

“Marshall, Sraffa and Keynes: Incompatible Bedfellows?”

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I

I want to make my principal theme, the role of the (Classical) concept of centres of gravity in the work of three great Cambridge economists: Marshall, Sraffa and Keynes. There are a number of obvious connections among them apart from their Cambridge connection for both Sraffa and Keynes spent much of their professional life either using, amending, criticising, overthrowing and/or evaluating Marshall's work. Thus Sraffa's 1925 paper and, especially, his 1926 paper specifically were directed to a critique—a devastating one, I would say—of Marshall's contributions to value theory. (Sraffa makes this perfectly clear in his reply to D. H. Robertson in the 1930 *Economic Journal* Symposium on “Increasing Returns and the Representative Firm.” “We seem to be agreed that [Marshall's] theory cannot be interpreted in a way which makes it logically self-consistent and, at the same time, reconciles it with the facts it sets out to explain. Mr. Robertson's remedy is to discard mathematics, and he suggests that my remedy is to discard the facts; perhaps I ought to have explained that . . . I think it is Marshall's

theory that should be discarded.” [p. 93]) Keynes used, developed and ultimately rejected much of Marshall's theory of money and interest. He attempted to absorb Marshallian value theory into his theory of the general level of prices in the *General Theory* in which his purpose was to integrate value, production and monetary theory into one system. He abandoned the specifically Marshallian aspects in 1939 (unless we adopt the Cambridge maxim that it is all in Marshall—it probably was, see pp. 374–77 of the *Principles*) when in reply to Dunlop and Tarshis, he outlined in rudimentary form the normal cost pricing hypothesis. Keynes wrote a superb biographical essay on Marshall before he was fully into his own revolutionary stride and so was more accepting of Marshall's monetary theory than he subsequently was to be.¹ Finally, Marshall belonged to, or at least, thought that, or presented himself as belonging to, the Classical tradition which Sraffa inherited (and for which in his 1960 book, *Production of Commodities* . . . , he provided the foundations both for a revival and an updating).

One of Sraffa's purposes, though, was, to attack the inner logic of Marshall's system of thought because, whether Marshall really believed he was fulfilling what the Old Masters had sensed but not got quite right or had not fully developed—Joan Robinson for

*University of Adelaide and Scarborough College, University of Toronto. The argument of this paper has been influenced by comments from Tom Asimakopulos, Melanie Beresford, Krishna Bharadwaj, Jon Cohen, John Eatwell, Pierangelo Garegnani, Peter Groenewegen Prue Kerr, Bruce McFarlane, Murray Milgate, Dušan Pokorný, Alessandro Roncaglia, Tom Rymes, Claudio Sardoni, Bertram Schefold, and Ian Steedman. As ever, I thank them all but implicate none. The paper itself should be seen principally as speculative and exploratory.

¹“Alfred Marshall” in *The Collected Writings of John Maynard Keynes*, Vol. X, *Essays in Biography* (Macmillan for the Royal Economic Society, 1972), pp. 161–231.

one thinks that Marshall was foxing on this²—certainly, in Sraffa's view, his *effect* was both to subvert and to emasculate the robust Classical tradition on which he drew. When I say in Sraffa's view, I think that there is direct evidence for this in Sraffa's own writings and that it can be read indirectly into them as well from the hints and asides which he gives. Even more, there is another source of indirect evidence which is provided in the writings of Pierangelo Garegnani and Krishna Bharadwaj who, of Sraffa's colleagues and friends, are among the most in accord with Sraffa's own views. In Krishna Bharadwaj's fine 1976 R. C. Dutt Lectures, *Classical Political Economy and Rise to Dominance of Supply and Demand Theories*, she explicitly acknowledges that the themes developed there owe much to discussions over the years with Sraffa.³ She refers in many places, especially in the second lecture, to the 1925 and 1926 papers.⁴ Sraffa gave her access to Marshall's copy of Pigou's *Wealth and Welfare* in which Marshall's misgivings about Pigou's extension of his—Marshall's—mode of analysis are only too plainly expressed.⁵ There is also her paper on Marshall's early work on value, edited by J. K. Whitacker.⁶

²Joan Robinson is but one amongst many, for example, Cannan, Schumpeter, Ashley, Dobb, Meek, Sraffa himself.

³"I have drawn liberally upon the writings of Piero Sraffa and on the innumerable and stimulating discussions we have had over a long period."

⁴"Sulle Relazioni fra Costo e Quantità Prodotta," *Annali di Economia* II, 1(1925), pp. 277–328.

⁵"The laws of returns under competitive conditions," *Economic Journal*, 36, 1926, pp. 535–50.

⁶Krishna Bharadwaj, "Marshall on Pigou's *Wealth and Welfare*," *Economica*, XXXIX, 1972, pp. 32–46.

⁷Krishna Bharadwaj, "The Subversion of Classical Analysis: Alfred Marshall's Early Writing on Value," *Cambridge Journal of Economics*, 2, 1978, pp. 253–71. The subversion referred to in the title consisted essentially of moving away from the objective facts of production to a psychological, subjective view of consumption, *et al* in value, distribution and production theory, and in scrapping the central concept of the surplus—its creation, extraction and allocation—in favour of a theory of

I have chosen to make centres of gravity the main theme because not only do they feature extensively and centrally in Marshall's and Sraffa's own work (also, I shall argue, in Keynes's work though, of course, in a different context) but also because they have become a major point of disputation in current developments of Post-Keynesian theory by Joan Robinson and her followers, on the one hand, and Pierangelo Garegnani, on the other. (I had better own up immediately to belonging to, I think, the former team!) This is both with regard to the fundamental nature of the critique of orthodoxy which arises out of Sraffa's work (and takes in Marshall's contributions as well as Walras's and Wicksell's and their modern offshoots) and also with regard to the methodology which ought to be followed in the new developments, what the late Ronald Meek called the rehabilitation of Classical Political Economy. If we might summarise briefly the general nature of the disputes, Garegnani is inclined to say that the orthodox theory is wrong but its methodology (in so far as it consists of comparisons of long-period positions) is sound, while Joan Robinson says it is

the allocation of *given* resources, in which the role of substitution in consumption and production could have full play. Walsh and Gram sum it up very surely when they write: "... in neoclassical economics production is an aspect of exchange, whereas in classical economics exchange is an aspect of production (fulfilling the requirement of equalising the profit rate in all sectors)" [Harvey Gram and Vivian Walsh, "Menger and Jevons in the Setting of Post—von Neumann—Sraffa Economics," *Atlantic Economic Journal*, VI, 1978, p. 49.] Even though Marshall tried to be a half way house, he nevertheless got trapped nearer the end of the subjective theory of value: witness the real costs which lie behind the supply curve and the almost tautological relationships implied in his demand and supply schedules—the inducements needed to call forth efforts, demands and so on. "What is actually paid out is what is *necessary* to be paid out in order to induce the sacrifices: or else, the services would not be forthcoming." [Krishna Bharadwaj, *Classical Political Economy and Rise to Dominance of Supply and Demand Theories* (Orient Longman, 1978, p. 35).

the methodology which is wrong. (The theory *within* this context, she argues, is not too good either, but that is *not* the principal or most fundamental criticism.)

I now outline what I think is entailed in the notion(s) of centre of gravity. (Discussions with John Eatwell, Pierangelo Garegnani, Bertram Schefold and Ian Steedman, together with a run along the Cam tow path greatly helped me to sort these out.) The first three are analogies drawn from physics, the fourth, an analogy drawn from meteorology. The first relates to a frictionless pendulum which always swings but always passes the same minimum point on its path to and fro. The second is a pendulum, the motion of which ends eventually *because* of friction, and so actually settles at the minimum point of its path i.e. *at* the centre of gravity of the first notion. (As we shall see, this notion is most closely related to Marshall's concept of a long-period position or at least to the end point of the process.) The third is, I think, David Champernowne's example of a dog always running towards its master who is riding a bike. The bike is the centre of gravity which itself is moving but the dog's direction of movement at any point in time can be predicted by knowledge of where the bike (and its master) is, at that point in time. The fourth analogy is, in many ways, I think, the most illuminating. We all know that various places and seasons may be characterised (in part) by the concept of their average temperature, even though, on any one day, because of the influence of special or temporary factors, the actual temperature may not coincide with the average. Yet the average is a good sustained predictor and description over the years, from year to year, and as between places. Similarly, the average temperature itself is explicable in terms of relationships between the *average* values of the factors which determine temperature, even though the actual value of each one of these factors in

turn may depart from *its* average value on any particular day. The average temperature thus is a centre of gravity in the sense of a central tendency to which actual temperatures will tend, the outcome of sustained and fundamental forces, that is to say, the average serves to explain most of the orders of magnitude of the values actually observed from day to day.

What of Marshall's work have I reread in order to write this paper? I suppose that I have concentrated on the Prefaces and Book V of the *Principles* and within that, on normal values; the famous (or infamous) Appendix H on increasing returns and long-run normal supply price; and Appendix I on Ricardo's theory of value, on how Marshall came not to bury Ricardo but to fulfill him. In addition, I have looked at Marshall's early essays on international and domestic values, noting, as I think Keynes did, that he worked back from the demand and supply schedules to what he thought underlay them—ultimately utility and disutility—rather than, as the Austrians did, and Wicksteed also, starting from subjective introspection and working outwards to the market phenomena. I also reread the symposium on increasing returns in the 1930 *Economic Journal* (and I thought again how remarkably acute Gerald Shove was—I suspect that Keynes asked him to contribute in order to pry out of him a substantial chunk of that "unpublished study of the relations between cost and output on which [Shove] had been engaged for some years," which formed part of the Cambridge oral tradition at that time). In addition to the work of Krishna Bharadwaj, I read Keynes on Marshall, Mary Marshall (I liked *it*—or perhaps her—best) and Jevons; and I have had access to a number of unpublished papers by Garegnani and Joan Robinson in which some of the issues discussed are relevant for the present theme. Part of what Garegnani has to say may be found in his comment on

Samuelson in the Brown, Sato and Zarembka volume⁷ and in his 1970 *Review of Economic Studies* paper; the last part of the latter paper is concerned to attack the sort of demand and supply theory that Marshall would have needed to build up if he wanted to tell a value and distribution story which gave answers to Classical questions concerning the determination of natural rates of profits and wages. Marshall approaches and retreats from such stories in a tantalising way all through the chapters on distribution in the *Principles*. He wants to tell them because they illustrate his faith in the principle of substitution, but he does not want to be nailed down by the logical inconsistencies which he also senses.

Why do I think the criticisms of natural or normal or prices of production (I stress that they are *not* necessarily synonyms) are important? Because there is currently a debate going on concerning whether they should or should not be maintained in a Marxist and/or Post-Keynesian analysis of a modern capitalist economy moving through time. I think that their natural place probably is in the theory of the pricing which characterizes important sectors of such economies, in the normal cost pricing hypothesis and the connection of pricing to the investment decision. In particular, I think that there may be a role for the concepts of the natural rates of profits and of wages, that they are macro concepts associated with the working of the system as a whole, which impose themselves as norms on group behaviour within the system. The centre of gravity concept also may have a part to play in the Keynesian context of short-run rest states, a view which, I think can be supported by Keynes's own writings and by Jan Kregel's interpretation of them in his 1976 *Economic*

⁷Pierangelo Garegnani, "On a Change in the Notion of Equilibrium in Recent Work on Value and Distribution. A Comment on Samuelson," in M. Brown, K. Sato and P. Zarembka (eds.), *Essays in Modern Capital Theory* (Amsterdam: North-Holland, 1975), pp. 25-45.

Journal article.⁸ This is not an interpretation with which Joan Robinson would agree, nor, I should add, David Levine, who recently gave two stimulating and challenging lectures on cyclical growth in capitalism, in which he denied, in effect, that the prices of production had any operational significance in the development of the analysis of the processes involved.⁹

II

I return now to Marshall himself. It is clear, I believe, that he wanted to make the concept of normal values, and especially long-run normal values, the centre pieces of his theory of value (and distribution). The long-run equilibrium positions and the prices associated with them were the partial equilibrium, firm and industry, versions of the long-run positions of the economy as a whole which are found in the Classical scheme (and, of course, in Marx). Marshall realized that he would eventually need to have a theory of the levels of the normal profits and normal wages to fit into his theory of the composition of the individual prices of the commodities of individual industries—otherwise there would not be an explanation of a *general* level to which the prices, *et al*, of each industry would have to measure up—but he kept backing away from going the whole way in supplying them because he sensed the logical difficulties contained therein. Where Marshall differed from the Classics was that his long-run prices were the outcome of the symmetrical

⁸J. A. Kregel, "Economic Methodology in the Face of Uncertainty: the Modelling Methods of Keynes and the Post-Keynesians," *Economic Journal*, 86, 1976, pp. 209-25.

⁹See also David P. Levine, "Aspects of the Classical Theory of Markets," *Australian Economic Papers*, 19, 1980, pp. 1-15; "Production Prices and the Theory of the Firm," *Journal of Post Keynesian Economics* (forthcoming).

and opposing forces of supply and demand.¹⁰ The forces which affected prices conveniently could be placed in one or other of these categories; at the level of a firm or an industry, one set reasonably could be regarded as independent of the other; the prices would settle or tend to settle at levels which the two blades of the scissors (or balls in the bowl) dictated; and in normal conditions the forces were sustained and fundamental enough to make it reasonable to talk of an equilibrium being struck, or being potentially there to be struck, providing the *cet. par.* clauses held long enough in practice. A normal price is a price "which any one set of conditions tends to produce" (p. 372). (If we were to insert "ultimately" before "tends", we would have our second analogy, the pendulum the motion of which is subject to friction.)

Marshall wanted to use the concept of equilibrium because, despite his many asides and protestations about biology being the Mecca of economists, in his actual analysis (which he was very reluctant to apply directly to actual situations), he really did want to use mechanical analogies—pendulums, etc. He wanted to argue that the state of rest which ultimately would be reached had an effect on what was happening at any moment even if, in fact, that movement itself was *away* from the equilibrium position. It is true that he tried to be careful, or rather he knew that he should be careful, to keep his statements confined to what would happen near or at an equilibrium, i.e., that any displacements considered were from an already achieved equilibrium—hence his starting point of analysis of a stationary state—but, nevertheless, many of his applications had to occur well away from the intersections and take in the curves (and the areas

¹⁰Peter Groenewegen reminds me that "the Classical concept of natural price was not necessarily independent of supply and demand." But I think it is fair to say that, as a generalisation, their role was minor to non-existent relative to the central role that they play in Marshall's analysis.

under them) associated with those positions away from them. There is no doubt that as wily a bird as Marshall knew that there was the world of difference between starting an analysis at an arbitrary point and seeing whether there were any forces that would take the economy, industry, firm, towards any equilibrium that might be implied by forces present at that moment of time; and starting the analysis at an equilibrium from which a chance notional or conceptual displacement has occurred. But with typical Marshallian fuzziness (foxiness), this is never brought out as clearly or as starkly as it should have been.¹¹

It was on these very points that Sraffa went after Marshall in his 1926 article—trying to find those situations where the assumptions of partial equilibrium analysis were actually met so that the dependence of supply factors on demand, and vice versa, really was "of the second order of smalls" and thus could be ignored: "to what extent the supply curves based on the laws of returns satisfy the conditions necessary to enable them to be employed in the study of single commodities under competitive conditions" (p. 538). The answer was, as we know, dispiriting—only in those cases where economies were external to the firm but internal to the industry, "precisely the class which is most seldom to be met with" (p. 540), were the conditions likely to be met.

Marshall also wanted to introduce the concept of substitution—"the dynamical principle of "substitution" . . . seen ever at work" (p. xv)- ; the universality of the theory of value, distribution and production (instead of their separation as in Classical political economy), i.e., that all could be embraced in

¹¹In saying that Marshall wanted to use mechanical analogies, I am aware, of course, that he had in common with Marx a view of economies as structurally changing organisms though he would have objected to the thesis anti-thesis, inherent contradictions, seeds of its own destruction, aspects of Marx's analysis.

effect within exchange theory; and his adaptations of what he took to be the essential truths of the laws of diminishing and increasing returns into his supply and demand analysis. He wanted, as Joan Robinson has stressed, to analyze an economy moving through time with accumulation going on, yet still use the supply and demand apparatus as the principal illuminator of the process. (This illustrates another conflict in Marshall's thought and approach: he was at one and the same time the master of empirical generalizations and also a superb user of deductive reasoning.) His attempt to centralize the principle of substitution and his equal stress on continuity led him to develop marginal analysis ("In economics, as in physics, changes are generally continuous" [p. 409, n. 1]), choice and change at the margin, drawing, he argued, on Classical uses to justify this, as it first arose in the theory of rent.

However, as Krishna Bharadwaj shows, he slurred over the essentially different concepts of the margin in Classical political economy—those associated with different *qualities* of land, where there is no change of a neoclassical sort occurring, no counterfactuals as Sen characterizes what Sraffa is doing in *Production of Commodities . . .*,¹² as opposed to the typical marginal procedure—in Wicksteed's view, the only nonspurious one—derived from changing the intensity of production on a given piece of land. Marshall attempts to encompass within one framework the essen-

¹²[Sraffa's methodology] can be seen as exploring how much can be said about the *inter-relations* between prices, distribution and quantitative magnitudes using only directly observed data, without making any use of *counter-factuals*, [the use of which] is an essential part of any 'marginalist' analysis (what *would have* happened had the facts been different, e.g., if one more unit of labour had been applied?)" (A.K. Sen, "On the Labour Theory of Value: Some Methodological Issues," *Cambridge Journal of Economics*, 2, June 1978, pp. 180–81.). What, though, are we to call Sraffa's procedure when he considers different values of (say) the wage, while holding the methods of production constant?

tially different historical tendency to diminishing returns in agriculture and the increasing returns phenomena of manufacturing and the accumulation process generally. The latter was sensed and first developed by Smith and understood and completed by Ricardo and, especially, Marx, but only kept alive, really, in modern times, by Schumpeter, Allyn Young, Kalecki, and Kaldor—and also Arthur Smithies, many years ago, in a perceptive comment on one of Solow's papers.¹³ Apart from the mixing of the essentially different processes involved, it was the difficulties of obtaining a *functional* relationship between cost and output levels and changes in the latter which especially concerned Sraffa and which he felt undermined the partial equilibrium method.

Marshall also vitally breaks with Classical methodology when he attempts to derive a uniform framework—a common set of principles—with which to deal with value, distribution and production (with the latter not separated from the former, as in Marx's analysis). Classical methodology did not look for universality and uniformity but for specificity¹⁴—for separate explanations of the different incomes of the different classes, with the explanation of value being different from and logically subsequent to the explanation of distribution. Thus, distribution preceded value in a logical sense—a view that is in Ricardo and in Marx as much as it is in Sraffa, with his construction of the Standard

¹³"Perhaps the whole problem is too complicated for adequate reflection in a formal model. In that event, we could do worse than reread Adam Smith. . . . In Book I, he said that the division of labour was the mainspring of economic progress; and in Book II, that accumulation was a necessary condition for increased division of labour. How far have we gone beyond that?" (A. Smithies, "Comment on Solow," *American Economic Review*, Papers and Proceedings, LII, (1962) p. 92.)

¹⁴It is ironic that Marshall who was so keen ultimately to adopt biological methods nevertheless should have adopted in practice the very method which distinguishes the method of modern physics from that of modern biology.

system and Standard commodity in order to "give transparency to a system and render visible what was hidden."¹⁵ Such an approach contrasts with the symmetry that Marshall found in diminishing marginal utility and decreasing returns, despite their different roots "in the quality of human nature . . . [and] the technical conditions of industry." Sraffa's response to this, as Krishna Bharadwaj noted (p. 51), was to wonder why "two such heterogeneous elements as human nature and industrial technology should bring about results so similar?" His answer was that there was an underlying behavioural premise which implied an ordering between alternatives but also destroyed the neat dichotomy of the facts of nature, psychology and engineering, on the one hand, and economic choice and valuation, on the other, so common in the usual presentation of orthodox theory.¹⁶

Nevertheless, as we have seen, Marshall wished to have a theory of normal prices which included as ingredients normal profits and normal wages, and he wished them to be long period as well. He thus distinguished, as we all know, a number of time periods, though he argued that they merged imperceptibly one into another, and could be short or long according to the purpose in hand, and actual or potential, according to the realism of the factors caught in the *cet. par.* pound in any particular case. He himself seemed to vacillate between whether they are actual or potential, sometimes naming periods of calendar time as illustrative of what he had in mind, wishing, I suspect, to have it both ways. That is to say, he wished his long-period normal prices, *et al.*, to be real centres of

¹⁵Of course, production—and human labour—logically precede both, as Marx explicitly stresses and Sraffa, to some extent only implicitly, too. See G. C. Harcourt, "The Sraffian Contribution: An Evaluation" in M.C. Howard and

¹⁶See Krishna Bharadwaj, *Classical Political Economy . . .*, pp. 51–54.

gravity—to help make sense of *actual* observations in *actual* time, yet he knew that if he simultaneously had in mind an economy which was moving forward through time with accumulation and technical advances occurring, there were puzzles in setting them up as being revealed by the averages of observations on actual prices. Indeed he himself says that only in the stationary state can the normal and the average coincide, be "convertible terms" (p. 372). It was, of course, Salter (*Productivity and Technical Change*, Cambridge: Cambridge University Press, 1960) who first solved this puzzle—or went as far as it is possible to solve it in a neoclassical framework—by having the equilibrium prices determined by intersections of supply and demand curves but, at the same time, insisting that it is only the latest vintages which have to earn the normal rate of profits. This gives any one industry a chance for its process of accumulation to reach this point, with output at the appropriate level, before the next wave of innovations occurs and changes the centres of gravity involved. Marshall, I am sure, sensed this solution but he never spelt it out as neatly nor as fully as Salter did. A similar concept of the centres of gravity that are associated with the latest techniques and the prices of the products which they help to produce is to be found in Marx and the modern Classical writers, e.g., Garegnani, Eatwell.¹⁷

¹⁷"At any one time a given commodity may be produced by means of a variety of techniques: some "fossils" embodying out of date methods, . . . not being reproduced since at existing prices they would yield a rate of return on their supply price lower than the general rate of profit, but which . . . do yield positive quasi-rents; some "superior" techniques . . . used only by a limited number of producers . . . yield super-profits . . . [T]heories of value and distribution are not concerned with these, but with "the conditions of production normal for a given society" (Marx, 1976, p. 129), the "normality" being defined by dominance throughout the competitive market." [John Eatwell, *Theories of Value, Output and Employment* (Thames Papers in Political Economy, Summer 1979), p. 4.]

Because Marshall was wedded to demand and supply analysis, and to having his long-period equilibrium positions determined at the intersections of supply and demand curves, he had to concern himself with problems of uniqueness and stability in a manner which the Classical economists could avoid. This had the further implication, as Krishna Bharadwaj argues, that neoclassical analysis only applies in situations where the conditions for stability are met—there is not the freedom of action in this framework which exists in Classical analysis and its modern counterparts, the “horses for courses” approach of Joan Robinson and Kalecki, for example. “[T]he classical theory [of value] is not constrained to permit only some specific changes of the many possible ones as alone consistent with the theory . . . does not have to presume more than is necessary for the limited objective of determining relative values at one ‘observed’ position of the economic system . . . [T]he supply and demand theories sought to explain a single observed position in terms of potential changes . . . as brought about by the balancing of marginal quantities operating through the principle of substitution . . . to be consistent with these explanations, the changes had to be in a direction . . . of the type postulated by theory.” (p. 67). Moreover, once Marshall had committed himself to a long-period position which is the outcome of the forces of supply and demand, and to having schedules, he was committed to not allowing the *approach* to equilibrium to affect the equilibrium position itself. More, he was committed to an assumption of reversibility, if the supply curves were to mean what he said they meant, which, as he knew, and Sraffa made very plain in his 1926 *Economic Journal* article, makes the concept of the long-run supply curve a very fuzzy one indeed. (That was why Marshall was so worried about Pigou using a

long-run supply curve in *Wealth and Welfare*.) Marshall discusses these issues in Appendix H and wants to dodge the conclusion that the long-run supply curve is irreversible, that it can only be an historical statement as opposed to a conditional one. But certainly Sraffa does not let him get away with this, either by use of the device of the representative firm or by other means.

I do not wish to imply that Sraffa himself does not believe in the value of what he calls the statical method, the use of the concepts of long-period positions and their comparisons. When he introduced the downward sloping demand curve analysis in 1926, with each firm its own little monopoly and so on, he said that he was dealing with situations where events stay steady enough for long enough to allow the statical method to be used. “Many of the obstacles which break up that unity of the market which is the essential condition of competition are not of the nature of ‘frictions,’ but are themselves active forces which produce permanent and even cumulative effects . . . frequently . . . endowed with sufficient stability to enable them to be made the subject of analysis based on statical assumptions.” (p. 542). It was Marshall’s use of the method in conjunction with supply and demand curves which he found objectionable. That is to say, Sraffa is *not* attacking the centres of gravity concept as such and, indeed, his entire book (1960) *is* about them—the pattern of the prices of production implied by the levels of activity and techniques of production in an economy at a given instant of time, when we make an assumption of a uniform wage rate (or rate of profits) to be included in them. Some usually orthodox economists have argued that Sraffa’s systems are already in, or are necessarily in, balanced or appropriate states, with the input-output arrangements and the prices of production being consistent with reproduction (and/or

expansion). This, for example, is Joan Robinson’s interpretation of Sraffa’s system¹⁸; it is *as if* we had *already* reached the end of Marshall’s long period, with long-run values established (but *not* by the forces of supply and demand) and all fossils from previous epochs removed, and all production processes in their most appropriate form. But the fact that Sraffa stresses that there is no implication that what happened this period need *necessarily* happen again, or had happened before, or that the means of production used this period *necessarily* came from the last period, or that those produced this period will *necessarily* be used to replace those used up this period, nor that there is any historical significance to be attached to his procedure of reduction to dated quantities of labour, suggests that this interpretation, though not unreasonable, is not necessarily what he had in mind.¹⁹ Rather, he takes the economy just as it is and asks: What pattern of prices of production is implied if we know (say) the wage rate and assume that a uniform rate of profit is to be received in each activity? And how do they alter when *we* alter the value of the wage rate? Of course, it can be argued, what *is* the point of such a procedure if there is *not* an implication of stability, so that those positions can serve to underlie the patterns of actual prices. These, of course, are influenced

¹⁸See, for example, *Collected Economic Papers V*, (Oxford: Basil Blackwell, 1979), pp. 285–87.

¹⁹Indeed, I know from personal experience that it is not: it was at Sraffa’s insistence that Vincent Massaro and I put the following paragraph in our 1964 *Economic Journal* note on Sraffian Syb-systems: “it must be stressed that the relationships in the economic system described here relate to one year only. They occur within the bounds of that year, and there is no necessary connection between them and the relationships that exist in other years . . . *not* implied that the means of production come from the immediately preceding year . . . that those parts of the gross product which are *equal* to the means of production will be used as means of production in the following year.” (p. 716).

by temporary factors as well, yet we *have* isolated the effects of sustained and fundamental forces by our procedures—this certainly is Garegnani’s argument and it is at this point that Joan Robinson (and Levine) part company with him.²⁰

III

The same problems arise when considering Keynes’s system of thought in the *General Theory*, and as it has been adopted and developed in the Post-Keynesian macro theories of distribution. If his theories are to have operational content, it is argued, and, in particular, if they are to guide model builders and policy makers, we need to be able to say that the actual observations on national production, expenditure and income, and their components, are pretty good approximations to their theoretical counterparts. The latter themselves are the values that would be associated with the short-run rest states of the system that are implied in actual situations at any moment of time.²¹

We all know how we make this point in our first year classes: we draw the aggregate planned expenditure schedules (*not* Keynes’s aggregate demand schedule, which shows, rather, the total of what businesspeople expect their receipts will be, for any given level of employment) and the 45° line (which, given certain, not unreasonable, assumptions about expectations and businesspeople’s behaviour, may be interpreted as a functional relationship between the total of sales expected at any moment of time, including desired stock accumulation and the total of the production and, therefore, employment

²⁰As Peter Groenewegen again reminds me, “this is the crux of the problem of the interpretation and use of natural prices in economics, at least as Ricardo saw it.”

²¹But see below for a discussion of whether they are short-run states.

that they will call forth from the existing short-run utilization functions). We argue that if the economy is not at the point of intersection of the two when we start the analysis, so that unintended changes in stocks occur, what changes is *not* either of our two schedules but the businesspeople's perceptions of what their expected sales are. They change their production and employment levels accordingly, collectively taking the economy closer to the point where what is expected and what actually happens coincide. In other words, the initial non-fulfillment of investment plans leads not to *their* change but to a change in the perceptions of what sales are. In this way, there is a reasonable chance that the economy will not be too far away from its implied rest states at any moment of time, unless, of course, it has been buffeted by a large shock or shocks.²² The point I want to make is that Keynes (and we) are using the concept of centres of gravity to tell this story, a procedure which Joan Robinson would regard as illegitimate, at least in some instances. Thus, she criticizes IS/LM analysis because unless the economy is actually at the intersection of the IS and LM curves, its actual movement will disturb the *cet. par.* factors underlying their construction; or to return to the textbook model, she argues that by the time income has moved to a level where planned saving is equal to a previous level of planned investment, the level of planned investment may well have changed. We then are required to scrap the short cut of using the statical method and tell a much more sophisticated story of the initial failure to reach an implied rest state changing the rest state itself, by, for example, changing the rate of planned investment, especially in stocks. That is to say, we let expectations have their way

²²The same story may be told in terms of Keynes's aggregate demand and supply schedules, and the three cases of possible relations between short-run and long-run expectations and their feedbacks or lack thereof, which Kregel outlines in his 1976 *Economic Journal* article.

and so work with a theory which, as Garegnani said of Hicks's temporary equilibrium method in *Value and Capital*, is barren of "definite results" *because* it had scrapped the notion of long-run positions determined by sustained and fundamental forces (and comparisons of them) in favour of a method of short-period equilibrium and a sequence of them. (We have gone even further, of course.)

Keynes himself moved back and forth between both views, for there are many stories in the *General Theory* where a process is traced out over 'time', while at other places Keynes concentrates on the characteristics of a sustained rest state. His principal purpose was, of course, to show that a capitalist economy left to itself could get stuck in a prolonged slump, that it would not give out signals that would lead it from there to a position of full employment. Moreover, by the time he had published the *General Theory*, he had all but despaired of finding a determinate unit of time into which all the various interrelated processes and decisions he was analyzing could be fitted—so he decided never to push any particular piece of analysis very far past its starting point, preferring to get across the central message only. Even then, as he says in his 1937 lectures, as Kregel (p. 213) has documented,²² were he to write the book again, he would start with the simplest model in which (in textbook terms), the economy is already at the total planned expenditures, 45° line intersection, because short-run expectations are assumed to be correct and fulfilled, so as "to distinguish the forces determining the position of equilibrium from the technique of trial and error by means of which the entrepreneur[s] [discover] where the position is."²³

That Keynes himself at times used the

²³*The Collected Writings of John Maynard Keynes*, Vol. XIV, (London: Macmillan, 1973), p. 182, quoted by Kregel on p. 213.

Classical concept of a sustained rest state, a long-period position around which the actual positions fluctuated, may be inferred from, for example, the passages at the end of Book IV of the *General Theory* (Chapter 18, Part III, pp. 249–54). There, he states that "it is an outstanding characteristic of the economic system in which we live that, whilst it is subject to severe fluctuations in respect of output and employment, it is not violently unstable . . . seems capable of remaining in a chronic condition of subnormal activity for a considerable period without any marked tendency either towards recovery or towards complete collapse" (p. 249). Keynes discusses four conditions (relating to the characteristics of the consumption function, the marginal efficiency of capital schedule, the money-wage rate and the impact of achieved investment rates on the marginal efficiency of capital schedule) which "together are adequate to explain [why] . . . we oscillate, avoiding the gravest extremes of fluctuation in employment and in prices in both directions, around an intermediate position appreciably below full employment and appreciably above the minimum employment a decline below which would endanger life . . . a mean position . . . determined by "natural" tendencies . . . which are likely to persist" (p. 254). Sadly we see today the consequences of his views about the upward stability of money-wages (and therefore prices) *not* being borne out.²⁴

²⁴I am especially indebted to Murray Milgate for bringing this point of view to my notice and making me re-read the *General Theory* with it in mind. John Eatwell (*Theories of Value . . .*, pp. 5–6) interprets Keynes in this context as abstracting in his analysis from the effects of accumulation (other than on employment), i.e. from changes in the dominant and persistent forces which lead to secular movements in the rest states and the other centres of gravity e.g. normal prices. If we accept *this* interpretation, what I have called a short-run rest state, he calls a long-period position "whereby long-period is meant not that which occurs in a long period of time, but rather that which is determined by the dominant forces of the system within a period in which those forces are constant or changing but slowly" (p. 4).

The same trouble with handling time, which as we have seen troubled Marshall too, also may be illustrated by Keynes's discussion of the marginal efficiency of capital and the reasons why it is a declining schedule. You will remember that he argued that the shorter is the time period involved, the stronger will be the influence of rising supply prices of capital goods in reducing the marginal efficiencies of particular assets; while the longer is the time period that we have in view, the greater will be the influence of expected? actual? falls in the prices of the products which the assets are to help to produce, as larger and larger supplies of the products come onto markets to be sold. Obviously, Marshall's influence is of major importance here. Equally obviously, the analysis won't do because we are trying to determine *now* what the rate of planned investment expenditure will be—there is not the time to wait for these processes to work themselves out, which they never would in fact, because of the factors that would escape from the *cet. par.* pound in the meantime.

IV

Exactly the same puzzles hound the concept of prices of production, especially when we try to incorporate them as operational concepts in an analysis of modern oligopolistic economies. It is not just that the existence of a tendency to equality of rates of profit in all activities may be questioned because of oligopolistic structures, barriers to entry and all the other paraphernalia of modern industrial organization analysis. It is, rather, that the dynamic nature of capitalist development with the embodiment of technical advances through investment expenditures is so rapid in most periods as not to allow sufficient historical time for centres of gravity of a lasting nature to be formed. There is not the time, as Joan Robinson puts it, for the traders to become familiar through actual

experience, with what is the norm, so that when their bearings are cut loose, they literally are all at sea, rudderless, not knowing *where* they are heading, either back or to. The factors that we need theoretically to take as constant in order to allow the centres of gravity which they imply to be struck (for example, by the forces making for the formation of normal prices) are changing as fast or even faster than the outcomes that the relationships between them are intended to determine. In making this point, though, we should notice Eatwell's comment (p. 2) that the forces which determine the centres of gravity are the more dominant, systematic and persistent and that "[w]hether this centre of gravity is a temporal constant, or takes different values through time, does not affect the essence of the method."

And yet: there must be a grain of truth—or more than—in the centres of gravity concept. It is true that Solow is sceptical of the validity of the great ratios, the stylized facts which it was his and Kaldor's, Joan Robinson's and Henry Phelps Brown's purposes to explain, i.e., he asks: Does what is to be explained exist or not? But we do know that well-established rules of thumb exist in the business world, that pay-off periods, or desired rates of return, for example, fall within definite ranges, where exactly depending on expectations, confidence and the extent and intensity of uncertainty at any moment of time. We know that wage-earners have concepts of what is fair in relative wage structures, and in the overall share of wages, too, and that conservative politicians remind us, as they attempt to cut real wages, that they are trying to re-establish the historical share of profits without which it is not possible to expect the accumulators to accumulate. All of these

notions derive from the macroeconomic characteristics of the system, are imposed by its total workings on the individual decision-makers and therefore have a macroeconomic basis, as Pasinetti, for example, often has stressed. Hence, we have had in the Keynesian-Kaleckian tradition the development of macro theories of distribution, the Post-Keynesian theories of the rate of profits, where $r = g/s_c$, with g assumed to be independently given, mistakenly in my view, and corrected, of course, in von Neumann's model and in Joan Robinson's model.²⁵ It is suggested that these factors feed through, in a very general way, to the determination of individual prices, and so on, and that it is when there are wide departures from them, that instability and crises are most in evidence, because microeconomic behaviour is not in accord with the macroeconomic constraints imposed by the structure of the system.²⁶ Moreover, if we ally some of these notions with Sraffa's contribution of production inter-relations, we get close to the normal cost pricing hypotheses where the expected trend values of costs are married to profit margins, sometimes constant, sometimes variable in order to take in investment expenditures and their finance. All of this grows out of Sraffa's and then Keynes's criticisms of the original and simplest form of Marshall's explanation of pricing.

²⁵Thus, in Joan Robinson's version, $g = g(r)$, $g' > 0$; $g = s_c r$. Then $g(r) = s_c r$ and r may be solved for.

²⁶Sadly, we now seem to have entered a stage in the history of industrialised economies where both the centres of gravity themselves change rather rapidly and departures from them into the range of values where instabilities result now are more often the rule than the exception. I am indebted to Dušan Pocorný for some very perceptive comments on this baldly stated theme.