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Essays on Piero Sraffa
A Review Article*

Chidem Kurdas

I. *Overview*

Among great economists, Piero Sraffa must rank as the most enigmatic. The tantalizing tidbits of writing he left have to be understood in the context of several schools of thought. It is well known that he had a dual purpose. The 1960 *Production of Commodities by Means of Commodities*, according to its subtitle, provides the basis of a critique of marginalist economic theory. His other goal was to revive and develop the "submerged and forgotten" approach of the old classical economists. Furthermore, some economists have attempted to ground Keynesian or Kaleckian models of output determination on the Sraffa price system. There is, finally, the issue of how the theoretical propositions of Sraffa and his followers relate to the Post-Keynesian growth models of Joan Robinson and Nicholas Kaldor, and to issues of accumulation and technical change in general.

The book under review consists of the proceedings of a conference held in 1985 to commemorate the 25th anniversary of *Production of Commodities by Means of Commodities*. The essays cover the three major areas just described: criticism of marginalist economics, reconstruction of the classical school, and the study of output, demand, and accumulation according to classical-Sraffian concepts.[1] This rich collection of essays provides an opportunity to assess both the positive and the critical contributions of Sraffian analysis.

The essays document diverse judgements on Sraffa's achievement and the current status of the classical reconstruction project. For Sraffa's

* Review of Krishna Bharadwaj and Bertram Schefold (eds.) *Essays on Piero Sraffa: Critical Perspectives on the Revival of Classical Theory*, London, Unwin Hyman, 1990. The page numbers and names in parentheses in the text refer to this publication.

[1] The contents of the book are discussed below according to this tripartite classification of the issues.

followers the reconstruction of the classical tradition provides an alternative price theory that is superior to supply and demand models, both in terms of logical coherence and potential for understanding economic reality (Bharadwaj, Garegnani). It is suggested that Sraffa's analysis may also be "an adequate starting point of an investigation of accumulation and effective demand" (Kurz: 397).

Other authors give discouraging answers to the question: What can one do with classical-Sraffian economics that one cannot do without it? Post-Keynesian Hyman Minsky replies: Nothing that matters in understanding the late 20th century industrial economy. On the marginalist side, Paul Samuelson and John Hicks argue that one cannot do much with Sraffa's propositions that one could not do better with some marginalist model or other. It is not surprising that Hicks and Samuelson take this position, but Minsky's verdict is troublesome. One, because he in generally sympathetic to the project of providing an alternative to marginalism. And two, because his voice is the only one in the collection that sounds unmistakably contemporary. The reader will have the opportunity, in the following survey of the essays, to judge whether Minsky is too harsh in his judgement.

The individual papers are discussed in the sections below. Here it should be noted that the editing of the volume leaves much to be desired. The editors have done the collection a disservice by deciding not to provide an introduction. Without a guide, the nineteen papers and scores of commentaries, spanning a wide spectrum of topics and approaches, are accessible only to those who already know the issues and debates. The book smacks of preaching to the converted. An introduction could have also brought some cohesion to the disparate essays. Furhermore, the haphazard organization of the papers gives an impression of confusion. There are two untitled sections and two "round tables". The first untitled section appears to be on price and distribution theory, consisting mostly of papers that contrast the classical approach with the marginalist. Two of the three papers in the second untitled section are on the relationship between Keynesian theory and Sraffian economics. But the other paper in this section, by Samuelson, is on price theory and belongs to the first section. The poor reader is expected to slog through 500 plus pages of text unassisted, with no idea as to how the articles connect to one another or which one makes an appropriate beginning and which, if any, provides a summing up.

II. *Criticism: Sraffa Against Marginalist Economics*

What features of the Sraffa system distinguish it from marginalist models? In the neoclassical scheme, factor endowments determine income distribution, while Sraffa takes a distributional variable, either the wage

rate or the profit rate, as given, so that factor endowments do not play a determining role (Garegnani: 112). This key difference follows directly from Sraffa's criticism of the concept of capital endowment in neoclassical distribution theory, and underlies the reconstruction of classical economics.

In view of the importance of the criticism, it is worth repeating the main point. The supply and demand explanation of distribution, Sraffa and others have argued, is logically flawed for the following reason. The continuous inverse relationship between the demand for the factor capital and its price does not hold. The decreasing slope of the demand for capital function expresses the substitution between factors in production. By showing that the factor of production capital cannot be specified independently of income distribution, the critics undercut this determining mechanism. The demand for capital is different at two rates of profit, not just because of factor substitution as supposed to be the case along the demand for capital schedule, but because the specification of quantities of capital changes with the profit rate. This means that, in the absence of restrictive assumptions, the inverse relationship between price of capital and quantity of capital is disrupted by re-switching and reverse capital deepening. These phenomena cause no problem for Sraffa's classical system because there income distribution is not determined by supply and demand relationships.

In the book under review, Paul Samuelson once more accepts that "it is, in general, not possible to associate lower interest rates with... 'more mechanised'... methods of production..." (271) Given this admission, it is surprising that some three decades after it was levied, the Cambridge critique of marginalist distribution theory has had such little impact. If anything, during these decades an immense literature founded on marginalist distribution theory has blossomed. Some neoclassical economists who were part of the debate profess not to understand what it was about. To Robert Solow, the Cambridge controversy "seems ... to have been a waste of time, a playing-out of ideological games in the language of analytical economics".[2] The occasion for Solow's remark was the lecture he gave when he received the Nobel prize in economics. As he notes, his theoretical contribution to growth economics and the growth accounting he initiated attracted many economists and led to a whole new literature. The measurement of factor contributions to growth, with technical change coming out as residual, is based squarely on the marginal productivity theory and aggregate production function Sraffa and his followers thought they had demolished. In order to measure factor contributions, factor payments are assumed to be equal to marginal products. Why does the logical problem acknowledged by Samuelson have no impact on what economists actually do?

[2] ROBERT M. SOLOW, "Growth Theory and After" in *American Economic Review*, June 1988, p. 309.

Those who use neoclassical growth theory do not seem to realize that the critique was about distribution. Thus Angus Madison sees the issue as purely concerned with the measurability of capital.[3] Madison is a wide-ranging economic historian with no particular commitment to marginalist ways of thinking. Why, then, do he and others ignore the supposedly deadly Cambridge critique of this way of thinking? For one thing, the abstract logical point about the demand for capital function does not show up empirically. Even if it did, the lack of an alternative way of measuring technical change would be adequate excuse to go on using neoclassical distribution theory. Furthermore, as Burmeister points out in a different context, "There is no single neoclassical model; each economist working in the neoclassical tradition introduces special assumptions to address specific issues". (204) Marginalism has more heads than the proverbial dragon, and is a lot more difficult to slay. Solow told the Nobel prize audience: "I have some faith that the ideas of 'neoclassical' growth theory are viable just because they have attracted a research community...". It is a good guess that the research community will remain attracted, in the absence of an alternative theory capable of empirical application.

The *Production of Commodities by Means of Commodities* is about relative prices and the relationship between these and income distribution. Garegnani has argued that these relationships constitute the "core" of classical economics. In this framework technology is specified in the form of production coefficients. Consumer tastes underlie the composition of output. These, in addition to the level of output and a distributional variable (the wage rate or the profit rate), constitute the data of the system. On the basis of this data, the equations determine relative prices and the other distributional variable. In contrast, marginalist theory determines income distribution and the level and composition of output, as well as relative prices. To do so it takes as its data factor endowments, consumer preferences, and technology. The last two items are also part of the classical data.

The endogenous variables determined in the classical core by the price equations are subject to "general quantitative relations of sufficiently definitive form" to constitute a generally applicable model. Other relationships, asserts Garegani, are too complex and variable to be represented by a quantitative model (123-4). Among these are interactions between the exogenous variables, such as the relationship between technology and the composition of output. These are to be studied separately from the core price relations. This distinction between two kinds of relationships means classical analysis breaks into successive logical stages. The level and composition of output is determined prior to relative prices,

[3] ANGUS MADDISON. "Growth and Slowdown in Advanced Capitalist Economies: Techniques of Quantitative Assessment", in *Journal of Economic Literature*, June 1987, p. 677.

and within a different framework, perhaps one that is more time and place specific. In short, quantities and income distribution are studied separately, and are not necessarily subject to formal relations. This contrasts with the simultaneous determination of prices and quantities by supply and demand relationships.

This structure of the classical core has led to the marginalist charge that the classical-Sraffa model is incomplete or based on assumptions that make it a special case of neoclassical general equilibrium. Garegnani replies that such a view reflects "an unwitting tendency to take marginal theory as the only conceivable explanation of the facts" (p. 133). It is the achievement of Sraffa and his followers to remove the marginalist blinkers and show that an alternative determination of relative prices is possible. But, to get a full measure of classical economics, one has to go beyond the price equations. Now that Sraffa has clarified the core, the classical research program is to forge ahead, building around these relationships.

III. *Continuity: Sraffa and the Classics*

Neoclassical theory rests on *a priori* quantity-price relations in the form of well-behaved supply and demand functions. By contrast, Sraffa's followers argue, classical theory does not require such relationships. Instead, the open structure allows historical and institutional factors to enter, specifically in the analysis of income distribution, output, technology, and investment. What strikes neoclassical economists as a sign of incompleteness is for Bharadwaj and Garegnani an advantage, conferring greater flexilibility and openness, as opposed to the sterile reduction by marginalist theory of all economic phenomena to supply and demand relations. To illustrate the open methodology, Bharadwaj refers to Smith, Marx, and Keynes, although the latter's relevance to a Sraffian point of view is questioned by other writers in this collection.

For Bharadwaj, Sylos-Labini, and others, Sraffa's formulation of classical value theory opens the way to historically specific economic theorizing. For those in the opposite camp, it is an example of a "deliberately static" model, since "A model in which outputs are unchanging over time, from period to period, can only be a model of a stationary state" (Hicks: 100). Furthermore, Hicks sees Sraffa's economics as a stationary version of one of his own models in *Capital and Growth*. For Bharadwaj this is a misinterpretation: "the fact that the propositions in the book do not depend upon change does not imply the assertion that no changes would or could follow".

The word "change" has two meanings in conventional economics, represented by movements along a given schedule versus shifts in the schedules. The second kind is exogenous to the model in that these changes

in the data of the model are beyond its explanatory power. Given a change in technology or tastes, a marginalist model will trace out the effect of the change on prices and quantities, according to the supply and demand relationships. This comparative static analysis is the bread and butter of textbook economics. But changes in the exogenous variables may be qualitative rather than quantitative. Such changes may take place in what Joan Robinson called historical time, beyond the Marshallian long-run. This, of course, is precisely what puts such changes otuside the realm of neoclassical analysis. Changes in prices and quantities are quantitative and take place along abstract, reversible schedules, notwithstanding Marshall's reservations about reversibility along the supply schedule. These changes are from one long period to another, with temporary positions in between. Historical time is not involved.

In the Classical-Sraffa model the scope of comparative statics is severely limited. The effect of a change in, say, production coefficients on relative prices can be calculated by plugging in the new coefficients. In the absence of determinate price-quantity relations, the model says nothing about the effect of this on quantities. This is what Samuelson, Hicks and Burmeister perceive as a limitation of the Sraffa model. In contrast, Bharadwaj and Garegnani reply that the impact of an exogenous change, say in technology, is not really captured by the *a priori* determination of supply and demand models. The open classical approach, they point out, can cover the full ramifications of such a change, taking account of its specific historical context.

The "openness" of the Sraffa model requires some elaboration. If the criterion of a model being open in some respect is the exogeneity of those variables, then neoclassical models are open with regard to technology, tastes, and initial endowments. On this basis a neoclassical partisan can claim that there is nothing to stop historical studies on technology or tastes from being appended to a neoclassical construction. In actuality this does not happen, most likely because economists who are trained in supply and demand functions are likely to use supply and demand functions in their work, and not ask questions that cannot be answered with this apparatus. The evolution of technology and tastes is uncharted territory that most economists do not enter. Sraffian equations, on their own, give no direction as to the relationships between technical change and output, output and consumer tastes, or tastes and income distribution. The Sraffa model is as "open" in these respects as neoclassical models are about the relationship between technology and tastes. Judging from the neoclassical example, such openess does not encourage exploration of the exogenous areas. The defense of Sraffa's model rests on the rich heritage of the classics, who did study such topics. In other words, the proof that the "open" classical structure is superior depends on the insights facilitated by this structure.

The classics are different things to different people. If in Sraffa there

is a paucity of directives on a variety of subjects, there is a bewildering array of directions in the classics. Ricardo did not agree with Malthus, and Marx did not agree with Smith. As for John Stuart Mill, is he to be considered a full-fledged classic or a watershed on the way to marginalism? To make things more complicated, the classics themselves provided building blocks for the neoclassical edifice. As Bharadwaj puts it, "the classical theory of rent became the fountainhead of most basic marginalist ideas" (56). She argues that this was an improper generalization from agriculture to other sectors of the economy. Another example is Ricardo's comparative advantage theory, long the centrepiece of mainstream thinking on international trade. Again, one can argue that it has been subverted by the marginalist interpretation. Be that as it may, these examples show that marginalism and the classics share a border. The common border means endless haggles over who and what is properly "classical" in Sraffa's sense of the word. Thus Hollander's casting of Ricardo as a proto-marginalist lead to a long debate, and the furor over Ricardo's corn model is not yet over. To top it off, a "New Classical Macroeconomics" has come into being in recent years. The school of thought that has assumed this title has nothing to do with what Sraffa and his followers present as the classics.[4] The project of building a new economics on classical foundations is handicapped by the ambiguities about the basic content and identity of classical writings. A large part of the Sraffian project to date has consisted of attempts to clarify these ambiguities.

One does not have to go far to document the diversity of classical authors. In the very first paper of the book, Sylos Labini analyses the difference between Smith and Ricardo with regard to dynamic returns to scale. Smith identifies dynamic increasing returns as the primary condition governing the wealth of nations. Ricardo takes diminishing returns to land as the dominant historical motif in his analysis of income distribution and growth. It may be argued that Smith and Ricardo were using the same method of analysis, and the difference in their assumptions should not obscure the more fundamental similarities between them that constitute classical economics. Sylos Labini also elaborates and extends Sraffa's 1925 and '26 critique of the Marshallian returns to scale. He highlights the sharp distinction between the two classical authors' dynamic laws of return and Marshall's re-interpretation of these in the static marginalist framework.

Smith and Ricardo can be described as working on different parts of the classical project. Ricardo primarily within the core, Smith mostly outside it, with the consequent differences in emphasis. Of course, Smith's increasing returns and Ricardo's diminishing returns lead to divergent

[4] For a description of what "New Classical Macroeconomics" is, see *Modern Business Cycle Theory*, edited by Robert J. Barro. (Cambridge: Harvard University Press, 1989), in particular the introductory essay by Barro.

growth scenarios. Sylos Labini, for his part, favors the Smithian story as being more relevant for the 20th century capitalist growth experience.

Sylos Labini's paper is a persuasive and magisterial exercise in the history of economic thought. But it has a discomforting implication for the resurrection of the classics in the 20th century. We know precisely what relationships and results follow from the marginalist assumptions — after all, these have been elaborated and formalized for well over a century. Outside the core, as defined by Garegnani, the word "classical" implies no such set of generally agreed-upon propositions. It appears that in appealing to the classics, one has to pick and choose among the many ideas they put forth — just as Sylos Labini chooses increasing returns.

Samuelson's evaluation of Sraffa as an economist also illustrates the variety of interpretations the classics give rise to. Samuelson focuses almost exclusively on what Sraffa's analytical devices do for Marx. (Or rather, he shows what these do not do for Marxian economics). Thus, Samuelson shows the standard commodity is useless in a defense of the labor theory of value. Sraffa's demonstration that lower interest rates are not necessarily associated with more mechanised methods of production is 'as fatal to the neoclassical parable as to the notion of recognizable shifts in the organic "composition of capital."' (271) In effect, we sink, but you closet (or out-in-the-open) marxists sink with us. The commentators on Samuelson's paper exhibit no particular wish to defend the labor theory of value or Marx's rising organic composition of capital argument. The debate boils down to what exactly the auxilary device, the standard commodity, and Sraffa's distinction between basic versus nonbasic commodities, are useful for. As Schefold points out, "the real issue concerns the explanatory power of the classical and the neoclassical theories, each taken as a whole, of which the explanation of long-run prices is only a particular aspect." (316) But it seems, from the articles in this volume, that there is a prior issue, namely the definition and limits of "classical theory". The rising organic composition of capital argument is presumably not a necessary part of this theory. (If anybody thinks it is, Samuelson's point that the capital theoretic criticism applies to it is surely on the mark). But according to what criterion is this particular bit excluded from what Bharadwaj describes as the classical-Marxian edifice? Samuelson, with his polemicist instinct, fastens on those precise bits that come out badly in terms of logical cogency and current relevance. In the absence of a consistent and agreed-upon set of classical propositions, he is within his scholarly rights to do so.

In choosing among classical propositions, an obious criterion is current relevance. But this begs the question; What are the salient features of a late 20th century post-industrial economy? In his incisive reply to Samuelson, Schefold writes:

The question is what (the classics') conceptual tools can contribute to

the analysis of a modern world that, it is true, has changed a great deal but that quite obviously is not that of Walrasian equilibrium either (316).

This is another issue in applying the historical-theoretical approach of the classics: the latest example of the genre is well over a century old. The insights on income distribution and accumulation in the 18th and 19th centuries may have been brilliant. What relevance do these have for the 1990s? The applied works in the volume, discussed in section V below, throw some light on this issue.

IV. *Ambiguity: Sraffa versus Keynes*

We have so far focused on Sraffa's reconstruction of classical price theory and the mostly theoretical literature that has resulted from it. Several articles address the question of how this literature relates to Keynesian and Post-Keynesian economics. Asimakopolos and Minsky represent what may be called the North American Post-Keynesian view. Both see an unbridgeable chasm between the economics of Keynes and that of Sraffa. Asimakopolos considers the basic difference to be one of the teatment of time: "Sraffa does not deal with dynamic processes of adjustment". He emphasizes the key role investment plays in Keynes's analysis and the unreliability of the factors that underlie investment, factors "influenced by our views of the future about which we know so little", as Keynes put it. For Post-Keynesians like Shackle and Asimakopolos, expectations have a subjective element, rather than being fully determined by objective variables.

Hyman Minsky, likewise, bases his assessment of the two schools of thought on Keynes's treatment of investment spending. He highlights financial and monetary factors. Existing financial commitments and due payments on debt shape the ability to finance new investment, and interact with expectations of future returns, causing fluctuations in investment. Thus an understanding of the dynamics of accumulation requires the consideration of money and banking as a central force, and a theory with this ambition has to include these topics at its very inception. The absence of such considerations from the *Production of Commodities by Means of Commodities* leads Minsky to conclude that Sraffian economics is irrelevant to the understanding of modern capitalist economies. (It is also irrelevant to Keynesian economics, but this charge pales beside the more serious one of general irrelevence to the world).

Garegnani on the Sraffian side, and Nell taking an intermediate position, reply that the long-run problem of how capacity adjusts to demand is part of the theory of accumulation rather than the study of business cycles. Garegnani suggests that aggregate demand controls the speed of growth, not just the cyclical underutilization of productive capacity described in standard Keynesian analysis. For Nell, the determination of relative prices

is part of the problem of recouping investment over time, and should be studied within the theory of growth. Nell implies that the Sraffa core facilitates the synthesis of topics, like growth and output determination, that until now have been studied separately.

In practice, Nell's own attempt to come to grips with transformational growth shows that Sraffa's writings provide little, if any, positive help in such a project.[5] What the price equations do is to clear the way of supply and demand analysis. There is little, in the theoretical literature that follows Sraffa, of the transformations that carry growth onward, nothing of the factors that underlie business decisions in this process, nothing of the role banks and financial structures play in it. But thanks to Sraffa's reconstruction of classical price theory, those, like Nell, working on these topics can dispense with the supply and demand explanation of relative prices. Instead, they refer to the Sraffian price equations.

Thus Sraffa provides a bare-bones frame of reference, but no positive guide as to how to approach any of the substantive issues involved in the study of growth or monetary and fiscal factors. By contrast neoclassical economics does guide research on a wide variety of topics, ranging from political decision-making to the demand for education. So much so, that it has invaded other disciplines.

There is also the relationship between Post-Keynesian growth models and Sraffa's classical reconstruction. In the 1950s Kaldor argued that the "Keynesian technique" of taking investment as exogenous could be alternatively applied to the determination of long-run income distribution, if the level of output is taken as given, or the determination of the short-term variables, level of output and employment, if distribution is taken as given.[6] The Keynesian growth models constructed by Kaldor and Robinson centered on the following relationship between capital accumulation and the profit rate:

$$I/K = s_c . P/K$$

Taking capitalists' savings propensity, s_c, as a stable fraction of profits, Kaldor and Robinson argued that the price level varies directly with the rate of investment, I/K. On the assumption that the money wage is relatively sticky, the rate of profit, P/K, is (within limits) a function of the investment rate.

Recently this model of accumulation and income distribution has been criticized on the grounds that capitalist economies routinely carry sufficient

[5] See NELL's *Prosperity and Public Spending. Transformational Growth and the Role of Government* (Winchester, Mass.: Allen and Unwin, 1988) and "Transformational Growth and Stagnation" in *The Imperiled Economy* R. CHERRY et al. (eds.), volume 1 (New York: Union for Radical Political Economics, 1987).

[6] "Model of Distribution" by NICHOLAS KALDOR in *Growth Economics*, A. SEN (ed.) (Harmonsworth: Penguin, 1970).

excess capacity to accomodate fluctuations in the level of investment with changes in output.[7] If this is so, there no need for the long-run distribution of income to change in response to changes in the rate of accumulation. In his article Ciccone develops this point, persuasively arguing that average capacity utilization can rise above normal utilization, and thus higher than expected levels of aggregate demand can be satisfied with higher levels of output. Vianello had pointed out that if the rise in investment is large enough to cause supply bottlenecks, the real wage can fall temporarily. On the basis of both of these arguments, there is no reason for variations in autonomous demand to have an impact on the long-term profit rate.

This implies that the only relationship between the level of investment and the level of income is through the multiplier and goes in one direction, from investment to income. If the level of output exerts an influence on investment,that is, if the ratio I/K has a significance apart from the multiplier, then the distributional mechanism is the only way a higher rate of growth of capital stock can be accommodated, since in this case increases in the degree of utilization of capacity and in output only lead to further rises in the level of investment. To put it in different terms, if there is a long-run rate of investment, then it will affect the normal rate of profit. But I know of no argument to show that the share of investment in national income has any significance apart from the output multiplier. Therefore the Garegnani-Vianello-Ciccone argument sounds plausible: economies adjust to cyclical variations in the level of investment through the multiplier.

The paper by Kurz points to various logical problems in the Cambridge growth model. But Kurz also objects to the Garegnani-Vianello-Ciccone argument on the grounds that a higher rate of accumulation is likely to run into supply bottlenecks and lead to price increases. He analyses the concept of normal capacity utilization as part of the choice of technique problem. The way a firm meets excess demand is a matter of optimal choice among alternatives such as an extra shift or increasing intensity of work. As noted above, Vianello agrees that temporary reductions in the wage rate are possible in response to a higher rate of investment. In effect Kurz seems to be closer to the Garegnani-Vianello-Ciccone position than the Cambridge equation. Long-run income distribution, he suggests, is determined by the bargaining power of different social groups and the technological conditions that accumulation itself is changing over time. Steindl heartily assents to this suggestion, and favors a shift of research to the study of such dynamic factors. It is hard to disagree with that. We will now turn to the work of those valiant researchers toiling in the ill-charted terrain outside the theoretical core.

[7] This criticism is presented in a formal model by FERNANDO VIANELLO in "The Pace of Accumulation", *Political Economy, Studies in the Surplus Approach*, 1985. See also GAREGNANI'S "Some Notes for an Analysis of Accumulation" in *Beyond the Steady State: A Revival of Growth Theory* JOSEPH HALAVI, DAVID LAIBMAN, and EDWARD NELL (eds.) (London: Macmillan, 1991, forthcoming).

V. *The Makings of a New Economics*

Several papers in the volume represent attempts to apply the Classical-Sraffian approach. After the review in section III, it should be clear that the proof of the Sraffian pudding is in the success of such attempts. The following studies are discussed in this section. In a rare sally outside the core, Garegnani points to institutions and conventions as the basis of the wage rate. Pivetti argues that monetary authorities, by fixing the interest rate, exert a strong influence on the profit rate. Schefold makes an ambitious try at understanding the evolution of consumer preferences. Levine looks for a classical theory of the firm. Ros draws implications for international trade. Roncaglia surveys possible policy applications.

To start with, there are two suggestions as to the distributional variable taken as given in the core. Garegnani argues that upper and lower limits to the real wage are set by the conventions and institutions of each society. Indeed, such social bounds are required "in order to ensure the maintenance over time of conditions for a sufficiently orderly working of the economy and society" (120).

In recent years there has been research on Sraffa's brief suggestion that the rate of profit is "susceptible of being determined from outside the system of production, in particular by the level of the money rates of interest".[8] In his paper Pivetti proposes the following mechanism as the link between the long-term rate of interest and the profit rate. The interest rate is a determinant of production costs, together with money wages and production coefficients. Prices are tied to normal costs through competition. So, a lasting change in interest rates causes a change in the same direction in the price level relative to the wage rate. For example, a persistent rise in interest rates leads to a higher profit rate and lower real wage. As discussed in section IV above, post-Keynesian growth models also postulate a direct relationship between the price level and the rate of profit. But the mechanism is different: Kaldor and Robinson theorized that the price level depends primarily on the investment spending component of aggregate demand, not on interest rates.

There is more to the story than the above capsule description. There is the issue of the excess of profit over interest, which, following the classics. Pivetti attributes to the "risk and trouble" of investment. This he considers an autonomous component, added on to the interest rate to determine the rate of profit. No analysis is offered as to what determines the remuneration for "risk and trouble". In presenting his argument Pivetti supposes this remuneration to be stable, but presumably it depends on perceptions of the degree of uncertainty attached to future returns, and this may shift

[8] The quote is from *Production of Commodities by Means of Commodities* (Cambridge: Cambridge University Press, 1960), p. 33.

for a variety of economic and non-economic reasons. The stability of the relationship between interest rate and profit rate then rests on the relative stability of such perceptions, arguably not very solid ground.

On the surface, the interest rate determination of income distribution appears to contradict Garegnani's argument that wages are conditioned by social conventions. In fact the two explanations are meant to be complementary. Interest rates vary within the upper and lower limits posed by institutional and historical circumstances, including the strength of labor unions, levels of employment, and other factors. Pivetti suggests that monetary authorities can follow a policy of low interest rates in order to insure a socially acceptable real wage. He does not deal with the possible complications of such a policy. After all, monetary policy is a major countercyclical tool. If a central bank regulates interest rates on distributional grounds, what is it going to do, for example, about demand-pull inflation? For that matter, can a central bank really regulate long-term interest rates?

This brings us once more to the disagreements between post-Keynesians and the followers of Sraffa. Representing the first camp, Steindl registers vigorous opposition, on several grounds, to the monetary determination of income distribution. Among these is the equilibrium method used in the reasoning. Not all followers of Sraffa subscribe to the interest rate theory of profit, either. Pasinetti objects on grounds that two alternative determinations of distribution, namely an exogenously given wage and the post-Keynesian growth relationship, may be more appropriate to particular historical situations. On the basis of the various papers and comments, it is not possible to do more than register the arguments on both sides. The criterion of logical consistency does not appear to give one side an advantage over the other, and no empirical evidence is presented to test the arguments.

Levine proposes a marriage between Sraffa's economics and the concepts of imperfect competition, customer markets, and cost-determined pricing *à la* Arthur Okun. Sraffa's system, Levine shows, can accomodate oligopoly and barriers to entry. But the issue remains: What additional insight do the Sraffa equations bring to the study of the oligopolistic firm? After all, there are plenty of neoclassical imperfect competition models, and the superiority of a Sraffa-based alternative needs to be demonstrated. The same question can be raised about some other proposed applications and extensions of Sraffa's classical revival.

Schefold proposes an alternative to the neoclassical theory of consumption. This classical approach to demand is to be based on "hierarchies of needs" and a topography of life styles associated with different social classes. Individuals learn a new pattern of consumption when they move to a higher income level, and belong simultaneously to diffrent groups that shape their decisions as to the allocation of their time and income. Consumption patterns are social conventions that tend to acquire force of habit in individual households. All this sounds vaguely plausible,

but as Burmeister puts it in his comment, it begs the question: What is a "need"? Applied historical work may flesh out and clarify such categories. Schefold also points out that simultaneous demand-and-supply determination of prices does not allow for the multiple and evolutionary repercussions of technical change. He claims these can be incorporated into the sequential classical approach. But "can be" is a safe term. The volume of essays under review shows no evidence of such an incorporation. Is such an exercise possible in practice, as opposed to theory? And what new insights will it bring?

Jaime Ros shows that in the presence of differential rates of technical progress, increasing returns to scale, differential income and price elasticities of demand, the long-term effect of international trade may not be positive for a trading economy. As he points out, standard trade theory based on comparative advantage simply assumes away all such phenomena. What Ros does not mention is the new, growing trade literature on imperfect competition and increasing returns.[9] The results of these models may not be exactly the same as the conclusions Ros reaches, but the ideas are similar. The new trade theorists see their conclusions as complementary to comparative advantage arguments, and stay within the marginalist framework. Does the structure Ros uses provide more insight into these issues than the conventional imperfect competition models used by new trade theorists like Paul Krugman? The answer is not clear.

Roncaglia persuasively argues that the relevance of Sraffa's analysis for policy lies less in his specific analytical tools than in his reconstruction of classical economics.[10] He provides a balanced evaluation of the tools. Take Sraffa's basic versus nonbasic distinction. Taxes on or technical improvements in nonbasics do not affect prices of basics, while changes in basics have a general impact. But there is a problem in using this distinction to study real world policy issues, since technical change and other changes over time can transform basics into non-basics and vice versa. Furthermore, at any one time, "we will have commodities 'in transition' from basic to non-basic status, or the other way round" (469) The concept obviously has to be handled with great care. Actually there are even more complications for anyone wishing to apply this abstract distinction. Changes in nonbasic producing sectors may have limited direct impact on relative prices, but cause changes outside the Sraffian core, and thus change the data undedrlying the price equations. For example, Roncaglia points out

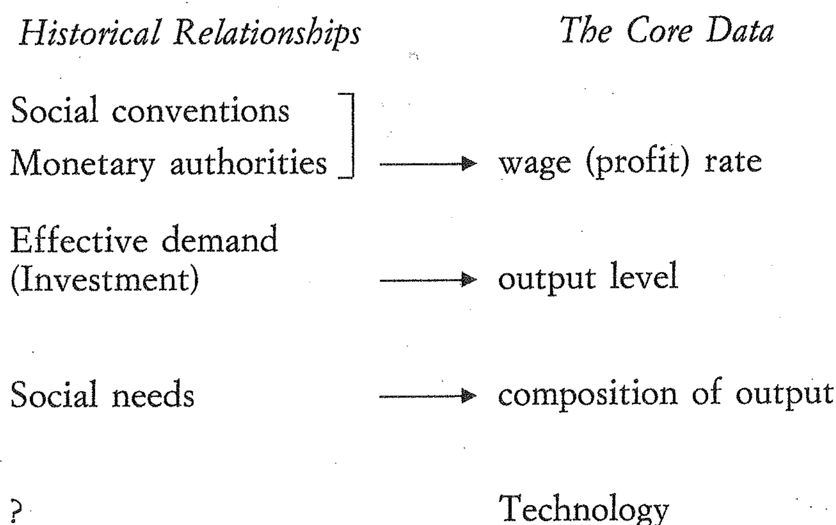
[9] See PAUL R KRUGMAN, "Increasing Returns, Monopolistic Competition, and International Trade" in *Journal of International Economics*, 1979. Also KRUGMAN (ed.) *Strategic Trade Policy and the New International Economics*, (Cambridge: MIT Press, 1986). An interesting study on the interaction between technical change and international trade is JAMES BRANDER and BARBARA SPENCER, "International R&D Rivalry and Industrial Strategy" in *Review of Economic Studies* 1983.

[10] Various other authors, prominently Garegani and Shefold, also imply that generally the tools are not the main point of Sraffa's legacy.

that wage goods are non-basics. Then education and health care are nonbasics, and changes in these sectors do not affect prices of basics. But such changes have the potential to reconfigure the entire productivity picture, leading to changes in all production coefficients. In the wilderness outside the neat little core, life is complicated. It is difficult to see what practical usefulness the basic-nonbasic distinction can have.

Steindl's paper further illustrates the pitfalls of putting Sraffa's analytical constructs to practical use. Steindl tries to show that the standard commodity can be used in macroeconomics as a means of measurement and aggregation. In his comment Kurz points out that the standard commodity is a theoretical tool, useful in clarifying value and distribution relationships, not a solution to index number problems. Steindl wants to use it in comparing different economies and the same economy over time. But, Kurz counters, different technologies have different standard commodities. Furthermore, where there is joint production, a standard commodity may not exist.

The chart below sums up the various propositions, reviewed in this section, about the determinants of the core data. Two observations can be made about the overall picture that emerges from these essays. One, the only relatively clear and well-defined relationship between the core data and the underlying phenomena is that between effective demand and the output level. The output multiplier is the one part of Keynesian economics that seems to fit into the Sraffian reconstruction. The other suggestions beg questions of the kind asked above. The second observation is that, just like neoclassical economics,



the Sraffian classical alternative has little to say about the sources and mechanisms of technological change. The classics, of course, especially Smith and Marx, had a lot to say about technical change in their own time. Sylos Labini's defense of Smithian increasing returns is illuminating, but is not in itself a treatment of 20th century technology. Kaldor worked on growth

models based on the Smithian view of technical change in the 1950s and 1960s, but the papers in the book do not deal with his technical progress function, or with any other explanation of this key variable.

VI. *Conclusion: Achievement and Failure*

Marginalist economics has been dominant in the profession for almost a century now. It is still going strong, in spite of the shortcomings pointed out by Sraffa and by the authors of the *Essays on Piero Sraffa*. What explains this incredible success? One explanation is the versatility of the supply and demand approach: it has something for everybody, for every type of question, for every level of abstraction. An economist can do pure theoretical exercises with abtruse general equilibium models, or trace the impact of a tariff rate increase on the sugar market. From the abstract to the concrete, the theory provides a frame of reference, a way of checking coherence and consistency. The consistency may be an illusion and the coherence an intellectual straitjacket that stands in the way of a real understanding of the economy. But as long as no alternative exists, all such objections are futile.

Sraffa's work and the theoretical exercises based on it liberate researchers from the narrow confines of supply and demand schedules. Without the Classical Sraffian indictment of marginalism, it would be difficult to justify, for example, Levine's search for new insights on firm behaviour, or Schefold's study of the evolution of demand. The demonstration that relative prices can be determined separately from output and demand does indeed lay the groundwork for alternative explanations of these variables. This, surely, is a great achievement. The book under review can be regarded as a stage in the construction of the alternative approach. On the basis of the above review, we can try to judge how well the construction is going.

I just described how Heinz Kurz shows the inapplicability of the standard commodity to empirical aggregation problems. He is persuasive, and the standard commodity remains a pure theoretical construct. The same thing can be said about almost all the applied papers reviewed in the previous section: most are shot down on theoretical grounds. As the rundown of the essays shows, theoretical and history of thought exercises dominate the research project. The theoretical constructs that are on the level of abstraction of marginalist general equilibrium equations are well elaborated. But the steps going from this level to more concrete analyses are not theoretically elegant enough to meet the approval of the Sraffian camp. Perhaps the purity of the theory is being maintained by sacrificing intermediate and applied variants.

There is little agreement on propositions outside the core. There is little agreement on the exact use to which the core relations can be put. Neither

is the relationship between Sraffian theory and other non-marginalist theory clear. In particular, there are different views on Michael Kalecki's output and employment model and Nicholas Kaldor's growth model. Are these disagreements a sign of progress and clarification? Robert Solow, in his Nobel lecture, said that marginalist economics is viable because it has attracted a research community. The authors who have contributed to this volume do not appear to build on each other's work. Neither does the book represent a serious attempt to persuade outsiders. For these reasons, the uneasy alliance of Post-Keynesian and Sraffian researchers does not appear to be a growing research community. Of course, one could reply to Solow that there is more to judging the success of a school of thought than a head count.

It is to be hoped that Sraffa's clarification of classical ideas leads to a new economics in the full sense of the word. For this, the core relations have to percolate down to lower levels of abstraction, and to the study of today's economic issues. In the *Essays on Sraffa*, some scholars are trying to achieve just that. In the meantime, the vast majority of economists show no signs of being persuaded that this particular alternative to marginalism holds promise. And time is running out.

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