CHAPTER NINE
Philosophical Assessment of Sraffa’s System

In Chapter 8 I presented a simplified Sraffa system. Although more complicated cases demand further apparatus, that system reveals the essentials of Sraffa’s work. He shows that one can calculate prices without mention of utility or production functions whenever one is given enough information.

The implications and significance of Sraffa’s work are not clear. Some of his interpreters have greatly exaggerated the consequences of Sraffa’s formal achievement; his work does not show that exchange values are unaffected by demand, nor does it show that the rate of profit or of interest is not the result of individual exchanges. In addition, it does not show that the distribution of income is determined by the relations between capitalist and workers. Other theories, compatible with Sraffa’s work, may enable one to reach these conclusions, but they are not consequences of anything in The Production of Commodities by Means of Commodities. One might argue that Sraffa does provide some reason to believe that capital is not a commodity or service and that interest is not the price of capital, but well-informed neoclassical economists do not believe that capital is a single commodity or service.

Sraffa does not provide a theory of capital or of interest. Sraffa’s work might be usefully combined with some theory of capital or of interest, but it is not itself a theory of either. Sraffa does say what capital is—the value of the produced means of production—but he says nothing about what determines its quantity or its growth. The rate of interest or of profit is just given exogenously. At most, Sraffa provides a theory of exchange values and of the relations between exchange values and the rate of profit or interest. The extent to which Sraffa’s work is not only compatible with equilibrium theory, but is a kind of equilibrium model does not preclude the possibility that Sraffa has provided a distinct theory of exchange value. Has Sraffa in fact provided such a theory? As in the assessment of general equilibrium models we face here questions which are as much philosophical as they are economic.

In considering whether Sraffa has provided an alternative account of exchange values or of the relations between interest and exchange values, we shall encounter other interesting questions. Sraffa’s approach to theorizing is quite different from the sort which characterizes equilibrium models. The differences need to be clarified and evaluated. Through this clarification we shall come to a better understanding of the foundations of mainstream economics.

I. Does Sraffa Explain Exchange Values?

It is not clear whether Sraffa’s work satisfies the conditions a theory must satisfy in order to be regarded as explanatory (chapter 7). At least it comes closer to doing so than do general equilibrium models. Sraffa’s work contains only one lawlike generalization, FI, that the rate of profit is equal in all productive activities. This generalization is, I think, lawlike and in many important domains reliable, refinable, and excusable. There are natural classes of circumstances in which, after one allows for risk and perhaps prestige, the rate of profit in most firms is roughly equal. Adding further qualifications, the generalization can be made more reliable or reliable in more domains. When it fails, economists can usually point to specific interferences to explain why. Whether FI satisfies the reliability, refinability, and excusability conditions is an empirical question. I suspect that most economists would agree with my belief that FI does satisfy these conditions. FI seems at least as reliable as the fundamental general statements of equilibrium theory.

Less clear, however, is whether the simplifications Sraffa employs satisfy the confirmation, no-accident, sensitivity, and convergence conditions. Until empirical work is done there is no way to know whether Sraffa’s system satisfies the confirmation condition. Applying Sraffa’s work on the scale of a whole economy is beyond our mathematical capabilities, but there is no reason why one could not attempt to apply it to some sub-economy after making adjustments for the openness of any sub-economy. Given how little “theory” there is to Sraffa—given the weakness of Sraffa’s assumptions—economists are not in much doubt about the results of such tests. If Sraffa’s work can be confirmed, that fact would not seem an accident. Moreover it seems
that, when one introduces the complexities of joint products and the like, the results of the calculations should be better confirmed. More dubious is whether Sraffa’s work satisfies the convergence condition. Lipsey and Lancaster’s results (1956–57) provide possible grounds for doubt. Is it possible that the elimination of a monopoly, for example, might decrease excess demand yet result in prices which are in worse agreement with those calculated from Sraffa’s apparatus? In most of the cases in which Lipsey and Lancaster show that eliminating some obstruction to competition brings one further from a competitive equilibrium, one is also moving to situations in which Sraffa’s assumptions are less well satisfied. Given current knowledge one can, however, compare Sraffa’s work favorably here with general equilibrium models. Current abstract general equilibrium models clearly do not satisfy the confirmation and convergence conditions. Sraffa’s work may satisfy them.

Yet few mainstream economists would concede that Sraffa provides an explanation of exchange values. Remember that the conditions of chapter 7 are only necessary conditions. Sraffa’s work may satisfy those conditions yet still not be explanatory. Economists have, in fact, explicitly challenged the explanatory worth of Sraffa’s system for reasons other than its ability to satisfy such conditions. Consider the following comments of Y. K. Ng:

For example, Sraffa’s conclusion that prices are determined by the methods of production leaves unexplained why a particular system of production is being used. Since producers are able to vary input proportions, at least over time, this explanation cannot be advanced without bringing in the preferences of consumers which are completely absent from Sraffa’s system. In other words, Sraffa’s system of analysis . . . simply ignores the very real role of consumer preferences and hence cannot serve as a relevant explanation of price formation and income distribution. (1974:121–22)

Ng is evidently arguing that Sraffa’s work is in some ways inadequate as an explanation of exchange values or “price formation.” In what ways does Ng find Sraffa lacking? Is he right to conclude that Sraffa’s system “cannot serve as a relevant explanation”?

Ng is criticizing at least the narrowness of the scope of Sraffa’s work. It “leaves unexplained why a particular system of production is being used.” This criticism is justified. Sraffa’s system contains at most one lawlike statement. As a result its explanatory power is limited. Isolated “laws,” as compared to theories, have limited scope and provide relatively superficial explanations. Since the evidence upon which theories are based is more diverse and usually less closely related to the particular data to be explained, theories provide better explanations. In addition, one is less certain that unexplained generalizations are laws than that the highly systematized statements of a theory are. As a narrow system with only one lawlike claim, Sraffa’s work has little explanatory power.

Second, Ng may feel that Sraffa’s work is not explanatory, because it provides no causal account of the determination of exchange values. As Sylvain Bromberger points out (1966:71), the derivation of the height of the Empire State Building from some specific data and the principles of geometrical optics satisfies the conditions of the deductive-nomological model just as adequately as the derivation of the length of the shadow. Yet we believe that the latter derivation explains the length of the shadow while the derivation of the height of the Empire State Building does not explain why it has that height. What is the difference? One answer with considerable initial plausibility is that the derivation of the length of the shadow tells a causal story, while the derivation of the height of the building does not. Many scientific explanations are causal explanations. Ng may be questioning whether Sraffa provides a causal explanation.

Sraffa’s work is not in any strong sense causal. Consider one simple analysis of causality in economics. H. O. A. Wold argues that those variables which one controls in an experiment should be regarded as “cause variables,” while those whose values one then observes are “effect variables” (1954:165). One should, Wold suggests, regard a functional relationship of the form \( y = f(x_1, \ldots, x_n) \) as causal “if it is theoretically permissible to regard the variables as involved in a fictive controlled experiment with \( x_1, \ldots, x_n \) for cause variables and \( y \) for effect variable” (1954:166). This criterion is appealing, but it is not well worked out. Not all the variables which one controls in a real or fictive experiment exert any causal influence on the outcome. Wold fails to elaborate on how judgments about causality depend on the theories one accepts. Whether it is “theoretically permissible to regard the variables as involved in a fictive controlled experiment” depends on prior judgments concerning which relationships are causal. Unless these prior judgments can be justified by actual experiments, which are unavailable for the theories we have been considering, one has only postponed answering the question of whether a given relationship is causal.

Thinking about “fictive” experiments does help us make our intuitive judgments vivid. One might offer the story that, before prices are calculated, capitalists and workers bargain for shares in the net prod-
uct. Those shares determined, prices then result. Finally people buy what they want. Can one, however, make sense of this producing and bargaining before prices are known? Perhaps production might be determined by tradition, but it is difficult to understand this supposed bargaining for shares in the net product. Sraffa himself does not mention it. How could such bargaining work? It seems quite implausible to believe that Sraffa describes what causes exchange values to be what they are.

To reach this conclusion is not to condemn Sraffa, since in Wold’s strong sense of cause, general equilibrium models are not causal either. The hypothetical story of tâtonnement (see Arrow and Hahn 1971:264) is not much more plausible. Can one conceive of all bargaining occurring before any actual production? Perhaps we need a different analysis of causal explanation in economics (see Hicks 1979).

One way of resisting this conclusion and arguing that general equilibrium models, unlike Sraffa’s work, meet Wold’s vague criterion is question-begging. One might argue that Sraffa provides no causal account of price formation, because economists have already learned from equilibrium theory what the real mechanism of price formation is. Given the acceptance of equilibrium theory it is not “theoretically permissible” to regard Sraffa’s variables “as involved in a fictive controlled experiment.” Consider the following analogous case. If one knows the prices, the rate of profit, and the actual inputs used to produce each kind of commodity, one can calculate how much of each commodity is produced. Can one regard prices, the rate of profit, and the actual inputs used as causes variables in a fictive experiment? From the perspective of any current economic theory, the answer is clearly no. The calculation is not an explanation. In the same way, if one accepts the general account of price determination that equilibrium models provide, Sraffa’s apparatus is at most a calculating device. This criticism is persuasive only if one accepts the neoclassical account of price determination.

C. J. Bliss provides an alternative account of causality in economics which may lie behind Ng’s criticism of Sraffa. Bliss argues that one may speak of x causally determining y if and only if y is an economic variable, x is a “primitive feature” of an equilibrium system, and y is dependent on x; y is dependent on x if a change in x “might necessitate an alteration” in y (1975:34). x is a “primitive feature” if it is “largely outside the domain of theoretical economics as such” (1975:29). The “givens” to an equilibrium system—utility and production functions and the size and distribution of the initial endowment—are primitive features. Although the factors that determine exchange values in Sraffa’s system are exogenous to that system, they are not primitive, his account thus cannot be causal. All Sraffa shows is how to calculate prices, given other information. An accountant’s manual is in this sense an account of how the profit of a firm is determined.

Another way to put this objection is to claim that Sraffa’s apparatus overlooks the fact that in general all variables in an interdependent system must be solved for simultaneously. As thinking through Sraffa’s own system reveals (see ch. 8, §3), factors like technology or distribution cannot simply be taken as given, but must be determined along with exchange values and cannot be regarded as causal determinants of exchange values.

This objection to Sraffa’s work is forceful, but inconclusive. It can be directed against equilibrium theory as well. Although utility functions and production functions are taken simply as givens, as “primitive features” which are neither subject to economic analysis nor dependent on the nature of the economy, everyone recognizes that utility functions are affected by the form production actually takes and that economic factors cause technological innovations. A true simultaneous solution of all the mutually dependent variables would have to include as variables what general equilibrium models take as givens. Sraffa’s sins are of the same kind as equilibrium theorists’s.

It might be argued that such an ad hominem response is not fair, because utility functions, for example, are insensitive to changes in output or prices, while composition of output, a given in Sraffa’s apparatus, is extremely sensitive to changes in exchange value. The extent to which utility functions should be regarded as primitive givens is a matter of controversy; but some important difference between taking utility functions or output as given must be conceded. Sraffa does leave out of his apparatus important economic relations among his givens and between his givens and the values one solves for.

Does such an “error” render Sraffa’s work empty, noncausal, and without explanatory worth? The full answer will have to wait until we consider the contrast between Sraffa’s theoretical strategy and the approach implicit in equilibrium models. It is, of course, silly to deny that one feature (an increase in the wages of steel workers) causes another (a steel price increase) on the grounds that the first is not “primitive.” If we interpret Bliss charitably, we must understand him to be offering an account of causality within the framework of general equilibrium analysis. Even within such a framework, I do not see why one should want to deny that some economic phenomena affect others, merely because both depend on other relatively primitive features. Sraffa has shown that a functional relationship exists between output,
technology, distribution, and prices. I see no reason to deny that this relationship is of some causal significance.

Sraffa’s apparatus has features that limit its scope, the extent to which the relations it identifies can be regarded as causal and the precision with which it can explain. It is at best a limiting case of a scientific theory; but it may turn out to be the best account of exchange values economists have.

2. Sraffa and the Separate Science of Economics

Equilibrium models share a set of assumptions which, when pruned or augmented in various ways, are supposed to enable economists to account for all major economic phenomena. In a sense mainstream economics rests upon a single theory. If we examine the actual theories economists employ, we can readily see that this claim is not entirely accurate. Macroeconomic theories, for example, are not augmentations of the same fundamental model. Yet the ambition remains. Neoclassical economists would like to be able to employ variations on the same fundamental model in accounting for all economic phenomena.

Sraffa cannot share this ambition. The system that he presents cannot be augmented to provide, for example, a model of the role of demand in price determination or of the factors influencing size of firms. Sraffa may have designed his system to serve exclusively critical purposes. He does, after all, subtitle his book, Prelude to a Critique of Economic Theory. Yet it seems to me that, regardless of Sraffa’s intentions, his work may be considered as part of an alternative project for economic theorizing. Such an alternative project employs an utterly different strategy. This difference in strategy accounts, I believe, for the bewilderment many economists feel when they read Sraffa. Just what can he be up to?

Implicit in the manner in which equilibrium models are conceived and deployed is a conception of economics as a separate science. The terminology is John Stuart Mill’s and is, I think, useful. Mill writes:

Notwithstanding the universal consensus of the social phenomena, whereby nothing which takes place in any part of the operations of society is without its share of influence on every other part; . . . it is not the less true that different species of social facts are in the main dependent, immediately and in the first resort, on different kinds of causes; and therefore not only may with advantage, but must, be studied apart (1843, book VI, ch. 9, sec. 3).

Mill is not asserting, trivially, that some social phenomena depend principally on a limited number of causal factors. Rather he is suggesting that a few causal factors are sufficient to account for at least the major features of a whole range of social phenomena. A full statement of Mill’s view is the following:

There is, for example, one large class of social phenomena in which the immediately determining causes are principally those which act through the desire of wealth, and in which the psychological law mainly concerned is the familiar one that a greater gain is preferred to a smaller. I mean, of course, that portion of the phenomena of society which emanates from the industrial or productive operations of mankind. . . . By reasoning from that one law of human nature, and from the principal outward circumstances (whether universal or confined to particular states of society) which operate upon the human mind through that law, we may be enabled to explain and predict this portion of the phenomena of society, so far as they depend on that class of circumstances only, overlooking the influence of any other of the circumstances of society. . . . It makes entire abstraction of every other human passion or motive, except those which may be regarded as perpetually antagonising principles to the desire of wealth, namely, aversion to labor, and desire of the present enjoyment of costly indulgences. (1843, book VI, ch. 9, sec. 3)

Like economists today, Mill conceives of this separate science of economics as unified and reductive. Since a single set of causes is “immediately determining” for “one large class of social phenomena,” economics will be unified. It is not as if a single theory could serve all the explanatory and predictive purposes economists have. Instead a single theory is supposed to account for all the “major” economic phenomena.

Economic theorizing consists not of developing different general theories for different domains in economics, but in adding various assumptions to the basic equilibrium model in order to account for more phenomena. Furthermore, except for details and qualifications, economics is supposed to be complete within its domain. Both Mill and modern theorists believe that no significant explanatory or predictive purposes of economics would be served by fusing economics with any other science. There may be explanations of the fundamental “laws” of equilibrium theory, but those explanations are not part of, and have nothing to contribute to, economics. Since the fundamental “laws” are not to be explained within economics, the discipline is in a sense reductive. In principle one might explain economic phenomena by means of noneconomic laws and specifications of economic initial
conditions. If we ignore such a possible reduction, we can recognize the crucial point: all theoretically central parts of economics can be explained and predicted by equilibrium theory alone.

The general idea Mill espouses is relatively clear. If theorists can isolate the principal causal factors, they can develop economics as an inexact science. Economists will then be able incorrectly to explain and predict the principal phenomena. Neoclassical economists do not seek to make their fundamental theory increasingly exact. What they want in equilibrium theory is a versatile set of lawlike claims which, with qualifications and allowances, they can employ no matter what economic problem they face. In some of these applications economists should be able to test and confirm the resulting applied theory. Otherwise there would be little reason to take seriously the assumptions of the basic equilibrium model. Provided that this condition (and the others mentioned in chapter 7) is satisfied, neoclassical economists can then employ the basic theoretical apparatus as an all around guide to the understanding of economic phenomena.

Neoclassical economics thus has a distinctive theoretical strategy. Because economists have not been able to do better, and because they want to keep the fundamental theory, versatile, economists work with a loose and highly inexact basic set of assumptions. Applying them to provide empirical theories of different sorts of economic phenomena is quite difficult. Numerous revisions and additions are always necessary. Sometimes the problems of augmentation and revision remain unresolved for a long time, as is the case in the theory of capital and interest. Yet the basic assumptions provide guidance and confidence that problems like those concerning capital and interest are solvable— even where there are no immediate prospects of solving them.

This theoretical strategy might easily be assimilated to some of Kuhn's views concerning paradigms (1970) or to Lakatos' views concerning research programs (1970; see also Latsis 1976b). Kuhn says so many ambiguous things about paradigms that one can interpret him as arguing that scientific communities employ paradigms in just the way that I have suggested that economists employ equilibrium theory. I do not believe, however, that reading these views into Kuhn's suggestive but unclear discussion contributes to an accurate appreciation of equilibrium theorizing.

On my interpretation, equilibrium theory should not be regarded as what Lakatos calls a research program. I thus believe that the interest of a number of economists in Lakatos' work (see Latsis 1976b) is misplaced. According to Lakatos, in a research program one has a series of related theories. Later theories in the series have more content than earlier ones (unless the research program is degenerating) and are more exact. Consider Lakatos' discussion of Bohr's theory of the atom (1970:140-54). The research program is not identical to any of the specific theories; it is defined by the related series. What relates the theories in the series is a certain 'hard core' of theoretical assumptions and a general methodological approach. Although this characterization of research programs overlaps to some extent with my comments concerning the development of equilibrium models, it differs fundamentally. The goal of equilibrium theorists is not an increasingly precise general economic theory. Equilibrium theorists do not devote their major efforts toward developing better general theories. The hard core is the theory. The task is mainly to apply it. One does not find a series of improving theories like the successive versions of Bohr's model of the atom. Equilibrium theory, apart from a few details, has been fixed for nearly a century. The efforts of economists are devoted mainly to its application and its mathematical elaboration. In neoclassical economics, we find a (highly inexact) theory variously employed, not a 'hard core' to a series of theories with increasing content.1

Sraffa's work implicitly defines an economic methodology and a conception of the relationship between economics and other social theories unlike the neoclassical approach. Sraffa himself has not written on these methodological questions. I have no specific grounds upon which to assert that he would agree with my comments, except that the conception of economics and its relationship to other social sciences sketched in this section seems to me not only consistent with Sraffa's work, but to make that work more intelligible.

Sraffa does not take economics to be a separate science with its own distinctive laws and causes. It is to be distinguished roughly from other social studies by its concern with production, distribution, exchange and consumption of commodities and services, and with social phenomena closely connected with these. Its laws are not necessarily individualistic or psychological, although some may be. In seeking to explain given economic phenomena one should draw freely upon the results of other social studies. No special set of causal factors is predominantly responsible for all major economic phenomena. In each given problem situation, the economist must isolate the major causal factors by empirical investigation and theoretical ingenuity. Occasionally these may coincide with the factors equilibrium theorists pick out as primitives. Often there will be no such coincidence. Economists should seek many different (but mutually consistent) explanations at

1 Utility theory, for example, has had a decreasing content.
different levels of detail. Examples of such work are macroeconomic theories and some of Marxian economics.

Sraffa does not base his account of exchange values on factors that can be taken as givens for all economic analyses. Nor is his work either individualistic or psychologistic. In fact, the motivational forces and the individual actions that are responsible for the equalization of the rate of profit and the congruence of market prices and exchange values are concealed. These elements are undeniably important, but Sraffa apparently believes that they can be relegated to the background in his account of exchange values. If one's conception of the enterprise of theoretical economics is that of an equilibrium theorist, these aspects of Sraffa's work will seem bizarre. If, on the other hand, one envisions economic theory proceeding in a piecemeal way, Sraffa's efforts are completely intelligible.

Is there anything wrong with piecemeal theorizing? Are there any methodological grounds upon which to dismiss Sraffa's work? I think not. I can see no justification for the requirement that explanation in economics be unitary. Equilibrium theorists may object that Sraffa's work senselessly chops up an interdependent system (of which neoclassical economists possess the outlines of a unified theory). Defenders of Sraffa can reply that Sraffa disputes that theory and repudiates the methodological ideal of a separate science of economics. Of course, for all I have said, it may be that economists can learn nothing at all from further piecemeal theorizing compatible with Sraffa's work. I see, however, no general methodological reason to believe that such is the case.

3. What Sraffa Has To Offer

Sraffa's work thus withstands, I believe, philosophical criticism. It is not yet clear, however, what, if anything, one can learn from it. Sraffa's work does not support the strong conclusions (discussed in chapter 8) that Dobb and others have drawn. Is it anything more than mathematical analysis of functional relations between certain variables and parameters? Apart from its contribution to the Cambridge criticisms of aggregate simplified capital theory, does Sraffa's work have any general interest and importance?

It might be argued that no account of exchange values in terms of physical costs is possible. Not only are values dependent on distribution, but recall Samuelson's objection (1959:21f) mentioned in chapter 1 (see also Findlay 1974:6). Both Ricardo's and Marx's theories of rents make the margin of cultivation and thus the labor necessary to produce agricultural products depend on demand for agricultural products. Samuelson argues that for this reason price cannot be determined by the cost of production or by the labor inputs.

Is the dependence of labor values on demand that Samuelson points out a refutation of the labor theory of value? Why can one not claim that exchange values are determined by labor value (or physical cost cum distribution) but that labor value or physical cost are themselves affected by demand? Robinson appears to make just such a claim:

When we are provided with a set of technical equations for production and a real wage rate which is uniform throughout the economy, there is no room for demand equations in the determination of equilibrium prices. (When we take down our protective fence, and allow that changes in distribution affect the composition of output, we shall need a fresh set of equations, but that is quite another matter.) (1961:57)

The passage is not entirely clear. Robinson may be making the mistaken claim that demand is irrelevant to exchange value. On the other hand, she may be suggesting that we can accept a physical cost cum distribution theory of pricing despite the dependence of "physical costs" on demand. Garegnani expresses this point more explicitly. He relates the views of the classical economists (as he interprets them) to his own views as follows:

Since they regarded these "necessaries" as determined by social as much as physiological conditions, we may see them as recognizing that distribution is governed by social forces, the investigation of which falls largely outside the domain of the pure theory of value. The proper object of value theory was seen to be the study of the relations between the wage, the rate of profits and the system of relative prices. . . .

Thus, the separation of the pure theory of value from the study of the circumstances governing changes in the outputs of commodities does not seem to meet any essential difficulty. On the contrary, it may open the way for a more satisfactory treatment of the relations between outputs and the technical conditions of production. Moreover, by freeing the theory of value from the assumption of consumers' tastes given from outside the economic system, this separation may favor a better understanding of consumption and its dependence on the rest of the system. (1970:427–28)

Garegnani would thus like to adopt a physical cost cum distribution theory of exchange value while conceding that physical costs are not
primitives, but are sensitive to other economic factors, including demand. Sraffa's work makes such a position possible. Assuming that Sraffa's system can satisfy the conditions of chapter 7, it shows how a physical cost cum distribution explanation of exchange values is possible. Sraffa thus shows how the theory of distribution might be separated from the theory of value. He shows that one need not believe that the rate of profit is established through exchanges. Neither Sraffa nor his followers can explain in detail why prices are what they are. The economies they study are, after all, not in stationary equilibrium. Even if there were stationary equilibria, economists do not now and will never possess enough data for such detailed explanations. Recognizing this fact should not, however, blind us to the value of explanations in principle of the sort Sraffa helps provide.

Sraffa thus gives a new shape to theorizing about competitive economies. If one regards the theory of exchange value as the fundamental economic theory, then Sraffa's work is certainly inadequate. His account of value cannot serve as a foundation from which theories of demand, technology, distribution, or whatever could be deduced or developed, for the simple reason that his account of value takes so much as given. Instead of being basic, the theory of value turns out to be derivative, perhaps even superficial. Sraffa is saying in effect, "Look, this is all there is to exchange values. The neoclassical (and classical) preoccupation with exchange values is a mistake. Now let's get down to the real problems." It is in this way that I would reconstruct Garegnani's comments quoted above. If Garegnani is correct that Sraffa "opens the way for a more satisfactory treatment of the relations between outputs and the technical conditions of production" or favors, "a better understanding of consumption and its dependence on the rest of the system," Sraffa does so by minimizing the role of the theory of value. Such a step is not without dangers. Economists may be lost in a maze of special theories without any adequate means of knowing whether they are even consistent.

Such a view of the theory of value is, of course, in sharp contrast to the general vision of economic theory that has dominated mainstream economic theorizing. Whether such a demotion of the theory of value is tenable depends on whether one needs a unified equilibrium theory to theorize adequately about population growth, technological change, demand, the nature of firms or of financial institutions, and so forth. Some general account of how commodities and thus the various activities of people are related is a crucial part of economic theorizing, but must that account be a fundamental and unified theory of exchange? Sraffa answers negatively.

4. Marx and Sraffa

The relationship between Sraffa's work and Marx's work is controversial. Some Marxian economists have applied Marx's criticisms of Ricardo in Theories of Surplus Value to Sraffa's work. In Volume 2 of Theories of Surplus Value Marx repeatedly chastises Ricardo for confusing value and average price (by which Marx means exchange value) and for not theorizing in terms of labor values (1968, esp. p. 106). Since both Ricardo and Marx recognized that in equilibrium goods will not exchange according to the ratio of the labor embodied in them, what is the disagreement? Perhaps it is only some Hegelian baggage that Marx was never able to discard. Marx insists on the distinction between value and average price because the former is "prior" and the latter must be "derived" from it. Consider a passage like the following: Ricardo "does not realise at all that in order to create the general rate of profit value must first be transformed into cost-prices." (1968:434; Marx's emphasis). Marx's criticism seems obscure and, in a pejorative sense, "philosophical."

There are two ways in which this criticism might be clarified. Medio (1972:320–21, 326) and Rowthorn (1974:77) argue that by focusing on exchange values or average prices, Sraffa diverts our attention from the central task of analyzing the formation of the surplus under capitalism. They object to Sraffa's abstraction from the social relations involved in capitalism. Labor values, unlike Sraffa's prices, supposedly provide a key to understanding capitalist social relations. Second, one might interpret Marx as criticizing Ricardo for using the wrong theoretical construct for analyzing production and distribution under capitalism. Some reference to prices of production or exchange values is unavoidable. Marxian values must be related to prices. Yet for basic theoretical investigations even of purely economic aspects of capitalist production and distribution, values, not prices of production are the more fruitful theoretical terms.2

Rowthorn (1974:81) and Roosevelt (1975, esp. pp. 8, 19) have offered one further related objection to Sraffa's work. They argue that Sraffa isolates production from distribution in an artificial and misleading way. He focuses on the effects of different distributions of income, as if the actual relations of production did not already determine the distribution of income. Sraffa's work might in this way be linked to "vulgar" socialists whose principal complaint against capitalism concerns inequalities in distribution.

2 I am indebted to Mr. Jake Shearer for this interpretation of Marx.
One can only invest in productive capital goods if one owns "capital," and most people own little or no capital. This fact itself needs explaining: how could capitalism get started? According to Marx, the key event was the creation by expropriation of the free laborer. The greater productivity of capitalist enterprise perpetuates and expands the system, while the greater productivity of larger scale enterprise leads to a concentration of capitals.

In order for the ownership of capital to become exclusive and remain so, society must have special features. First, it is crucial that the legal system permit private ownership and free transfer of wealth. It must be legal to employ wage labor and to earn a profit. Second there must either be no state administration of wages, prices, or profits, or that administration must directly insure the existence of profits. In a competitive economy, the existence of profits is only threatened when, as a result of capital accumulation, labor becomes scarce enough that wages rise sharply. Scarcity of labor is not generally a problem, since a rising wage induces investment in labor-saving technology, which insures the existence of a pool of the unemployed. The actual theory is developed by Marx in great sociological detail. Moreover, he calls attention to the role of ideological factors in social reproduction. The peculiarities of profits as a particular form of the social surplus are discussed at considerable length.

Although I have stated some of the major features of Marx's theory of profits without mentioning the labor theory of value, there are some value theories with which Marx's theory of profits cannot be combined. Neoclassical price theory, for example, leaves no room for any story concerning class struggle over the surplus. Sraffa's apparatus, in my view, provides Marx with the theory of exchange value that he needs. It shows how the results of production manage to get sold with an equalized rate of profit. Whether it is legitimate to discuss the basic features of production, distribution, and accumulation, as Marx does in Volume I of Capital, in terms of labor values is a separate question, which I am leaving open.

5. Normative Factors

As other writers on capital theory have noted, normative issues lurk behind analytic ones (Bliss 1975:345–52; Blaug 1975:75–78). To analyze what is lurking and to begin to assess its significance is a difficult and subtle undertaking. I shall offer only some brief comments, sketch-
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. Unfortunately, these models and theorems do not enable one to explain real phenomena of capital and interest; and I know of no theory undiscussed 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 defeat 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 imp-