstream economists have avoided the question of how the broader economic system reproduces itself (and how it brings about its own transformation). This ideological criticism is only addressed to the pretense that equilibrium theory is, in outline, the whole of economics and that its challengers are ipso facto mistaken. I have offered no ideological criticism of equilibrium theory itself. I see no way to do so, because I see no obviously avoidable mistakes in equilibrium theory.

5. Capital Theory and Liberal Ideology

Instead of seeking ideological criticisms, one might discuss the relations between theories of capital and interest and systematic bodies of attitudes called “ideologies.” In giving an ideological critique, one need never refer to an ideology. In speaking of ideologies one need not engage in ideological criticism. The two enterprises are distinct. By an “ideology” I mean a short description or theory of society and of human beings about which a large number of people in a certain society have two beliefs. First that this theory provides the best short description available, that it captures the essential features of society; second that it is a description of a good society or of a society that must and can be transformed into a good society. As a result, this description serves as the relevant standard for assessing individual conduct and social policy. Different varieties of Marxism and of liberalism have been ideologies in this sense. I shall deal with one question to which talk of ideologies sometimes gives rise. The basic equilibrium model is a refinement of the theoretical vision sketched in chapter 1. That vision is related to some forms of liberal ideology. What is that relation and how does it help one to understand theories of capital and exchange value?

Marxist or liberal ideologies are not sophisticated philosophical doctrines, but popular creeds. They thus resist any precise definition. Nevertheless, I hope the reader will grant that the basic vision of a competitive economy is a recognizable part of classical liberal views of society. Such a view is less prevalent today than it was a century ago, although it may be making a comeback.

The sort of liberalism expressed in the popular agitation against the
have, obscure argument, methodological disagreement and difficulties in testing are only to be expected.

Mill's comments, with which I began this book were a suitable prologue; many differences of opinion in capital theory are caused by different "conceptions of the philosophic method of the sciences." Yet the choice of a methodology is not the spontaneous act of a free will. Different "conceptions of the philosophic method of the sciences" may originate in different views concerning what society is and what it ought to be. The philosophy of science leads one in thinking through capital theory, to the study of ideology.

The relations between capital, profits, and prices remain perplexing. Economists have offered plausible hypotheses, but they are still unable to understand the phenomena. Their efforts have been subtle and intelligent, but they have not yet succeeded. Why not? Are the questions just too difficult? Does this failure reveal some fundamental inadequacy in the dominant theory, perhaps due to the influence of ideology? We know that people feel strongly about profits and the economic power that capital brings. We know that most economists rigidly cling to a theory whose "laws" are dubious and whose simplifications border on the outlandish. Yet neoclassical theory also has immense heuristic power and is unchallenged by any evidently superior alternative. With these doubts and a plea for methodological tolerance I close this book, but not this inquiry. There is too much still to learn.

Corn Laws, for instance, not only accepted the vision of economic life expressed by the three claims of chapter 1, §3, but regarded that vision as a sort of practical ideal. Liberal ideology thus links economic and moral theorizing. Economic and moral theories should not, be regarded as part of any ideology, although they may be inspired by or support an ideology. Sophisticated theories rarely fit perfectly into any ideology; they always rely upon evidence and traditions that transcend ideologies, and they are, in any case, far too complex to permit mass understanding or to command mass allegiance. To point out the relationships between moral and economic theories and ideologies is not in my view to criticize those theories. The claims ideologies make are not necessarily false. Those who are influenced by an ideology can perfectly well do conscientious and rigorous theoretical work.

The vision of the market as reconciling efficiently the plans of self-interested individuals through their voluntary exchanges is a crucial part of liberal ideologies. That vision is central to general equilibrium models. Indeed one might point to equilibrium theory as a scientific success to which an ideology has led. Not all economists who employ equilibrium models accept any version of a liberal ideology, but the historical and heuristic connections are clear. If the discussion in chapter 1 is correct, accepting a liberal view of economic life commits one to regarding the theory of exchange value as the fundamental economic theory. The liberal vision of economic life thus leads to the conception of economics as a separate science. If the properties of economies result from voluntary exchanges between rational self-interested individuals, one ought to be able to explain economic phenomena entirely in terms of the principles regulating individual behavior and the constraints on exchanges. Economists should be able to achieve a unified theory, since all major economic features of a competitive economy result from voluntary exchange behavior. Obviously this is an oversimplification; but ideologies are oversimplifications. Mill's and Robbins' methodological positions, although congruent with the vision presented, are more sophisticated.

We can thus see another link between the method Sraffa uses and his demotion of the theory of exchange value from its central position. Given a liberal vision of the functioning of competitive economies, Sraffa's work makes little sense in either regard. Such a vision is, however, neither unchallengeable nor unchallenged. Sraffa's work is methodologically legitimate. So are neo-Marxian efforts to make use of Sraffa's work. When a dispute about the details of an esoteric subject has such broad ideological ramifications as the capital controversies