DRAFT

On the Cambridge, England, critique of the marginal productivity theory of distribution[*]

"… the marginal productivity theory of distribution is all bosh." (Joan Robinson ([1961]; 1965, 13.))

INTRODUCTION

The Cambridge, England, critique of the marginal productivity theory of distribution is hard to disentangle from (indeed, in the event, I found it impossible to do so) the related theories and developments that occurred alongside it. These include value theory, capital theory, growth theory and methodology. The economists associated with the critique – Krishna Bharadwaj, Pierangelo Garegnani, Richard Kahn, Nicholas Kaldor, Luigi Pasinetti, Joan Robinson, Piero Sraffa – were simultaneously developing their own macroeconomic theories of distribution to replace marginal productivity. Their theories were embodied in theories of growth and reflected the methodology associated with the critique and their own developments. The aspect of the critique that was most emphasised was set within the context of capital theory. Initially, it concerned the meaning and its corollary, the measurement of capital and its marginal product in an explanation of the distribution of the national product between wages and profits.

In Joan Robinson's 1953-54 article, "The production function and the theory of capital", which started the exchanges in the public domain, i.e., outside Cambridge, England, she complained that "The dominance in neoclassical economic teaching of the concept of a

production function has had an enervating effect on the development of the subject [and had] been a powerful instrument of miseducation. The student of economic theory is taught to write Q = f (L, K) where L is a quantity of labour, K a quantity of capital and Q a rate of output of commodities". (81) The units in which to measure L and Q are discussed and then the student "is hurried onto the next question, in the hope that he [sic] will forget to ask in what units K is measured. Before ever he does ask, he has become a professor [handing on] sloppy habits of thought from one generation to the next". (81)

But within Cambridge this puzzle, at least that concerning the meaning of a constant amount of capital, had been raised many years before in an especially astute article by Dennis Robertson, "Wage grumbles" (1931)[1]. He asked: what did it mean to hold capital constant when one more person, the tenth, say, was put to work with the existing nine in order to be able to measure the marginal product of labour and the given stock of capital? He cited spades as capital and asked were nine existing spades transformed into ten inferior ones, together with a bucket for one of the wage-earners to fetch beer to be drunk at "the smoko" period of their shift?

Neither Joan Robinson or Robertson explicitly pointed out that one of the reasons why the marginal productivity theory arose in the first place was in response to dissatisfaction with, not to mention outright hostility to the theories of value, distribution and growth of the classical political economists and, especially, of Marx. These were centred around the concept of the surplus – its creation, extraction, distribution and use – in their explanations of the origin and size of profits, and of the overall distributive shares, in which came to be embodied by Marx in a theory of the exploitation of labour, the original source of value. The theories were macroeconomic ones because the economy was treated as made up of classes, with the

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[1] Joan Robinson refers to it in n2 p48 of her 1953-54 article reprinted in Harcourt and Laing (1971). I am indebted to Peter Kriesler for reminding me of the significance of Robertson's article.

individuals within each class having different functions with regard to spending, saving, investing and producing. The marginal productivity theory was developed partly as a response to these views, but also as part of the development of the subjective theory of value in which utility was the source of value and the payments for the services of the factors of production were treated as prices in principle no different from the prices of commodities. In its most extreme form it was argued that what was put into production at the margin was what was received as payment.

I

There are at least four aspects to the Cambridge, England, critique:

1. If marginal productivity theory is to explain the distribution of the national income between wages and profits (and the origin and size of profits and the rate of profits), it has to have a unit in which to measure the quantity of capital which is independent of distribution and prices, see, for example, Sraffa 1960, 38, 70; 1962; Harcourt 1972, 192; Cohen and Harcourt, 2003.

2. One attempt to dodge the puzzles thrown up under 1 was to concentrate on the (Irving) Fisherian concept of the rate of return on investment, see Solow (1963). Pasinetti (1969) argued that this attempt floundered on the arbitrary procedure that capital-reversing (an "unobtrusive postulate") be ruled out by assumption so that the neoclassical intuition that scarcity underlies *all* prices may go through, see the discussion in Harcourt 1972; 58-69; 1976; Sardoni (ed.) 1992, 151-56, where Solow (1970), Dougherty (1972) were not amused.

3. The non-applicability of the results of the critique under heading 1, including the implications of capital-reversing and reswitching for marginal productivity theory, to general equilibrium theory *à la* Arrow-Debreu. The phenomena of capital-reversing and

reswitching were in Joan Robinson (1953-54), Champernowne (1953-54), Sraffa (1960) and Garegnani (1970)[2]. Their significance fully emerged in the discussions of the robustness or otherwise of what Samuelson (1962) called the neoclassical parables when examining the full MIT, Arrow-Debreu general equilibrium model, the jewel in the crown of modern neoclassical economics.

The parables were: negative associations between the rate of profits and (1) the capital-labour ratio, (2) the capital-output ratio and (3) sustainable levels of consumption per head; and that in competitive conditions, the wage rate and the rate of profits (or rental on capital) are measured by and measure their respective marginal products. That these parables are concrete implications of scarcity is clear. The capital-reversing and reswitching results rebutted the robustness of these findings which may be established rigorously in the one all-purpose commodity "corn" model.

Capital-reversing (the Ruth Cohen curiosum) is that a *less* productive, *less* capital-intensive technique may be associated with a *lower* value of the rate of profits (r). Reswitching is that the same technique, having been the most profitable one for a particular set of values or range of values of r and the wage rate (w), could also be the most profitable at another range (or ranges) of values of r and w, even though other techniques were the most profitable at values of r and w in between.

Once (or rather if) these results and their implications can be shown to be embodied in full blown general equilibrium intertemporal models, that is to say, in all forms of the supply and demand theories of value and distribution, as Krishna Baradwaj (1978) dubbed them, Sraffa's prelude to a critique of economic theory (of value and distribution) would be established as the starting point of a telling critique, together with

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[2] Velupillai (1975) has pointed out that Irving Fisher (1967) provided a numerical example of reswitching but did not, unusually for him, realise its significance. Garegnani (1970) took at least eight years backwards and forwards at the *Review of Economic Studies* before it was published. Christopher Bliss was the editor with whom he dealt. Sraffa (1960) was over 30 years in the making.

the base on which to erect an alternative theory developing in the modern age the approach of the classicals and Marx.

4. The inability of comparisons of equilibrium positions, Joan Robinson's "differences", to explain processes, her "changes". This criticism takes in the inescapable link between distribution and growth first discerned and analysed by the classical political economists and Marx – the laws of motion of the capitalist mode of production. The arguments concern method (where the Sraffians e.g. Garegnani, Kurz, part company with the Kaleckians/Robinsonians and also many neoclassicals, e.g. Bliss, Franklin Fisher, Samuelson, Solow).

It also takes in the 'true' nature or 'vision' of capitalism: is it primarily driven along by the consumer queen trying to maximise her expected life-time utility through her saving and consuming decisions, with all other institutions of capitalism being but the means by which her plans may be achieved? Or, is it driven along by ruthless, swashbuckling capitalists (all three sub-classes – industrial, commercial and financial) for whom accumulation and profit-making are a way of life; they call the tune and all other persons (in their respective classes) and institutions ultimately serve to dance to their tune?

II

I concentrate mainly on heading 1 because it is concerned with the inescapable need to explain the origin and sizes of profits and the rate of profits as macroeconomic concepts and magnitudes even within the general equilibrium model extending from here to eternity. Such a viewpoint implies that the implications of the phenomena of capital-reversing and reswitching are relevant in that context too. This is so despite the 'conventional wisdom', much reinforced by Frank Hahn's 1982 paper, "The neo-Ricardians", that they are not.

Secondly, I concentrate on heading 4 and how Joan Robinson, despite having no formal training in maths (she often said: "I never learnt mathematics so I had to think"), really nailed Samuelson and Solow on one of their most central and fundamental points e.g., in her 1959 *Economic Journal* article on crawling down the production function. Here I draw on Harvey Gram's insights because he understands the issues and Joan Robinson's thought more deeply than anyone else I know. He took Prue Kerr and me to task in our 2009 biography of Joan for not emphasising this criticism enough, see Gram 2010, 361-2. He was right but then he really is the expert. He praises the 1959 paper for its clarity, adding that "Her analysis of how 'a private-enterprise economy would continuously accumulate, under long-period equilibrium conditions, with continuous full employment of a constant labour force, without cyclical disturbances, in face of a continuously falling rate of profit' [(Joan Robinson [1959]; *C.E.P.*, vol II, 1960, 132-33)] should be required reading for all who embrace backwards induction (dynamic programming) as the best available technique for analysing a growth process. The associated saddle-path trajectories make clear that Robinson's main complaint has never been answered: 'there is still lacking any plausible account of a mechanism to keep the economy in equilibrium' (Joan Robinson [1959]; 1960, 131)" (Gram, 2010, 361).

Gram draws attention to the last public exchange between Joan Robinson and Paul Samuelson in *The Quarterly Journal of Economics* in 1975, Joan Robinson 1975, Samuelson 1975. He stresses that "the mother lode for the MIT approach to capital theory … is Chapter 12 of *Linear Programming and Economic Analysis … (DOSSO 1958)*", not Samuelson's 1962 *R.E. Studs* surrogate production function article. Samuelson, in his reply to Joan Robinson, cites various of his papers which build on DOSSO's equilibrium analysis and the "vast literature" on the 'Hahn problem' as the evidence on which to form a reasoned opinion on how tolerably inefficient or efficient are market and planned systems in the real world". (Samuelson 1975, reprinted in Joan Robinson, *C.E.P.*, vol V, 1979, 84). To Gram, this reply

is "remarkably coy". He wondered whether "it was mere sport for her critics to hide behind some rather advanced mathematics, when, in fact, Robinson (1959) had described in pellucid prose precisely the Achilles' heel embedded in their theory. The cat had … been let out of the bag in an almost casual aside: '… for society as a whole there is *need for vision at a distance* … in order that competition should lead a myopic market inevitably to the appropriate point …' (DOSSO, p.321, emphasis added). The intuitive understanding of this requirement [by] someone innocent of mathematics deserved a more candid response" (Gram, 2010, 362).

III

To mainstream economists the need to have a unit in which to measure capital which is independent of distribution and prices in any version of the supply and demand theories is incomprehensible.[3] For these economists the theory of distribution has been absorbed in a general theory of prices of all products and services, including the services of factors of production (inputs) where all equilibrium values that ensure that supplies equal demands are simultaneously and mutually (not the same thing) determined. Even with simultaneous determination, there is always the need to distinguish between the variables which determine and those, the values of which are determined. Furthermore, if the neoclassical intuition that prices are indexes of scarcity is to hold, we need to know what a "little" or a "lot" of capital actually is before we can say why the overall rate of profits is low or high because of the abundance or scarcity of capital. Hence Sraffa's 1962 reply to Roy Harrod: "What good is a quantity of capital … which, since it depends on the rate of interest, cannot be used for its traditional purpose … to determine the rate of interest [?]" (479). As the sphere of production has been absorbed into the sphere of distribution and exchange, the model of exchange may

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[3] Michael Mandler (1999) has argued that the production aspects of general equilibrium theory had completely superceded marginal productivity and the explanation of distributive prices and shares – a claim that surprised me most when I reviewed his book, see Harcourt 2002, F381.

now analyse all activity in the economy.[4] The need to explain why profits, whether pure or natural or normal, arise and what determines their size disappears – except, of course, in the real world that the theory is meant to illuminate.

In the Fisherian version which emphasises saving and investment, the theory concerns intertemporal prices, marginal rates of time preference (for saving) and marginal rates of transformation of present consumption into future consumption (for investment). In competitive conditions there is no reason to postulate an overall, economy-wide rate of profits, even if all individual rates of profit are equal to the rate of interest. Indeed, in mainstream theory, there is no conceptual difference between rates of profit and the rate of interest. By contrast, the conceptual difference is emphasised in the alternative approach. Profit is the return, expected and actual, on investment in capital goods. Interest is the hire price of finance (and as Joan Robinson, 1971, 28, reminded us) the yields of placements are the rates of return rentiers receive on the capital values of their financial assets. In the mainstream view the fact that the marginal product of capital for the economy as a whole may be an incoherent concept does not matter, for it is not needed to help explain an overall rate of profits that does not "exist" either.

IV

In the other approach there is no escape from the need to explain an overall rate of profits, which in the competitive situations analysed by the classical economists and Marx, sets the benchmark to which individual rates of profit expected on planned accumulation and received in each activity have to measure up. Moreover, it ruled the roost and was explained by how the potential surplus was created in the sphere of production as the outcome of the current state of the class war and existing techniques of production, while the actual surplus

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[4] This is why, though Marshall thought (claimed) he was evolving from, yet retaining, classical political economy, he was in fact emasculating it, see Bharadwaj (1989), especially Chs 6, 7.

was realised as profits in the sphere of distribution and exchange. This procedure was discussed rather vaguely by Marx as the realisation problem and more precisely in the post-Kalecki-Keynes era by the forces setting the point of effective demand through the interplay of overall planned investment and saving created by overall income and its distribution. The clearest exposition of this analysis comes from Donald Harris's contributions (1975, 1978).

They are neatly captured in the following diagram, see Figure 1. On the left-hand side we have the sphere of production. We plot the possible rates of profits and wage rates which the current state of technology will allow. We suppose the current state of the class war dictates a wage of w^* which implies that the maximum rate of profits which may be received is r^*. On the right-hand side we have the sphere of distribution and exchange, with the rate of profits on the vertical axis and the rate of accumulation, g, on the horizontal axis. From the Kaleckian-Keynesian saving relationship we get a line showing the actual profit rate as related to the rate of accumulation, $r = g \, / \, s_c$, where s_c is the marginal saving propensity of the capitalists (we assume for simplicity that s_w is zero). $g^* = g^*(r_e)$ shows planned accumulation as a function of the expected rate of profits r_e, itself a function of received profits, Joan Robinson's "animal spirits" function. Where the two relationships intersect shows how much of the potential surplus and thus profit have been realised. According to where the intersection occurs we may have depressed conditions, inflationary ones, even a sort of full realisation of the potential profits made possible by the current happenings on the LHS of the diagram. But, even here, this does not imply full employment of the work force, only that the capitalists and the economy are on Joan Robinson's version of Harrod's warranted rate of growth, g_w.
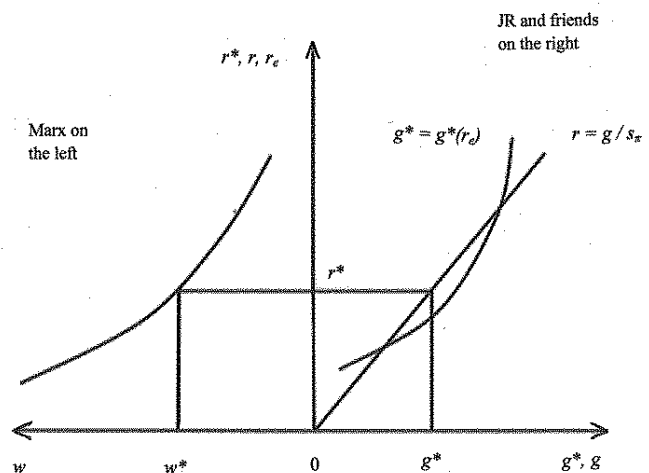
Figure 1: Class war, accumulation and distribution

V

Next, there is a debate about the role of econometrics in responding to the Cambridge, England, critique, that is to say, whether capital-reversing and reswitching are rare or common in reality – an incoherent question for Joan Robinson but not for Charles Ferguson (1969) who had faith (based on respect for Paul Samuelson) that production functions existed and were usually well-behaved. Parallel with these disputes are the criticisms of the use of the aggregate production function to 'explain' the distribution of income between wages and profits, and the respective contributions of deepening and technical advances to the growth of productivity over time, starting with Solow (1957). Apart from Franklin Fisher's important internal critique via the aggregation problem, Fisher (1971), the most important external critique is associated with Henry Phelps Brown (1957), Herbert Simon (Simon and Levy (1963)), Anwar Shaikh (1974, 1980) and John McCombie, later joined by Jesus Felipe, see, for example, Felipe and McCombie (2013).

Solow rationalised his procedure in 1957 as follows:

The factor-share device of my 1957 article is in no sense a *test* of aggregate production functions or marginal productivity … It merely shows how one goes about interpreting given time series if one starts by *assuming* that they were generated from a production function and that the competitive marginal production relations apply.

(Solow 1974, 121, emphasis in original)

This allows him to undertake his empirical work with a clear conscience *and* continue to carry out high theory within the most complex MIT model associated, for example, with DOSSO, see the discussion of Harvey Gram's arguments, pp 2-4 above. The external criticisms are more concerned with wrong specification so that even if an aggregate neoclassical production function is responsible for observed wage and profit shares, and wage rates and rentals – the authors do not for a moment think it is – the specification is flawed because the econometrics reflects an underlying national income identity which would be associated with *any* process that is responsible for establishing the observed data.[5]

Fisher (1971) makes a not dissimilar point when he argues that if the actual wage/profits share is relatively constant then it will be "as if" an aggregate Cobb-Douglas function was responsible for the observations. As Jesus and John are speaking at the session, I leave this strand now.

VI

I also wish to argue that the macroeconomic theories of distribution coming out of the structure of thought of post-Keynesian economics are consistent with *any* theory of the behaviour of individual firms, that, in particular, they are not restricted by an assumption of

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[5] In his Nobel lecture, Simon (1979, 49) noted that good statistical fits to the Cobb-Douglas production function "cannot be taken as strong evidence for the [neo]classical theory, for the identical results can readily be produced by mistakenly fitting a Cobb-Douglas function to data that were in fact generated by a linear accounting identity (value of output equals labour costs plus capital cost)".

perfect competition nor any specific form of the short-period utilisation function. I illustrate this by an account of Michal Kalecki's remarkable review of Keynes's *General Theory*. (It was published in Polish in 1936 but there was no full English translation until 1982, see Targetti and Kinda-Hass (1982).) In it Kalecki starts with a profit-maximising, cost-minimising firm, the production technique of which could well be Cobb-Douglas, situated in either a purely (freely) competitive or an imperfectly competitive market i.e., the firm is either a price-taker or a price-maker. (There is no implication that Kalecki was committed to these particular constructions; rather, they reflect his interpretation of what Keynes assumed about firm and production behaviour in *The General Theory* itself.) He nets out raw material costs and splits the value added implied by the net revenue and net cost curves into wage payments and surplus (= profits); he aggregates the values added of all firms in the economy to the economy as a whole and shows how wage-earners spending what they earn and profit-receivers receiving what they spend, given the level of overall investment spending, results in the overall levels of activity and employment, and the distribution of income between wages and profits, being determined at the same time. (It is clear that the latter may be interpreted to reflect determination by the class struggle and market power, not by "marginal products" of capital and labour.) A different value of investment expenditure would impinge on the relevant firms affected and the overall outcome would take on new values of activity and distribution consistent with overall saving and investment matching one another again. That is why systemic relationships have lives of their own.

VII

Next, I refer to the parting of ways between especially Garegnani and Joan Robinson. The issues were whether only the theory of the long-period position could be rigorous and so the basis of both the way forward positively *and* the critique of the supply and demand

theories, principally through the implications of the capital-reversing and reswitching results for the robustness of the universal application of the scarcity theory of value. Garegnani argued consistently throughout his working life that rigorous theory could only apply to the relationships in the long-period position, that this was as true of the classical political economists and Marx as it was of Marshall and Wicksell. In the long-period position the interrelationships of persistent forces associated with the concepts of natural prices, prices of production and Marshallian normal prices in turn could serve to illuminate reality. His final statement of this stance is in Garegnani (2012), which is published in the Special Issue of the *Cambridge Journal of Economics* of November 2012 on new perspectives on the work of Piero Sraffa, a statement that takes in his prior debates with Mandler in the pages of *Metroeconomica*, Mandler 2002, Garegnani 2005.[6]

Garegnani never saw (perhaps never admitted) that Joan Robinson and especially Richard Goodwin and later Kalecki through their independent development of cyclical growth models, whereby the development of capitalist economies over time may be analysed through the interrelatedness of happenings in successive short periods, in which is fused indissolubly the cycle and the trend, overcame his criticism, see Harcourt (forthcoming).

This proviso, however, does not affect his arguments that there is no avenue along which any of the supply and demand theories can escape the need to have a unit in which to measure capital which is independent of distribution and prices, nor from the need to have a uniform rate of profits in the long-period position and a theory of the overall rate of profits for the economy as a whole and of its (their) origin. Joan Robinson would have accepted the relevance of the latter for doctrinal debates at a high level of abstraction, see Bhaduri and Joan Robinson (1980) for her final statements on this issue.

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[6] Sadly the paper was published after his death in October 2011.

I close with the unresolved debates between Frank Hahn and Garegnani. Garegnani had argued for many years that unless a stable long-period position could be shown to exist and that required that the results of the neoclassical parables were robust in the general equilibrium system, these long-period positions, even if they existed, could not be shown to square with actual distributive shares in the real world which it was their purpose to illuminate. Both he and Bertram Schefold have spent the last 20 years and more establishing these propositions in various articles – Schefold's most recent paper was just been published in the September 2013 issue of the *Cambridge Journal of Economics*, Schefold (2013). Hahn's answer was so what? Even if stability proofs proved elusive the structure of the general equilibrium models was consistent with the axiom of a world of maximising individuals usually in a competitive environment.

Moreover, in Hahn's view, general equilibrium was not concerned with descriptive analysis but with the careful establishment of the conditions that had to be fulfilled if certain conjectures starting with Adam Smith about what could be expected of the actions of such individuals in a competitive situation were to be vindicated/confirmed. Hahn also argued that the system of Sraffa's 1960 book was a very special case of a general equilibrium system. He would not accept that very different economic intuitions, approaches and structures could have the same formal expression. Nor would he accept, I believe, the significance of the interpretation of Sraffa's system as a snap shot of the economy at a point in time, or that value could be determined by exogenous distribution. This last was a proposition that Krishna Bharadwaj understood very deeply indeed when, having followed the same intellectual pilgrim's progress as Sraffa through the classicals, Marx and the neoclassicals, especially Walras, Wicksell, Jevons and Marshall, she wrote her formidable review article of Sraffa's

1960 book with the title, "Value through exogenous distribution", Bharadwaj 1963; reprinted in Harcourt and Laing 1971.

Prior to his critical article, "The neo-Ricardians", Hahn (1982), Hahn (1975) argued that the neo-Ricardians (his portmanteau term inaccurately applied to all the critics) had textbook writers in their sights and in that context had done well but that neoclassical theories on the frontier were left unscathed. But since Hahn wrote that, many of the constructions in the textbooks have been adopted by those now working on the frontiers, especially within endogenous growth theory, and the critique of those constructions has mostly been ignored, or not even known about.

Alas, both Pierangelo and Frank are now dead but up until their deaths, Garegnani was continuing with developing his critique. I must be honest, I do not completely understand the formal details of either Garegnani or Hahn or Mandler's exchanges over these issues, so I can only suggest that if the critics carry the day, we could continue to build on the base of Sraffa's contributions, as I argued above, with renewed confidence.

Finally, one of the central issues is whether there is or is not any need to explain the origin and size of an overall rate of profits which was never attempted to be done, or thought needed to be done in the modern version of general equilibrium because it was argued to be neither theoretically or empirically important. Garegnani's discussion of this issue was allied with his discussion of the change in the concept and definition of equilibrium which he identified as associated with the rejection of the long-period method in J.R. Hicks's *Value and Capital* (1939), see Garegnani (2012).

CONCLUSION

I apologise for the discursive structure of this presentation but I found it impossible to extract

the Cambridge, England, critique of the marginal productivity theory of distribution from the

much wider context in which it was embedded. But, perhaps, such a discursive method and

structure may establish a perspective through which insight and relevance may emerge.

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