Critical Essays on Piero Sraffa's Legacy in Economics

Edited by

HEINZ D. KURZ
University of Graz, Austria

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Revisionist findings on Sraffa

Paul A. Samuelson

Piero Sraffa was a great economist whom I remember with warm admiration. He wrote too little, which is our loss. His reputation tends to get tied up with ideological jockeyings within our profession. Perhaps this is inevitable but I regret it – for, ideology aside, mainstream economists of the mathematical or literary persuasion can benefit much from Sraffa's contributions and also from the problems that his works pose for further investigations.

By chance, the *New Palgrave* (Eatwell, Milgate and Newman, 1987) contains two articles on Sraffa, both of some length. The one by me originated accidentally: I was not the editors' natural choice for this topic, but they wanted my participation and were willing to indulge my preferences to write out some views on Wicksell and also on Sraffa. The other *Palgrave* article on Sraffa, by John Eatwell and Carlo Panico, is seen to have some overlap with mine; still readers of this valuable new reference will, I daresay, benefit from the differences in viewpoint registered by the different authors. (It should be said that, until the finished volumes appeared, I could not benefit from the thoughtful Eatwell–Panico treatment; that is perhaps all for the better since differences can be more interesting than agreements.)

Here, also by invitation of hospitable editors, are some further thoughts on Sraffa. They are neither listed in strict chronological order nor ranked by relative importance.

1   Scholar as young man

One yearns to know more of Sraffa's early Italian years. Schumpeter, in his graduate lectures at Harvard in the mid-1930s, referred to Sraffa as unfortunately spoiled by having been born too affluent. No doubt

Schumpeter was expressing regret that Sraffa did not write more, but this diagnosis as to cause for that seems quite unwarranted. (Rumour has it that Sraffa did die well off, leaving behind him gold in Swiss vaults for Trinity College. According to Kaldor's obituary piece on Sraffa for the British Academy, this legacy seems to have been the fruit of a daring coup in which Sraffa staked his all on the comeback of Japanese bonds after that country's defeat. What theory of inductive inference, I wonder, could have persuaded me to make a like investment?)

2   Writer's block

The myth of Sraffa as dilettante gained from such stories as I heard in the 1930s (perhaps from Robert Bryce, the Canadian John the Baptist who brought Keynes' message to America in 1935). Sraffa is supposed to have begged off being named director of research at Cambridge when he learned this might require his attending an occasional before-noon meeting; as assistant director he could avoid that risk, and for many years the 'graduate' curriculum at Cambridge was reported to involve attending a seminar with other non-Englishmen at which Sraffa presided while classmates presented papers in turn.

Several times I have head the following sample of Keynes' wit. When Maynard was told that a mysterious ailment of Nicky Kaldor's was diagnosed as athlete's foot, he is reported as saying: 'I don't believe it. Next you'll be telling me Piero suffers from writer's cramp.'

Sraffa's block against lecturing was even more pathetic than his writer's block. The story used to be repeated in Cambridge of a series of lectures Professor Pigou had arranged on great economists. Sraffa of course was to speak on Ricardo. But as the day approached, he could not face the ordeal, pacifying Pigou by a gift of gooseberry jam and the promised substitution of Kaldor, who cheerfully agreed to volunteer as substitute. Posterity is grateful to the incident for Kaldor's brilliant 1955 article in which Ricardo's model is artfully presented as one alternative to neoclassical Clarkianism. (Since writing these words I have read the 1984 Italian interview with Kaldor – Christina Marcuzzo, 1986 – where a definitive version of this incident is told.)

3   Thursday conversations at Trinity

One yearns to know more about Sraffa's precise influences on Wittgenstein.

4   The spectacles one peers through

Sraffa's relationship to the Communist Party and to various Marxian factions, whatever its biographical interest and testimony for Italian intellectual history, has little bearing on his scholarly economics. Sraffa's general interest in Marxian economics is quite another matter. He was 50 when I first knew him; and the puzzlement this sophisticated intellectual engendered in me by orally defending such a notion as Smith's concept of *productive* labour (whereby concrete *goods* are given a primacy over ephemeral *services*) suddenly evaporated when I came to hypothesize that this sophisticated mind had a penchant for Marxian notions. This paradigmatic insight for understanding Sraffa served the observer well.

5   Monetary insights

One should constantly nominate suspicions in economics. Marx's legacy is valuable in this regard. But of course nomination and election are quite distinguishable procedures in an empirical science, and in this regard the algebraic worth of Karl's value-added is problematic.

Eatwell and Panico (1987, pp. 448–9) point out that Sraffa's early monetary writing – his 1920 Italian thesis and 1922 work for Keynes – sided with the angels (Cassel, Keynes and Hawtrey, as against Wicksell and Einaudi) in opposing a return to pre-war gold parities with the entailed deadweight losses from deflation. So far, so good. But one hopes that the following paraphrase of Sraffa's 1920 thought is taken out of context: '... the *normal* value of a currency is completely "conventional", i.e. it can be at *any* level that common opinion expects it to be.' Chancellor Churchill made his 1925 boo-boo in part because the current ephemeral belief that the pound would be restored to its 1914 value made it float *temporarily* and misleadingly near to that disastrous level of overvaluation. Of course, Wicksell was wrong and Cassel was right on post-war restorations of parity. *Any* absolute price level can obtain (if the right things are done to sustain it); but Couéism is silly monetary economics.

Also, Sraffa's nominated suspicion that class interests shape price-level policy decisions and achieve significant alterations in income distribution fares poorly under the test of economic history. To be sure, the industrialist Hugo Stinnes was splashed in the 1920–3 inflation and Heidelberg University was permanently penalized. But, as Schumpeter used to say, the rooms of capitalism's hotel are fully occupied, albeit, with a *changing* set of people. Even Keynes' eclectic view of the 1923 *Tract*, that long

waves in the price level generate systematic changes in the terms of trade between risk-taking entrepreneurs on the one hand and rentiers on the other, has had to be attenuated in this century. What is surprising is not how much *hysteresis* there is of the type Sraffa suspected, but rather how little! One capitalist gains when waves move sand to his Florida beach; another loses. But this speaks little for a Marxian model of oceanography.

6   Reproduction models of Quesnay–Marx

Sraffa's 1960 input/output economics, like that of Leontief in the 1925–39 years (and that of Adolph Löwe and Fritz Burchardt in Kiel around 1930), one had supposed was influenced by Marx's Volume II tableaux of steady and expanded reproduction. It is useful to learn from Eatwell and Panico (1987) that Marx's reproduction schemes were indeed the source of the initial notes that Sraffa showed to Keynes around 1927 and from which the 1960 Sraffa classic evolved. There seems no evidence that Sraffa knew the related 1898 works of the Russian V. K. Dmitriev; although von Bortkiewicz was Leontief's Berlin mentor, he tells me he knew Marx's work but not Dmitriev's. For the Metzler *Festschrift* (Samuelson, 1974b), eschewing the puffery in which Morishima (1973) declared Marx to be the peer of Walras as *mathematical* (!) economist, I pronounced Marx's most important *analytical* contribution to economics to be those reproduction models from Volume II, which gets least read.

Marx's admiration (merited admiration) for Quesnay stemmed from his own struggle with reproduction tableaux. Marx's successful struggle – which was purely arithmetic rather than mathematical and which avoided groping (successfully or otherwise) with the so-called 'transformation' problem – was motivated by Marx's erroneous belief that Adam Smith cheated in claiming to break down a good's price and a society's national income into the eclectic triad of wage component, land-rent component and interest (or, sans uncertainty, profit) component. Marx suspected that a *fourth* component of used-up capital goods somehow escaped appropriate inclusion: in a world where iron needs coal input and coal needs iron input – indeed where corn needs as input corn itself – Marx suspected Smith of perpetrating on his reader a value-added calculus that involved the swindle of an *infinite* regress. In tens of thousands of words and repeated MSS bequeathed to Engels, Marx grappled with this per-plexity – which has naught to do with a category of income that involves 'surplus' since it arises in a zero interest, zero rent model.

Paradoxically, Marx's successful depiction of tableaux of *stationary reproduction* and (geometrically) *expanding reproduction* does vindicate

Smith's triad in precisely the manner that Dmitriev ([1898] 1974), Leontief (1941), Dorfman, Samuelson and Solow ([1958] 1987), and Sraffa (1960) were to elaborate on. An infinite time-receding matrix multiplier series converges identically to the Dmitriev–Marx simultaneous equation solution to the problem when Marx carries forward at compound interest the earlier-stage factor outlays up to the final date of the present: $\sum_0^\infty a^t(1 + r)^t \equiv [I - a(1 + r)]^{-1}$ in matrix terms, post-multiplied into direct primary factor requirements.[1] The following numerical example elucidates the theorem.

Consider a model with two goods: (subsistence) corn and (luxury) silk. To produce 1 of corn requires 1 acre of direct land and $\frac{1}{2}$ unit of the other good as input; to get 1 of silk requires 1 of direct labour and $\frac{1}{2}$ of the other good as input. Each labourer needs a *subsistence* wage of $\frac{1}{4}$ corn, paid at the beginning of the period, to keep the labour supply stationary. The profit rate is observed to be $33\frac{1}{3}$ per cent per period. Like workers, property owners save nothing, spending all their rent and interest incomes on silk consumption. Suppose the wage is £1 per unit and the rent is £1 per acre (both paid *pre factum*). It can be calculated that, by coincidence, both goods have a competitive price of £4 per unit. Only one steady-state reproduction tableau is consistent with our specified data of 200 workers and 150 units of land. The tableau of Marx and bourgeois economics must be as in Table 1.

Marx's calculated tableau gives the same tripartite breakdown of income and price(s) as Smith will get from (i) adding up the value-added in all the (infinite) rounds of previous stages of production, taking care (ii) to reckon the compound interest earnable on all the earlier outlays on the primary inputs from time of purchase to the present day of price reckoning. All this is a routine exercise in Dorfman–Samuelson–Solow ([1958] 1987) and Sraffa (1960) algebra: with W and R being the nominal wage and rent rates, r the profit rate, C_j's the final consumptions,

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[1] My explorations in Marx's development do not fully concur with Garegnani's. There is no page on which Marx perceives how to solve the matrix *Mehrwert* relations $p = a_0(1 + m) + pa = (1 + m)a_0[I - a]^{-1}$. Such a mode of solving therefore cannot be a springboard to a successful solution of the Sraffa relations $P = a_0(1 + r) + Pa(1 + r) = a_0(1 + r)[I - a(1 + r)]^{-1}$. I hail Marx's numerical tableaux of Volume II, but find it odd that my matter-of-fact erase-and-replace explication of the transformation problem should be considered by Professor Garegnani as being in some sense rebutted by the Sraffa (1951) exposition of what I called in the last section Ricardo's lost-Atlantis 1815 paradigm. The 1815 fabrications are what we get by *not* making any detours or mistakes; the price ratio changes induced in the 1815 model by demand–taste changes are precisely what the *Mehrwert* model mistakenly puts at zero. It is a case of 1960 Sraffa against 1867 Marx.

Table 1. *Tableau of stationary reproduction (profit rate $= 33\frac{1}{3}\%$)*

Departments	Corn input	Silk input	Labour (wages)	Land (rent)	Profit	Gross outputs	Final outputs
Corn	£0	£300	£0	£150	£150	£600	£200
(150 units @ £4)							= £(600 − 400)
Silk	£400	£0	£200	£0	£200	£800	£500
(200 units @ £4)							= £(800 − 300)
	£400	£300	£200	£150	£350		£700

£(200 + 150 + 350) = national income = £(200 + 500)
wages + rent + interest = *NI* $= \Sigma P_j C_j$

and P_j's the goods' nominal prices, $\sum P_j C_j = (W[0\ 1] + R[1\ 0])$ $[I + a(1+r) + a^2(1+r)^2 + \ldots]$. Note that this contrived case of equal organic composition of capital carefully avoids any detour into distorting equalized rates of surplus values (percentage mark-ups on *wage outlay alone* to the neglect of return on raw material and/or rent outlays). No wonder Steedman regarded *Marx after Sraffa* (1977) as properly being where Smith was *before* Marx. Bortkiewicz would concur.

7   Macroeconomic skirmishing

My meandering thoughts regress from 1960 to Sraffa's 1932 polemic on Hayek's *Prices and Production* (1931). After a brief period in vogue, in accordance with Oscar Wilde's dictum that 'these days to be understood is to be found out', Hayek's book has earned its benign neglect. As both of the cited *Palgrave* articles aptly pointed out, Sraffa's 1932 article is notable for his defining the concept of an *own-rate of interest in terms of a specified good*. If 100 rice today trade for 106 rice next year, the own-rice-rate of interest is 6 per cent. If the market basket of goods in the price-level index rises in price by 2 per cent in the year and my dollar earns a nominal interest rate of 10 per cent, my real rate (own-rate in terms of the goods basket) is only 8 per cent – 10−2 per cent or, more exactly, $[(110/1.02) - 100]\% = 7.84\ldots\%$. In the literature on Irving Fisher and Keynes, the own-rate concept lives on purged of some of its *General Theory* ambiguities.

I reread the 1932 item for my *Palgrave* piece but scarcity of space accorded it only perfunctory mention. Let me report here that the allegedly pen-tied scholar, when urged by Keynes to attack Hayek, displayed

a brilliant and cutting argumentative style. However, now that the heats of the arguments have cooled down after half a century, the modern reader perceives that Sraffa did not identify the rotten core of the Hayek work but engaged mostly in formalistic word-play concerning such Jesuitical concepts as neutrality of money. Had Sraffa worked the other side of the street, his subtle mind could have formulated for Hayek impeccable descriptions of processes in which, for a time, interest rates are contrived to be lower in real terms than they can remain in the longer run. What was harmful in the Hayek lectures was not their failure to achieve Wittgenstein rigour in formulating their scenarios!

I heard David Laidler in Sweden, during the 1987 meeting celebrating the fiftieth anniversary of the so-called Stockholm School, speculate on why Keynes' *General Theory* had the lasting power that *Prices and Production* and the Ohlin–Myrdal–Lindahl–Lundberg paradigms of the Stockholm School failed to attain. Having lived through those years of yore, I could have told him there was no mystery: the Swedish School messages and paradigms overlapped with, and were dominated by, the post-*General Theory investigations*. Those of them that deserved to live on did and do live on (for example, the Lundberg-like dynamizing of the 1936 statics). The Hayek effort died because, in the midst of a great depression that almost ended capitalism and led down the road to serfdom, it concentrated on parables in which a system must for methodological reasons be started out at full employment and in which the induced shortening of the period of production was non-cogently identified with the depression process then in being. As Lionel Robbins came to regret in reflecting on his misguided *The Great Depression* (1936), every policy insight of the 1931 opus was perverse: with one in four unemployed in Germany and the United States, to regard any expansion of funds for consumption as an evil sin was a programme inviting rejection by good-sense lay people and economists. (Even Hayek had second thoughts.) What died deserved to die. Still, schemata that illuminate how policies that promote over-full employment will entail predictable reactions will Phoenix-like rise from the dead in other times. Science, one hopes, can do better than reel from one non-eclectic exaggeration to another.

8   The fatal 1926 error

The two *Palgrave* articles are diametrically different in their evaluation of the 1926 article on competitive returns that brought Sraffa early fame. Both of course agree with his critique of Marshall's attempt to paper over the incompatibility of a firm's falling marginal cost with perfect competition.

Helping 1920s Cambridge catch up to 1838 Cournot was a valuable, needed, Sraffian contribution, which did stimulate Joan Robinson and Kahn to those imperfect competition advances that paralleled the contemporary American work of J. M. Clark and Chamberlin. No argument on this.

But on the other half of the 1926 classic – Sraffa's purported demonstration that the category of *constant* competitive cost constitutes the only empirical box with appreciable content – the *Palgrave* articles are 180° apart. I state (Samuelson, 1987a, pp. 458–9). 'This is plain wrong. Sraffa's 1960 book demonstrates that... [as does] Joan Robinson's famous 1941 *Economica* article on rising supply price....' As soon as two competitive goods involve different land/labour proportions, the *production possibility* frontier is curved and not straight in the fashion Sraffa needs. A *Palgrave* editor (not Eatwell) in a letter to me suggested in effect that for small (enough) movements the curved frontier would look flat. When I replied that this is an evasion of the question of whether the second derivative is itself zero (or even 'near zero'), he responded by saying that Sraffa was not a trained mathematician – which agrees with my point that Sraffa by pure rhetoric convinced himself and my generation of students of a simple error. I reproach myself that, for a dozen years, I was taken in and passed on to students defective reasoning and conclusions. When I reread the 1926 article with a magnifying glass, I perceived it to be blue smoke: Sraffa does not even purport to provide a cogent proof of anything – by suggestion, and implicit appeal to what is legitimate in (Marshallian) *partial* equilibrium methodology, the cases where alterations in composition of demand alter competitive price ratios are minimized. This is not even good Ricardo! Ricardo never expected Bordeaux wine (*vin ordinaire* or select Chateau) to have a price independent of quantity; and he expected a shift in demand toward goods of high labour intensity (soldiers' services) to raise labour's distributive shares. Eatwell and Panico (1987, p. 448) quote without blushing Sraffa's innocent proposal that, since unit costs of production are (sic) 'constant in respect to small variations in quantity produced,... the old and new obsolete theory which makes it [price, under variations in demand] dependent on the cost of production alone appears to hold its ground as the best available.' The issue is not whether small changes in an independent variable induce small changes in a dependent variable, but rather whether the instantaneous derivative of the function is negligibly small. The economics profession, while saluting Sraffa (1926) as a classic, has resolutely ignored its novel supply-curve findings.

To underline that my criticisms are not captious, and that the exceptions to Sraffa's claims cannot be discounted as belonging to some

'peculiar' concatenation of industry and firm 'externalities–internalities', I here present an impeccable Marshallian model in which (a) each of n goods is produced by transferable labour and a specialized land specific to itself, (b) every person's demand function for each of the n goods is strictly independent of every other good's price or quantity (strongly additive independent utilities), (c) for every person the marginal disutility of labour is a strict constant ('objectively' identifiable from market data). The example glaringly contradicts Sraffa's constancy of costs and obeys *all partial* equilibrium requirements (at the same time that it is a *full* general equilibrium model, a congruence Alfred Marshall never quite achieved). See Samuelson (1971b), for more on such a rigorous partial equilibrium as applied to trade theory. Moreover, in the sense of the mathematicians Smale and Thom, the example's properties are *generic* not singular, persisting when industries use the *same* inputs in varying proportions. At the same time that every firm can be regarded as a price taker, a systematic shift in the composition of demand towards some goods and away from others will systematically raise the market terms of trade of the newly preferred items. The 1926 attempt to fob off on the twentieth century a value taxonomy that was already obsolete in the early nineteenth century can hardly be termed a brilliant failure.

9   Demand effects when production is joint

Smith, Longfield, Cournot, Thünen, Mangoldt and Marshall well understood that joint production makes price ratios depend on the composition of demand (even, I may add, in a timeless technology where labour is the sole factor of production). A new taste for mutton lowers the relative price of wool, etc. In 1926 Sraffa never brought these banalities into doubt.

It is a pity therefore that Eatwell and Panico (1987, pp. 449–50) glean the impression from Part II of Sraffa (1960) that the influence of demand composition on relative prices is absent in Part II in the same way that it can be absent from Part I's model involving labour as the sole factor of production and no jointness of production. (See their exact sentence bridging pages 449 and 450 for explicit utterance of their untruth.) Although Sraffa's 1960 treatment of joint production (1) is quite fragmentary, (2) involves a number of non-optimalities and a few errors, and (3) contemplates *special equalities* cases that bias the reader towards overlooking demand influences, Sraffa, as far as I can remember, never explicitly claims the absence of demand influences.

Let me spell out a few examples that negate the Eatwell–Panico contention. To get 1 corn suppose we need either 5 of labour and 1 of land or 2 of labour and 2 of land. (The reader is licensed in all cases to add the requirement that 1 of corn also requires 1/10 of corn as seed, thereby qualifying corn as a bona fide *basic* good.) Assume the interest rate is always zero. (The reader may put in any other constant for the interest rate that is less than 900 per cent per period.) Assume 1 of cloth is producible out of 1 of labour and 1 of corn. Assume steady-state fixed supply of land; and, for the supply of labour, assume either that it also is fixed or that its supply adjusts to fulfil a subsistence real wage in terms of corn.

This is a joint-production model. Land being permanently durable, our 5 of labour and 1 of land can be thought of as producing along with 1 of corn also 1 of land itself. Etc.

Now we must ask whether the terms of trade between corn and cloth are the same when property owners want to consume those goods in 100-to-1 proportions as in 1-to-100 proportions. The answer is strongly in the negative. It is no defence of error to say that the example violates Sraffa's frequent restriction to equality between the number of goods (three for corn, cloth and land) and the number of processes used (three or two depending upon the exact composition of final demands). On some pages of Sraffa (1960, Part II), moreover, that restriction would be self-contradictory.

A less obvious, and therefore more telling example of the narrowness of Part II's treatment of joint production is the following case: 1 of labour and 1 of corn produces 3 of wool and 1 of mutton; alternatively, 1 of labour and 1 of corn can produce 1 of wool and 3 of mutton. 1 of labour and 1/10 of corn produces 1 of corn. (Remark: instead of wool and mutton, the example can be made to involve machines and raw materials.) Sraffa observes a system in being, with three goods being produced by the three feasible processes: the terms of trade twixt mutton and wool, we see from symmetry without boring calculation, is 1-to-1 when final demands for wool and mutton are in exact balance.

Now shift final demand towards a bit more of wool and a bit less of mutton. What happens to P_{wool}/P_{mutton}? Nothing. It remains at 1/1. Eatwell and Panico are apparently (in *this* case) right? No. Only along a facet of *limited* quantitative magnitude does Sraffa contrive a horizontal, so-called *classical* supply curve ss'. Let the final demand for wool move to more than three times the final demand for mutton and the system will *endogenously* cease to use the process that produces relatively much of mutton. Sraffa *endogenously* loses the quality of number of goods and number of processes he provisionally specified initially.

What causes the loss? Shifts in composition of demand, the influence Eatwell and Panico thought to deny.[2]

Can counsel for Sraffa and Eatwell and Panico not find a joint-production case in which demand shifts are powerless to alter cost ratios? Yes, I can contrive a weak jointness case in which a generalization of the no-joint-product 1949 non-substitution theorem does obtain. Let the number of goods and feasible processes by specified as equal (already an arbitrary narrowing of reality). Denote by $[a_{ij}]$ the usual *input* coefficient matrix, in this case specified to be positive and square and obeying familiar Hawkins–Simon conditions for being 'productive'. Write the von Neumann *output* coefficient matrix as $[b_{ij}]$, also square and positive in this case.

Now restrict the $[b_{ij}]$ coefficients by requiring that the off-diagonal coefficients all be 'near' to zero; restrict the diagonal terms so that they are all near to the same positive constant, which might as well be unity. In effect, I am saving the day by weakening the degree of jointness of production as near to the vanishing point as will save the face of the argument.

In this contrived case of *diagonal dominance*, the following inverse matrix exists and has all positive coefficients: $[b_{ij} - a_{ij}]^{-1} = [A_{ij}]$. Then, provided the interest rate is sufficiently near to zero, we can be sure that no shift in (positive) *final* demands can induce a shift out of Sraffa's initial regimen of real individual goods costs calculable *from technology alone*.

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[2] Some continental mathematicians contemplate models in which my two processes are replaced by N processes for wool-or-mutton production: thus, add to $(3, 1)$ and $(1, 3)$ the new processes $(3/4, 25/8)$ and $(1/2, 26/8)$. Begin say with balanced final demand, so that both $(3, 1)$ and $(1, 3)$ are initially used equally. 'See,' they say, 'Sraffa has two goods (wool and mutton) and two processes used. After Samuelson alters final demand considerably toward wool, he may induce a competitive shift in the use of processes so that $(1, 3)$ and $(3/4, 25/8)$ are solely used: *still*, equality of number of goods and processes is *generically* implied; $2 = 2$. So it goes when the yen for wool goes up further. Sraffa is vindicated.' No. Only the face of the argument is saved. And saved but temporarily. Already demand shifts do *endogenously* shift terms of trade – in negation of 'classical' constancy. Furthermore, with the number of von Neumann activities specified to be finite, for large enough specified taste changes, eventually $2 = 2$ is replaced by $2 > 1$. Furthermore, suppose we choose to measure what is 'generic' by a metric that (say) asks what happens when all people choose to spend the fraction of income c on wool and $(1 - c)$ on mutton. Then the borderline point at which one of the processes begins to be substituted for another does not correspond to an interval that is of mathematical measure zero. So, after all, $2 > 1$ is *not un*generic looked at in this possible and relevant way!

The example is beautifully contrived to show how limited, not how universal, the costs alone approach to economics must be. (Added in light of Schefold's Comment: There are also cases where $[b - a]^{-1}$ is positive even though b is not diagonally dominant for any choice of units.)

10   The irrelevance of Sraffa's 'standard commodity'

Most sceptics of the microeconomics dominant from Smith and Walras to Debreu (1959) and von Neumann (1935–6), understandably, are not virtuosi of matrix inequalities. It is therefore a natural comfort to them to be told that Sraffa (1960) establishes the concept of a *standard* (or reference) basket of commodities. This helps legitimize Ricardo's hankerings for a labour theory of value, and illuminates Marx's parable concerning how competition exploits workers by making each of them work A hours of the day for themselves and B hours for the capitalists, with the B/A *rate of surplus value* (*Mehrwert*) somehow 'revealing' the source of profit and its laws of motion under capitalism.

Alas, this too is blue smoke. No substantive deficiency of the labour theory of value is ameliorated by the *standard* commodity concept pioneered in Sraffa (1960). Eatwell and Panico (1987, p. 450) advance no bold claim for the *standard* commodity, admitting its use is redundant once market competition has established the simultaneous equations of equilibrium, conceding that the concept may be too complex, but concluding lamely that 'It has the virtue, however, of being analytically correct'. The 'it' that is correct is not an existent 'revealer' or 'clarifier'. What is correct is only that a square, non-negative indecomposable matrix possesses a unique right-hand characteristic vector whose elements can be used as weights to form a market basket of goods, in terms of which there is defined a (*post factum*) real wage that has a linear trade-off with the profit rate over any interval of the latter where the matrix in question provides the competitively viable technology for a no-joint-product system. No word in this last sentence is omittable. And from that valid sentence no comfort for the labour theory of value can be gleaned – as I shall demonstrate.

Eatwell (1987) pens a separate *Palgrave* item on the *standard* commodity, calling it an illustrating and clarifying ancillary device, a *Hilfskonstruktion*. My exposition will explicate why I think it can as aptly be called a *roter* herring.

Now to spell out why the concept of the standard commodity is useless to ameliorate the faults of a labour theory of value or to reveal the essence of labour exploitation. The labour theory of value, let it be agreed, is faultless when profits and land scarcity are ignorable. Smith

knew that and was too shrewd to try to explain labour exploitation by a model whose correctness comes only when there is no exploitation.

The faults of the labour theory of value include the following:

A. *A change in the composition of demand, towards or away from 'relatively labour–land intensive' goods, in real life alters goods' relative prices.* (Sraffa's failure to single this out for notice is inexplicable: his own 1960 Chapter 8 negates his acceptance of Ricardo's belief that the complications of land rent for relative price can be finessed by concentrating on goods produced at the external margin of no-rent land. Where that margin falls is affected *endogenously* by the composition of demand – as many of Ricardo's own paragraphs reveal.)

Even in the Santa Claus case where Sraffa can define a single standard commodity, this fault of the labour theory is *not in the least bit* touched by any use of the standard commodity, QED.

B. The 1867 labour theory of value (cum exploitative mark-ups proportional to wages) fails to analyse how the distribution of income – the relative fractional shares of wages, rents, interest – is dependent on the *composition* of demand. Even when all lands are redundant and all rents are zero, and when the profit rate and the subsistence wage vector are somehow frozen, a shift in the composition of capitalists' demands can be expected to alter the profit/wages distributive share. The *standard* commodity is powerless to paper over all these shortcomings. (One exception: in the singular case of equal organic compositions of capital, all price ratios of goods *happen* to agree with ratios of embodied labour contents; relative factor shares *happen* to be invariant to demand shifts; the rate of surplus value *happens* then to be uniform over industries as a consequence of the empirical uniformity of the profit rate; but, on this razor's edge, the transformation problem evaporates and no scientific novelty is introduced by the *Mehrwert* treatment of surplus. Remark: impeccable Sraffa–Leontief steady-state analyses with the interest rate somehow specified *also* underemphasize the role that *supply* of capital goods can play in determining factors' distributive shares.)

C. To determine which technical innovations will be competitively viable, the labour theory of value and the 1867 Marxian paradigm of equalized rates of surplus value provide incorrect policy advice and incorrect predictions. After innovation there are definable as many *different* standard commodities as there are square sub-matrices viably choosable from a rectangular matrix. Now, generally, *the system* has no standard commodity: if the concept were able to help Ricardo's defective labour theory – which it cannot do – it would be helpful only in the unrealistic case where at *all* rates the *same* indecomposable set of techniques were competitively viable. Eatwell's *Palgrave* article on the standard commodity (Eatwell,

1987) needs to be augmented by demonstration of how *limited* the economic domain is in which a *standard* commodity can be defined.

Thus joint-product systems generally do *not* possess a *standard* commodity. Neither do systems that have any viable technologies different from their golden rule zero-interest-rate technology. Nor do simple Ricardo systems that are decomposable (labour hunts deer and labour hunts beaver). If a defence of the labour theory were a valid one, how could it self-destruct when certain input coefficients change from epsilon to zero or when certain characteristic vectors become complex numbers? The *standard* commodity is thus seen to be no saviour of a defective labour theory of value. QED.

To prove my various QED's, the reader need only contemplate one example. 1/10 of wheat and 1 of labour (or it could be 1 of land) produce 1 of wheat. 1 of wheat and 1 of labour produce 1 of bread. We need no matrix algebra to identify Sraffa's *standard* commodity as a basket containing only wheat. Every defect in the labour theory of value remains and cannot be erased by any use of the standard concept. The importance of the concept is especially evident in the plausible case where the subsistence wage involves bread, which is not even in Sraffa's market basket!

For later reference, the reader may contemplate an invention that can produce 1 of bread out of 1/6 of labour, 1/2 of bread, and 1 of wheat. The labour theory of value cannot tell us, what is true, namely that only at interest rates below a critical one will this invention be a viable one. Soon I shall show that this non-esoteric situation refutes the Ricardo–Stigler contention that the defective labour theory of value is 'at least 93 per cent accurate'.

D. Present-day sophisticated defenders of Marx's paradigms of the labour theory of value *à la* equalized rates of surplus value admit that the 1867 result differs substantively from competitive reality. But they hope to make the case that, somehow, Marx's approximation yields deep *macro*economic insights and 'understanding'. A patient audit of every different such claim, I and members of my MIT seminars have found, leads only to disappointment. Here is but one sample, a famous case in the post-1867 literature.

Marx refers often to an alleged 'law of the falling rate of profit'. Abstractly put, his attempted *deduction* of this law involves the following factorization of the rate of profit

$$r = \text{total profits/total capital}$$

$$= (\text{profits/wages}) (\text{wages/total capital}) = A \times B$$

where total capital is the sum of wages advanced plus non-labour items of capital (called 'constant capital'). The thoughtful reader will note that, if this approach is fruitful, it certainly does not presuppose the validity of the odd 1867 hypothesis that labour-intensive industries have the same (profits/wage) ratio that other industries do. The purpose of rehashing this ancient issue here is to show how Sraffa's 1960 paradigm brings into doubt the fruitfulness of Marx's factorization.

Marx's key notion is that modern times usher in a 'rise in the organic composition of capital', which is interpretable as a decline in the second factor of wages/(wages + constant capital), B. Then, provided we can presume that the first factor of 'exploitation' – profits/wages or A – stays the same, we can deduce that the product – the rate of profit – of the two factors falls.

Let us test the cogency of the crucial *ceteris paribus* clause that Marx invokes almost without realizing what is involved. Sraffian analysis will be our measuring rod. Different cases need analysing.

(i) The class struggle raises the wage rate in a fixed-coefficient Sraffa model that lacks land and joint products. In terms of Sraffa's input/output matrix a and his row vector of direct labour requirements $a_0 = [a_{01}, \ldots, a_{0n}] = [a_{0j}]$, Marx's factorization involves a monstrously complicated and ambiguous expression. For this thought experiment, as the rate of profit falls from its Sraffian maximum to zero, the factor A does not stay constant in the fashion Marx needs but rather falls monotonically!

(ii) Suppose the interest rate r is falling because of some possible 'deepening of capital' in the Clark–Solow fashion. Should Marx be given credit for an 1867 anticipation and proof of this process and its effects on the profit rate?

Hardly. Only along the singular Cobb–Douglas razor's edge does his fixed A, falling B story apply. For the case where the elasticity of substitution is greater than unity, Marx's A rises as B falls; for the more realistic case where the elasticity of substitution is fractional, Marx's A falls.

(iii) The case of exogenous technical changes is what Marx and most of us think is the most interesting one. By 1867 unsophisticated observers formed the strong impression that innovation and scientific advance had somehow enlarged the role of non-labour inputs relative to direct labour. An enhanced organic composition of capital due to technical change seemed the natural way to describe this – a greater role for dead labour at the expense of live labour would be the Pickwickian Marxian description of this. (Paradoxically, one of Sraffa's greatest contributions was to demonstrate that it is, in general, *not* possible to associate lower interest

rates with 'more roundabout', 'more mechanized', more time-consuming modes of production, a finding as fatal to neoclassical parables as to the notion of recognizable shifts in 'the organic composition of capital'.)

Students of von Neumann, Sraffa and Solow know that technical change can either *raise* competitive profits rates or raise real wage rates (or raise both). No law of the (necessarily) declining rate of profit is factually or deductively true. Even empirical presumptions have not been justified by knowledge of economic history.

The remarkable increase in the system's 'productivity' after the industrial revolution can take place with what Marx calls the organic composition of capital falling rather than rising. Sraffa has taught us to recognize 'Wicksell effects', which can be purely pecuniary effects masquerading as technical changes. Exactly the same thing can be said of 'Marx effects', $C/(C + V)$ changes that occur with the same *technical* coefficients at different profit rates.

Above all, the factorization $A \times B$ kept Marx in darkness about the Sraffa–Samuelson truth concerning a competitive economy sans land and joint products: if the profit rate falls, the real wage cannot fall and create worker immiseration; if the real wage falls for *any* reason, the competitive profit rate must rise. Similar truths concerning the real wage and the 1867 rate of surplus value *cannot* be asserted for a competitive economy.

11   Hollow victory of standard commodity for the transformation problem

Suppose by singular chance that the subsistence goods vector, which each worker's real wage must be able to buy if the labour supply is to be in stationary equilibrium, involves the various goods in exactly the same proportions as are involved in the standard commodity market basket – a monstrous assumption; and suppose that by singular chance all non-workers choose to consume goods in those same proportions. Or, alternatively, assume that by fortuitous cancellation the deviation of workers' subsistence from the standard vector happens to be just opposite in sign and of exactly the same magnitude as the deviation of the capitalists' consumption vector from the standard vector; in consequence their sum, the total consumption vector, is to be exactly proportionate to the standard commodity.

In this admittedly academic case, not expected remotely to be encountered in any real-life situation, we can draw up for Marx a tableau of steady reproduction expressed in correct prices of production terms. Presumably, he could draw up for himself an alternative tableau expressed in his 1867–85 marked-up values, obeying his usual Volume

II conditions of equalized *rates of surplus value (Mehrwert)* and *eschewing any explicit* rate(s) of profit.

To *his* values tableau, Marx could in this specially contrived case apply *his* proposed 1894 Volume III algorithm for the transformation problem. The resulting new tableau – known by us and by him generally *not* to be quite the desired correct tableau expressing the true competitive prices data – happens (in our singular case where the physical consumption vector is exactly proportional to Sraffa's existent standard commodity) to match exactly the correct Smith–Walras–Dmitriev–Bortkiewicz–Seton–Sraffa competitive prices tableau. (Newman, Meek, Burmeister and others have provided proof of this essentially matrix theorem, as described in Eatwell, 1987, pp. 478–80, and mentioned in the *Palgrave* article on the transformation problem – Hunt and Glick, 1987, p. 689 – in connection with the 1972 work of Alfredo Medio.)

This singularity-case defence of Marx is generally recognized to be a hollow defence, belonging to the category of argument: 'If my Aunt Sally had wheels she'd be a stage coach but generally, like most aunts, she's only a biped.'

Equally hollow is Marx's suggested device of concentrating on that particular good whose organic composition happens to be precisely that of the whole system. How does one recognize which of the vast array of industries is at (or really near to, and *stays* near to) the social average? And why think that *its* organic composition ratio is measured in Marxian values, c_j/v_j, rather than C_j/V_j whose capital letters denote the (as yet) unknown cum-profit *prices* magnitudes.

12   Perhaps unintended Sraffian insights concerning Ricardo and Marx: Ricardo

Most admirers of Sraffa, one discovers, believe that his 1960 classic (a) lends comfort to a Ricardian labour theory of value, and (b) provides a measure of justification for Marx's surplus value novelties of 1867. I believe that a careful reading of *Production of Commodities by Means of Commodities* and sympathetic mathematical extensions of it and of von Neumann's general equilibrium activity model leads to the reverse.

(a) Sraffian analysis confirms that Ricardo's model, shorn of all gratuitous approximations, has to be a three-component model of value: along with the well-understood labour component, there is the subtle interest-rate component and also the land-requirement component. The lost-Atlantis model of 1815

Ricardo, beloved by Kaldor, Sraffa and Garegnani (1987), bears out this contention of mine.

(b) Had the author chosen to apply himself, not only to a critique of 'the marginal theory of value and distribution', but also to a critique of Marx's paradigm of an equalized rate of surplus value by industries (*Mehrwert*), Sraffa's analysis could provide a sharp Occam's razor of demolition.

If my contentions are demonstrable, why the popular beliefs to the contrary? I abstain from attempting an explanation. However, at least two contributing factors can be identified. Piero Sraffa himself wrote in approval of aspects of Ricardo and Marx that are deemed dubious by mainstream historians of economic thought who write in the vein of what I call the Whig history of science (Samuelson, 1987b). A second factor is that Sraffa was critical of neoclassical marginalism and many of his readers were led by that to discern merit in classicism and in the Marxian alternative to mainstream economics.

This present revisionist exposition concerning Sraffa eschews all defence of neoclassicism and intends to couch its every basic argument in terms of von Neumann's discrete activities technology. In this I resemble such post-Marxians as Ian Steedman and John Roemer (and, for that matter, Joan Robinson).

Leaving to the next section what is entailed for the Marxian transformation problem by Sraffa (1960), let me here use my scarce time to explicate the failure of Stigler's defence of Ricardo's so-called *93 per cent labour theory of value*.

Stigler (1958) himself agrees with me that Ricardo knew he had to augment the labour component by an interest component to be generally accurate. (For the 1958 date, Stigler is remarkably tolerant toward Ricardo's belief that he could somehow exclude a rent component.) But he quotes Ricardo's numerical example (Ricardo, [1817–21] 1951, p. 36) of goods that differ in their time-roundaboutness, and accepts uncritically Ricardo's inference from the example that the price ratio of two goods could be affected by a feasible change in the interest rate and the wage rate that 'could not exceed 6 or 7 per cent'.

Two years later, after Stigler could have read Sraffa (1960), he could discover that we can pick for Ricardo a non-bizarre numerical example in which a change in the profit rate from 6 per cent to 6.1 per cent could easily alter P_2/P_1 from 1.0 to 10^6 or to 10^{-6}. Moreover, in Sraffa input/output matrices that look entirely plausible, as the interest drops from its von Neumann maximum to zero, a similar price ratio can oscillate by 70 per cent almost as well as by 7 per cent. Ricardo escaped noticing this

because his examples are 'Austrian' rather than 'Leontief'. As soon as outputs require other goods and themselves as inputs, the inverse matrices can become very ill-conditioned indeed.

Here is a horrendous example intended to frighten innocent children. To produce coal 1 period from now takes 1 of labour. To produce 1 of rye takes 9/10 of rye and 1/100 of labour. To produce 1 of corn takes 1/10 of corn and 1 of labour. Ricardo observes a profit rate of 10 per cent per period. Now, for some reason the profit rate rises to 11.1 per cent, surely not a fantastic change. Let Ricardo and Stigler calculate how P_{rye}/P_{corn} rises from unity to 100. Shall we coin for Ricardo a belief in a 1 per cent theory of value? (And what does the other 99 per cent refer to?)

Here is the 1815 lost-Atlantis corn-to-corn model attributed to Ricardo. 1 of corn a period from now requires 1/10 of corn, N of labour, $1/N$ of land, where N can be any of $1, 2, \ldots, 10, 1/2, 1/4$. Cloth requires 1 of labour and 1 of corn. When Garegnani (1987) and I calculate its behaviour under various assumptions about the supply of land, the sub-sistence wage vector, the capitalists' rates of time preference and/or sav-ing propensities, and when we vary the compositions of capitalists' consumption demands, we see that the resulting behaviour are inconsis-tent with any Marxian or Ricardian labour theories of distribution. Its price ratios alter with composition of demand as in Walras and Debreu and Solow. Indeed, it behaves like 1960 Sraffa, which is my point about it *and* about Sraffa's system.

13   How Sraffian analysis debunks proposed Marxian transformation problem solutions

Trojan horses within the Marxian camp are those who get right the economics of competitive price: namely, John Roemer and Ian Steedman, along with Piero Sraffa and Ladislaus von Bortkiewicz. They not only spot what is erroneous in Marx's transformation treatment (Marx, 1894, ch. 9) but perceive that the correct prices solution is achiev-able only by solving the high-degree Sraffian equations (a 15th degree polynomial if the system consists of fifteen Sraffian *basic* goods, etc.). This establishes the fundamental futility theorem: first solving the 1867 values tableau problem involving an irrelevant inversion of a 15-by-15 zero-profit matrix, and then solving a 1st degree subsistence wage equa-tion, gets you *nowhere* on your search for correct competitive profit rate and correct real-price and price-wage ratios. When I played the role of the Hans Christian Andersen child (Samuelson, 1970, 1971a) and pointed out that the Emperor wore no clothes – showing that from Dmitriev and Bortkiewicz, through Sweezy, Winternitz, Dobb, Meek, Seton, and the

rest, the transformation algorithm from 'values' to 'prices' was an 'erase and replace' procedure – no perceptible dent was made on the stable of *Palgrave* writers on kindred topics.

What I call a 'detour' Garegnani (1987, p. 567) calls a 'mistake' by Marx. Garegnani believes that the mistake was a fruitful one because it led Marx (almost) to a correct Sraffian theory of competitive price. Whether this theory of genesis of progress is accepted or rejected, one erases a recognized mistake and desists from perpetuating it.

Lack of time prevents me from applying Occam's razor to various 'new solutions' to the transformation problem – such as that of Duménil, Foley and Lipietz covered in Hunt and Glick (1987, p. 690) or that of Wolff, Roberts and Callari (1982). One puts to them the question 'Cui bono?', and hopes for a better answer than a quotation from Marx of possible relevance. The search for values tableaux that agree in various scalar totals with prices of production tableaux is a sterile search, as Seton (1957) long ago made clear.

14   The non-optimality of unequalized profit rates

Although Sraffa does not investigate the problems of *intertemporal efficiency*, his specification of equalized profit rates – as against, for example, equalized 1867 *Mehrwert* rates – can be shown to be a requirement of intertemporal Pareto optimality under capitalism or socialism. If Nobel Laureate Kantorovich were alive today in the age of Gorbachev, his mathematical proofs of this post-Marxian truth would receive the attention they have always deserved. Any critique of 'marginalism' should not blind us to such truths: that would be a case of throwing out the adult along with the bathwater.

15   Autonomy of truth

The ideas of a great mind transcend their creator. I cannot honestly affirm that Piero Sraffa would happily accept my revisionist findings concerning his brain-children. But why should I sell short the subtlety and flexibility of that noble economist?

Editorial note

Professor Samuelson had been unable to attend the conference but on later request kindly submitted a paper which arrived when the manuscript of the book was going to the press. The paper therefore could not be discussed by all the participants at the conference. The two main Comments and the Reply

here offered all had to be written at short notice. The editors are grateful to John Eatwell for agreeing to submit an additional short Comment although it had to be submitted within less than three weeks because of difficulties in communication on the one hand, and commitments to the publisher on the other. It is hoped that the issues will be taken up in a broader discussion in other publications.

Comment

John Eatwell

CAMBRIDGE

Having had very little time for the preparation of this Comment, I must confine myself to answering Professor Samuelson's specific criticisms of the articles by Carlo Panico and me (Eatwell and Panico, 1987; Eatwell, 1987), leaving comment on his wider excursions to another occasion.[1]

I will deal first with Samuelson's reference to an 'explicit utterance of untruth' by Eatwell and Panico concerning the effects, in joint-production models, of changes in the composition of output. Second, I will examine his criticism of the Eatwell and Panico discussion of Sraffa's 1926 critique of Marshallian partial equilibrium. Third, I will turn to Samuelson's comments on my *Palgrave* entry on the standard commodity.

Prices and demand

Samuelson refers repeatedly to the Eatwell–Panico 'error'. The error is our supposed contention that 'the influence of demand composition on relative prices is absent in Part II [of Sraffa's *Production of Commodities*] in the same way that it can be absent from Part I's model involving labour as the sole factor of production and no jointness of production' (p. 33).

Samuelson cites, as evidence of our error, a single sentence. If this sentence is restored to the passage from which it has been extracted, it

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[1] The short time-span available for this comment precluded consultation with Carlo Panico; hence he is not responsible for my remarks.

will be seen that our argument is totally different from that which Samuelson represents it to be. To avoid ambiguity it is worth quoting our argument in full:

Considerable puzzlement was engendered by Sraffa's statement in the Preface of his book that 'The investigation is concerned exclusively with such properties of an economic system as do not depend on changes in the scale of production . . .' (Sraffa, 1960, p. v). The absence of any reference to demand led unsuspecting readers to equate his results with the non-substitution theorem, and hence with the assumption of constant returns to scale. However, a careful reading of Sraffa's analysis reveals that no knowledge of any relationship between *changes* in outputs and *changes* in inputs, or between price and quantity is *necessary* for the solution of equations, and hence for the determination of the rate of profit and prices (given the wage). This contrasts with neoclassical theory, in which the determination of prices is dependent upon knowledge of the functional relationships between supply and demand. *If, in Sraffa's analysis, quantities should change, then any consequential change in conditions of production will result in changes in prices.*

In Part II of his book, Sraffa extends his analysis to multi-product industries and fixed capital, and to the analysis of economies with more than one non-reproducible input. As might be expected, the analysis is considerably more complex, and in some cases the results less clearcut (the solution of the system may not, for example, be unique, and the definition of basics and non-basics is more abstract than is the case with single-product industries). Yet the basic structure of classical analysis is preserved – the prices, the rate of profit, and other distributive variables (say, land rents) are determined by the conditions of production, given the wage. (Eatwell and Panico, 1987, pp. 450–1)

The final sentence of the first paragraph (emphasized here) was apparently overlooked by Samuelson. Perhaps we should have repeated it after the second paragraph too – it is the final sentence of that paragraph that he cites but does not quote. If he had quoted it, it would be evident to the reader that nowhere in that sentence is here any reference to the 'horizontal, so-called classical supply curve' or to the 'costs-alone approach' which Samuelson attempts to foist on us.

In Sraffa's analysis, any change in the composition of output that leads to a change in the conditions of production, for whatever reason, will change relative prices. There is no 'costs-alone approach'.

Sraffa's critique of the Marshallian supply curve

Samuelson argues that Sraffa's critique of the use of diminishing returns in the construction of the partial equilibrium supply curve is flawed, and that Panico and I replicate the flaws without comment. I will not deal

with Samuelson's example which, interestingly, fulfils exactly the conditions Sraffa laid down for the construction of a partial equilibrium industry supply curve.

Samuelson does not seem to have grasped that Sraffa's critique is exclusively directed at *partial equilibrium* analysis. Producing general equilibrium examples as a critique of Sraffa is an exercise in irrelevance. Moreover, Samuelson fails to take account of Sraffa's own reference to general equilibrium analysis in the 1926 paper. Panico and I make these points (1987, p. 448):

Apart from his contribution in the *Economic Journal* symposium on increasing returns, Sraffa did not participate further in the debate on the Marshallian theory of cost. The reasons are not hard to seek.

First, imperfect competition theory, instead of providing a new, more concrete approach to the analysis of value and distribution, was simply absorbed into neoclassical theory. The fact that imperfectly competitive models do not provide a foundation for a theory of value seemed to enhance the status of partial equilibrium analysis, rather than hasten its rejection; with the competitive theory of value still holding sway at the level of general equilibrium (a neat rationale is provided by Hicks, 1946, pp. 83–4). The survival of the 'U' shaped cost curve as an analytical tool, constructed from the presumption of increasing, then diminishing returns, is in no small part attributable to the longevity provided by models of the imperfectly competitive firm. Nonetheless, the appearance of the 'U' shaped curve in models of the competitive firm, more than sixty years after Sraffa clearly demonstrated the illegitimacy of the construction, is an indication of just how intellectually disreputable theoretical economics can be.

Second, Sraffa's implicit identification of classical and Marxian theory with the notion that competitive value is 'dependent on the cost of production' is clearly wrong, as examination of neoclassical models which take account of 'simultaneous equilibrium in numerous industries' readily demonstrates. Sraffa had deployed general equilibrium reasoning to demolish the theory of the competitive firm and the industry supply curve. Further criticism of neoclassical theory would require consideration of general equilibrium models of value and distribution. And a constructive rehabilitation of classical theory would require a general analysis too. It would require, that is, an analysis of 'the process of diffusion of profits throughout the various stages of production and of the process of forming a normal level of profits throughout all the industries of a country' – the problem Sraffa acknowledged was 'beyond the scope of this article'. ([1926] 1953, p. 197)

An example of dubious use of the 'U'-shaped cost curve may be found in Samuelson and Nordhaus (1985, p. 479), together with the discussion of 'long-run breakeven conditions' on p. 480. A more accurate discussion of equilibrium of the competitive firm may be found in Samuelson (1947, pp. 75–80).

The standard commodity

Samuelson criticizes my reference (Eatwell, 1987, p. 476) to the standard commodity as a *Hilfskonstruktion*, i.e. a mathematical technique that aids understanding, that clarifies. He then proceeds to 'spell out why the concept of the standard commodity is useless to ameliorate the faults of the labour theory of value or to reveal the essence of labour exploitation' (p. 36). Since neither the labour theory of value nor exploitation are mentioned in my article at all, it is difficult for me to comment on Samuelson's construction.

Whether use of the physical analogue implicit in the standard commodity clarifies the determination of the rate of profit by the simultaneous solution of *n* equations in a classical system is, I suppose, a matter of taste. But Samuelson's hostility toward the construction of a *Hilfskonstruktion* is a little odd, when his textbook (Samuelson and Nordhaus, 1985) is littered with simple supply and demand diagrams, which he must know are, in a general equilibrium setting, typically false representations of the characteristics of a market. Why doesn't he draw correspondences? Perhaps they would not 'clarify' the argument?

Conclusion

In his introduction, Samuelson refers to 'ideological jockeyings' within our profession.

Ideological messages are often conveyed by a careful use of language. Hence Panico and I do not simply err, we 'quote without blushing', and indulge in 'explicit utterance of untruth'.

Another powerful ideological device is to remove sentences from their context, to misrepresent arguments, or to erect straw men. Samuelson's paper provides examples of all three of these techniques.

Comment

Pierangelo Garegnani
ROME

1. Professor Samuelson's contribution to the present volume is an excellent example of the difficulty that a present-day theoretical economist has

in understanding Sraffa's theoretical enterprise. The theoretical approach of Smith and Ricardo had been so totally 'submerged' (Sraffa, 1960, p. v) – as opposed to criticized and found wanting – that its reappearance in Sraffa's terse lines of his introduction to Ricardo's *Works* (Sraffa, 1951), and in the rigorous modern presentation of *Production of Commodities* (Sraffa, 1960), was bound to raise considerable difficulties of comprehension. And the fact that the approach adopted by Sraffa had been developing for over a century before Ricardo raises, if anything, additional difficulties. The analysis reappears as a comparatively finished product while the process of its development, which is important to its understanding, has been forgotten or goes unrecognized. Ironically, the difficulties would have been less in the case of a completely new theoretical departure, the germs of which would lie in a literature generally known and understood.

Questions of deadlines will prevent me from going as much as I would like into the issues that Professor Samuelson touches with respect both to pure analysis and to interpretation of the classical economists.

1   The '1926 error'

2. In his section on 'the fatal 1926 error', Samuelson attributes to Sraffa's *Economic Journal* article of that date the idea that 'the category of *constant* competitive cost constitutes the only empirical box with appreciable content', and comments:

This is plain wrong...as soon as two competitive goods involve different land/labour proportions, the *production possibility* frontier is curved and not straight in the fashion Sraffa needs. (p. 32)

The idea that Samuelson attributes here to Sraffa is not however the one we find in the article of 1926. There is a subtle but all-important difference. Sraffa's argument was not that the category of constant competitive cost constitutes the only box with empirical content. It was that *among the boxes that the conditions of partial equilibrium allow us to consider*,[1] constant costs constitutes the only box with some content.

As Sraffa himself clearly states at the conclusion of his argument, in the passage quoted (only partially) in the Eatwell–Panico essay (1987, p. 448) to which Samuelson refers,

[1] Sraffa's expression is 'particular equilibrium'. I have here adopted the more generally accepted, if less exact, denomination of 'partial equilibrium'.

In normal cases the cost of production of commodities produced competitively –
*as we are not entitled to take into consideration the causes which may make it rise or
fall* – must be regarded as constant in respect of small variations in the quantity
produced. (Sraffa, [1926] 1953, pp. 540–1; emphasis added)[1a]

and Sraffa leaves little doubt about the fact that decreasing returns would
be the typical case in a general equilibrium context.[2]
3. Briefly, Sraffa's 1926 argument starts from the fact that

the point of view [of 'particular equilibrium'] assumes that the conditions of
production and the demand for a commodity can be considered in respect to
small variations as being practically independent both in regard to each other
and in relation to the supply and demand of all other commodities. (Sraffa, [1926]
1953, p. 538)

Otherwise, the *ceteris paribus* assumption would be violated and, in the
face of a change in supply conditions of the commodity – say, a tax or
bounty – we could not assume that the demand will remain fixed or that
the converse will be true in the case of a shift in the demand schedule.

   Now, with respect to Marshallian decreasing returns (rising long-
period costs), Sraffa's argument is that, when they do occur in a particu-
lar industry, there will generally be an effect of similar magnitude on the
costs of other industries as well (Sraffa, [1926] 1953, p. 539). This leaves
only two possibilities: (a) abandoning partial equilibrium when the effect
of the increased output of the industry on its own costs cannot be
ignored; or (b) assuming constant costs also in the industry in question,
when the effect on the costs of the related industries is small enough to be
compatible with partial equilibrium.

   The first case, Sraffa observes, is the one where the output of the
industry requires a *considerable* part of the quantity of the factor 'the
total amount of which is fixed, or can be increased only at a more than
proportional cost' (Sraffa, [1926] 1953, p. 539). In the language of general
equilibrium, this case can be described as one where a large difference
exists between the proportions in which the factors are employed in the
industry expanding its output and the proportions in which the same
factors are employed in the industries that will correspondingly have to

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[1a] In this Comment the pages given with respect to Sraffa's 1926 paper refer to the original
     paper and not to its reprint in Stigler and Boulding (1953). H.D.K.
[2] In fact, in a letter to Keynes of 8 June 1926 (quoted in Roncaglia 1978, p. 12), Sraffa
     explicitly rejects the very interpretation we find in Samuelson's paper: '[my] conclusion has
     been misunderstood and taken to imply that in actual life constant returns prevail... of
     course in reality the connection between cost and quantity produced is obvious. It simply
     cannot be considered by means of the system of particular equilibria for single commod-
     ities in a regime of competition devised by Marshall.'

contract their output.[3] In this case the increase in costs cannot be ignored in the expanding industry, but for exactly the same reason it will not be possible to ignore it either in those other industries in which the factors are used in a proportion close to that of the expanding industry. This will contradict the *ceteris paribus* condition of partial equilibrium.

The second case is where an industry employs only a small part of the 'constant factor' or, more generally, where the proportions of factors in the expanding industry are almost the same as in the contracting industries. Here, the effect on the costs of other industries using proportions of factors close to those of the expanding industry will be small, and so will be the effect on the costs of the industry in question. Then, to the extent that the former can be ignored, as is required by partial equilibrium, it must be likewise ignored in the expanding industry, where we shall accordingly have to assume constant long-period costs (Sraffa, [1926] 1953, p. 539).

As Sraffa summed up his argument in 1925:

The substance of the argument rests on the fact that the increase in production of a commodity leads to an increase in the cost both of the commodity itself, and of the other commodities of the group. The variations are of the same order of magnitude, and therefore are to be regarded as being of *equal importance*. Either we take into account these variations for all industries of the group, and we must pas from the consideration of the particular equilibrium of a commodity to that of general equilibrium; or else those variations in all industries are ignored, and the commodity must be considered as produced under constant costs. *What is inadmissible is that the equal effects of a single cause are at the same time considered to be negligible in one case, and of fundamental importance in the other.* However, it is necessary to accept this absurdity if one wishes to give a general, and not an anomalous character, to the supply curve of a product under conditions

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[3] Here a question arises into which it is not possible to go deeper on this occasion. It was generally accepted at the time that the Marshall *ceteris paribus* assumption implied constant quantities produced of the other commodities (e.g. Barone, 1894, reported in Sanger, 1895; also Ricci, 1933). This raises the question of where the factors required for the increased output of the industry we are studying would come from within a theory where the factors are taken to be fully employed. The reply appears to rest largely on the fact that that increase in output is assumed to be small (as stressed by Marshall and recalled by Sraffa in the passage above), so that the decreases in the output of the other industries that do in fact occur are so small (say, about 1/1000 of the already small change in the industry concerned if there were about 1,000 industries) that they can be ignored. The condition about the specialized factor 'causing decreasing returns' can then be translated, as we saw in the text, into the now more familiar form of the *different proportions* in which the quantities of factors will generally be employed in the expanding industry when compared with the contracting industries.

of increasing costs. (Sraffa, [1925] 1964, p. 324; translated in Sraffa, [1925] 1973; emphasis added)

There remains only a third possibility with respect to decreasing returns – the one in which the industry in question employs *the whole* of a scarce factor. This, Sraffa tells us, is the only case in which we can consistently consider increasing costs in the context of partial equilibrium – a case, however, of unlikely occurrence, in as much as partial equilibrium requires the industry to be small when compared wit the whole economy (Sraffa, [1926] 1953, pp. 539–40).

Considerations similar to these (which we need not enter here) are developed by Sraffa for the case of increasing returns.

4. It is surprising therefore to read in Samuelson's paper:

Sraffa...by...*implicit* appeal to what is legitimate in...partial equilibrium methodology [minimizes] the cases where alterations in composition of demand alter competitive price ratios. (p. 32; emphasis added)

One is tempted to interpret the word *implicit* in the passage as a misprint: clearly, whatever may be 'implicit' in Sraffa's 1926 article, it is not his appeal to partial equilibrium! However, the misprint interpretation will not do, because Samuelson's argument is in effect based on ignoring that appeal. The 'fatal error of 1926' would in fact exist only if Sraffa had been dealing with the supply schedule in conditions of *general equilibrium*.

No less surprising is the argument with which Samuelson attempts then to take care of Sraffa's 'implicit' appeal to partial equilibrium:

To underline that my criticisms are not captious...I here present an impeccable Marshallian model in which (a) each of *n* goods is produced by transferable labour *and a specialized land specific to itself*...The example glaringly contradicts Sraffa's constancy of costs and obeys all *partial* equilibrium requirements (at the same time that it is a full general equilibrium model, a congruence Alfred Marshall never quite achieved). (p. 32–3; emphasis added)

Rising costs are undoubtedly compatible with partial equilibrium provided each good is produced by a 'land specific to itself', but is that not exactly what Sraffa argues in his third case above?

It is in any case clear that Marshall's partial equilibrium was never meant to deal only, or even chiefly, with cases like the one described by Samuelson (or with a similar one to which Samuelson refers in another article – 1971b, p. 12). Partial equilibrium was meant to be of *general* applicability: it had therefore to be conceived as a sufficient *approximation* to general equilibrium, and *not* as a special case of the latter. (This incidentally explains why Marshall never 'achieved' Samuelson's congruence between partial and general equilibrium; that congruence was the

opposite of what Marshall intended to achieve, as it would have made the partial equilibrium method useless.[4,5])

5. The fact that we can today so lightly dismiss partial equilibrium in scholarly articles (though not in elementary textbooks) is perhaps a sign of the resigned acceptance of the inapplicability of contemporary mainstream pure theory to the concrete problems for which partial equilibrium was designed – it is a sign, that is, of the descent of pure theory towards 'rhetoric', to use Samuelson's word. That was not yet so in 1926, when Sraffa concluded the argument I have reported with:

If diminishing returns arising from a 'constant factor' are taken into consideration, it becomes necessary to extend the field of investigation so as to examine the conditions of simultaneous equilibrium in numerous industries: *a well-known conception, whose complexity, however, prevents it from bearing fruit, at least in the present state of our knowledge, which does not permit of even much simpler schemata being applied to the study of real conditions.* (Sraffa, [1926] 1953, p. 541; emphasis added)

And in the 1933 *Review of Economic Studies*, Umberto Ricci, a practitioner of general equilibrium who, together with Barone, had accurately studied the mathematical relations between Marshallian partial equilibrium and Walrasian general equilibrium, would still write in an article on 'Pareto and pure economics':

But when we have ... paid the tribute due to the authors of one of the most outstanding creations of human thought, we are compelled to limit the field of its application. The whole apparatus gives somewhat the impression of a magic castle, satisfying to the imagination, but of little assistance in solving the housing problem. In more prosaic language the theory remains abstract and without grip ... Among the theories of equilibrium enshrined in the formidable apparatus of the formulae of [Pareto's] *Manuel d'économie politique,* ... there is to be found no bridge leading to nine-tenths of the problems which economists set themselves ... we can by no means afford to put aside the theory of particular equilibrium as developed by Marshall and his many followers ... Pareto himself, the most jealous custodian of the theory of general economic equilibrium, the most

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[4] I have recalled elsewhere (Garegnani, 1987, para. 6, p. 563) Marshall's basic methodological position according to which 'the function of analysis and deduction in economics is not to forge a few long chains of reasoning ... but to forge rightly many short chains and single connecting links' (Marshall, 1961, Appendix C).

[5] Indeed, Samuelson himself implies a *general* applicability of partial equilibrium when he faces the problem of inducing laymen to accept as plausible the modern apparatus of demand and supply. He does accept there the basis of Sraffa's argumentation: 'other things equal ... specifically this means that as we change wheat's price, we must not at the same time change family income, or the price of the competing product or anything else that would tend to shift the demand schedule for wheat' (Samuelson, 1973, p. 66).

sarcastic belittler of literary economics, the less sarcastic but no less resolute adversary of the theory of particular equilibria, was compelled to forget general equilibrium when he was writing his superb chapters on applied economics. (Ricci, 1933, pp. 20–1)

Today's intertemporal general equilibrium is surely no less of a 'magic castle' than the Walrasian and Paretian general equilibria of 1933.

2   Demand and prices

6. In the article of 1926, Sraffa was in effect still moving largely within the ambit of marginal theory. The classical economists were seen through Marshallian eyes, as assuming horizontal long-period supply functions which allowed them to ignore the effects of demand on prices. This was a misleading picture of classical analysis, as we now know thanks to Sraffa himself. (By that picture, Marshall was in fact attributing to the classical economists an influence of demand on prices *through* distribution and costs, and was therefore taking for granted a demand and supply theory of distribution.)

However, the article needed a follow-up: it had left open the question as to what was to be done beyond that 'first approximation' of constant costs. And the indications given by Sraffa made it difficult to see how further progress could occur *within* the accepted stream of ideas. Thus, decreasing returns, when important enough, were thought to require turning to general equilibrium – but at the same time that conception was ruled out as being 'too complex to bear fruit'.

In fact, the solution toward which Sraffa was feeling his way lay in a direction entirely different from that of Walras and Pareto: it consisted of a re-examination of the theory of distribution, a line absent from the 1926 article.[6] This re-examination and the associated revival of the classical theory of distribution and relative prices is what emerges in (1951) and (1960).

7. It is not therefore surprising that Samuelson, who looks at *Production of Commodities* (1960) in the terms of marginal theory, should see limitations in Sraffa's treatment of the effects of demand on prices. However

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[6] At the end of the 1926 article Sraffa writes: 'the influence [of a normal level of profits throughout all the industries] on the formation of the prices of single commodities is relatively unimportant and [its] consideration is therefore beyond the scope of this article' (Sraffa, [1926] 1953, p. 550). However, where the problem of distribution indirectly emerges, in connection with the influences of the changed outputs on the prices of factor services, the solution remains the received Marshallian one, based on the equilibrium between demand and supply of factors.

what is in fact there is just a *different* way of dealing with the interaction between prices and outputs.

What is perhaps more surprising is that Samuelson should have expected to find there the demand functions of mainstream theory. Had not the criticism of capital theory been promoted by Sraffa and, in particular, by that very book (1960)?[7] And was not that criticism aimed at the explanation of distribution in terms of demand and supply of factors – as Samuelson himself did not fail to see on some occasions?[8] On the other hand, it will be agreed that the mainstream determination of the prices of commodities by their demand and supply functions is one and the same thing as that theory of distribution. (The Samuelson whom we have just seen insisting on increasing costs for commodities – the direct expression of a demand and supply equilibrium in the market for factors – is certainly well aware of this.) It would therefore seem reasonable to expect Sraffa's criticism of the traditional concept of capital (1960) to be accompanied by some different treatment of the interaction between outputs and prices.

8. In my contribution to this volume [the reference is to Chapter 5 in Bharadwaj and Schefold (1990)], I have in fact attempted to clarify how outputs can influence prices in the context of the theories of the classical economists and Sraffa. I have there pointed out how the principal mechanisms by which outputs influence prices in marginal theory –

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[7] The first expression of this criticism to appear in print was Joan Robinson's (1953–4); regarding the derivation of that criticism from Sraffa's work, see Robinson (1973), pp. 144–5.

[8] For example, with reference to 'reswitching' and 'reverse capital deepening', Samuelson wrote: 'Such perverse effects do have consequences ... stability and uniqueness problems may be raised for a Solow–Harrod growth model' (Samuelson, 1966b, p. 578), where the model in question, taken in its basic building block affected by these 'perverse' effects, is simply the long-period equilibrium of marginal theory. I cannot enter here into the question of how this criticism also affects, besides that long-period equilibrium, the 'temporary' and 'intertemporal' equilibria of contemporary theory. (It would, however, seem intuitively clear that instabilities of the long-period equilibria will show up in the sequence of the short-period 'temporary' or 'intertemporal' equilibria.)

In this connection we may incidentally note how the situation here referred to as 'long-period equilibrium', characterized by a uniform rate of return on the supply prices of capital goods, seems to be taken by Samuelson as equivalent to a 'steady-state analysis' (e.g. p. 37), with which it has in fact little in common (on the difference between the two, and on the origin of the present-day tendential identification of the two, see Garegnani, 1976a, pp. 27, 33n). In fact Samuelson appears to view Sraffa's analysis as a steady-state analysis. However, as I have argued in my Comment on Asimakopulos in this volume [the reference is to Chapter 10 in Bharadwaj and Schefold (1990)], Sraffa's analysis cannot be qualified as relating to an economy stationary or in steady growth, any more than the analyses of Smith, Ricardo and Marx can.

changes in relative outputs giving rise to changes in factor prices and hence in commodity prices – is absent in classical theory. This is because their different theory of the distribution between wages and profits (sketched in section 2 of my paper) allowed them, and Sraffa, to take the real wage (or the rate of profits) as given when determining prices.[9]

In the classical theories, therefore, demand functions and outputs could be conceived to affect prices *only through variable returns to scale and joint production.* However, the introduction of demand functions in those theories meets, among others, the difficulty that the level of individual incomes can no longer be defined by anything like the equilibria of demand and supply for factors. Thus it is difficult to see how commodity demand functions can even be constructed at a general theoretical level.

It appears that, as a result, the outputs will have to be dealt with by the different two-stage procedure which was essentially that of the classical economists (see my paper, par. 31). We determine the effect on prices of the change in our independent variable while taking the outputs as given (p. 130) (or as the independent variable in question). Then, at a second logical stage, we can consider the effects on the outputs of the change in both the prices and the independent variable causing them (say a rise in wages). At that second stage we can also consider, where necessary, the effects of those output changes back on the prices, etc.

9. This is the procedure that is reflected in Sraffa's (1960) treatment of the outputs of the several commodities as data. Instead Samuelson reads there an 'inexplicable' failure to single out that 'a change in the composition of demand, towards or away from relatively labour – land intensive goods, in real life alters goods' relative prices', and he continues:

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[9] We may note in this respect a passage in Samuelson, p. 33 above, where he refers to Part I of Sraffa (1960) as a 'model involving labour as the sole factor of production' and he implies that this (together with constant returns to scale) is why demand need not be introduced to determine prices there. This is surprising. Produced means of production are obviously present in Part I of Sraffa (1960), and a positive rate of profits (interest) is paid on them. It is therefore a situation that should be described as one in which there are several factors of production, that is, labour and capital, or labour and the several capital goods, or the several 'dated quantities of labour'. However, what Samuelson may mean is that, since a supply of capital goods is not introduced by Sraffa with a determining role, then it is *as if* there were only one scarce factor – labour (see, for example, Samuelson, p. 33 above). If that is what Samuelson means by his 'one-factor' passage, he is incorrect: the 'supply of capital goods' is not present in Sraffa because of the different classical theory of distribution, and not because of any 'underemphasis' of the role of such supply. And the reason why demand does not affect prices is that different theory of distribution, and not any one-factor hypothesis.

[Sraffa's] own 1960 Chapter 8 negates his acceptance of Ricardo's belief that the complications of land rent for relative price can be finessed by concentrating on goods produced at the external margin of no-rent land. Where that margin falls is affected *endogenously* by the composition of demand – as many of Ricardo's own paragraphs reveal. (p. 37)

In fact, Ricardo's idea about how to avoid the complications of land rent is irrelevant. What counts is only the possibility of taking outputs as given. When the position of the marginal land depends on distribution and relative prices, prices can be easily determined once outputs are given – just as they can under Ricardo's simplified hypothesis. What is relevant is whether, as a part of the two-stage classical analysis indicated above, outputs can be taken as given when determining prices, or have instead to be determined simultaneously with prices on the basis of the traditional demand functions.

The same considerations apply to the 'narrowness' Samuelson attributes to Sraffa's treatment of joint production (pp. 33–6). Sraffa has in fact no need for special cases where prices are independent of outputs. He can quite well take care of how prices change with outputs i.e. with the 'effectual demands' of the classical authors. Of course, joint production raises the additional problem of the equality between the number of processes and the number of commodities. This equality is the normal case, as Schefold (1985a) has argued, and would seem clear once we realize, for example, that highly substitutable commodities should be treated as a single commodity in this respect (the price of mutton in the wool–mutton case certainly has much to do with the price of beef, and therefore with its process of production).

3   Standard commodity

10. One of the surprising features of Samuelson's paper is that he seems to view the standard commodity as intended to validate the labour theory of value (p. 36) – and not in the sense that the standard commodity allows one to measure the aggregates of commodities entering the determination of the rate of profits independently of distribution, just as the labour theory of value allowed Ricardo and Marx to do. From Samuelson's argument one might gather that the intended validation is that of the principle that commodities exchange according to the quantity of labour required for their production.

Thus, for example, after showing that relative prices do depend on distribution (the rate of profits), contrary to the labour theory of value, Samuelson concludes:

even...where Sraffa can define a single standard commodity, this fault of the labour theory is *not in the least bit* touched by any use of the standard commodity. (p. 37; similar passage on pp. 38–9 and *passim*)

The obvious reaction to this statement is: how could the standard commodity touch that? Who said the contrary? (Samuelson gives no precise references. One would imagine that any such error, should anybody have made it, could be quickly disposed of by pointing out that the standard commodity can only do what numéraires can do, that is, modify the absolute expression of prices, and above all, the *shape* of key relations like the wage–profits curve (of which more presently), but certainly not alter the ratios in which commodities exchange.[10]
11. I shall therefore confine myself here to what Samuelson has to say on the standard commodity as such, leaving until later his remarks concerning the labour theory of value.

The starting point here must be that the 'standard system' is not there to validate anything in particular. It is there to render 'more transparent' what is valid independently of it, that is, the properties of the system – just what changes of the 'coordinate system' (of which the standard commodity is one particular instance) are generally intended to do in any science.[11] The standard commodity and system do in fact provide an extremely simple expression of the relationship between wages and profits, and any mathematician would be surprised indeed if that expression were not to make the study of that relationship easier and more complete – whether the measurement of wages in terms of the standard commodity were to be the ultimate one, or only an intermediate step in an analysis where wages are ultimately measured in terms of other commodities.[12]

Moreover, as I have argued (Garegnani, 1987, p. 569, and 1984, p. 312), this expression of the relationship between wages and profits can be obtained by means of the simple mental picture of a known physical net product, to be divided between wages and profits, with the rate of profits originating from the proportion that the corresponding physical share of the net products bears to the equally known physical amount of capital.

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[10] For the neat criticism of a claim concerning the relations between standard commodity and labour theory of value, advanced in Burmeister (1984), and similar to that by Samuelson I mentioned in the text, see Kurz and Salvadori (1987).

[11] See, for example, Rosenbaum (1963), ch. IV, p. 89. I owe the above observation about the generality and importance in mathematics of the method of changes in the coordinates system to Professor Figá Talamanca of Rome University.

[12] For instance, I personally found it easy to demonstrate by means of the standard commodity that any rate of profits *above* the maximum, the wage and prices cannot all be positive (Garegnani, 1976b, p. 427).

As a result, the mutual dependence between the wage rate and the rate of profits *is seen at a glance*. And Samuelson is undoubtedly aware of how arriving at known conclusions by a more direct and easily comprehensible route has often been of fundamental importance in science. (Some aspects of analytical geometry reflect the need for a visual representation of mathematical relations that could conceivably be treated independently of it, but I doubt Samuelson would think that these aspects of analytical geometry are useless.)

In that same article (1984), I provided some examples of that increased transparency, and correspondingly better grip over the properties of the system. One example in point was the property stated in a non-substitution theorem by Samuelson himself (Samuelson, 1966a). According to this, in a system of single-product industries, once the rate of interest (profits) is given, the real wage also is: a theorem that would have been unnecessary if the conception of the standard system had been known and in current use.

12. We may now proceed to some of the more detailed points raised by Samuelson on the standard commodity.

A. Contrary to what Samuelson holds (pp. 36–8) there appears to be no reason at all why a standard commodity (system) should be said to exist only when the same set of methods of production, one for each commodity (the same 'technique' in the common terminology, or the same 'system of production' in Sraffa's own terminology), is in use over the entire range of possible wage and profits rates (pp. 36–8) – just as there is no reason at all why we should say that Samuelson's 'factor-price frontier' for one particular technique or system of production exists only under such conditions. These theoretical constructs are intended to exhibit some properties of the system of production to which they relate, and this both in the interval (intervals) of r where the system is the most profitable, and where it is not.[13] The latter intervals are of no less theoretical interest than the former – for example, in ascertaining why the system of production in question is dominated by a second one.[14]

B. The fact that the standard commodity does not coincide with the basket of goods that workers will buy (Samuelson, e.g., p. 38) seems to be

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[13] Cf. the preceding footnote.

[14] The fact that the straightline wage–profits curve corresponding to the standard commodities of each alternative system cannot be drawn in the same diagram, whereas the wage–profit curves drawn for a wage measured in a common commodity can, seems hardly relevant. What is relevant is the usefulness of the standard commodity for the analysis *of the system it pertains to.*

of little relevance, once the use of that composite commodity is properly understood. It is not only that, owing to Sraffa's treatment of it as dependent variable, the surplus wage can be measured as an abstract value quantity and therefore also in terms of the standard commodity (Sraffa, 1960, p. 33; see also Garegnani, 1984, p. 322). It is above all that the standard commodity and system may be of use even when the wage has ultimately to be measured in a different commodity (just as the treatment of an ellipsis with its centre in the origin of the Cartesian coordinates may be useful for the analysis of an ellipsis in any position whatsoever).

C. Subsistence goods will be present in the standard commodity as soon as we consider the wage variable as consisting only of its surplus element, with workers' subsistence included in the means of production. This takes care of the necessity for an 'indecomposable' matrix of input coefficients, which Professor Samuelson sees as a limitation of the standard commodity (p. 36). In fact what is required is not properly the 'indecomposability' of the matrix, but only that such a matrix should possess an irreducible sub-matrix, corresponding to what are in economic terms a group of 'basic' products (entering, that is, directly or indirectly the production of all other commodities). Now, the treatment of subsistence goods as means of production ensures that they all become 'basic commodities', so long as labour directly or indirectly enters the production of all goods.

D. We may, however, conclude our argument on the standard commodity on a lighter tone, by noting how Samuelson ends up by apparently granting to the standard commodity more than its due. He refers (p. 40) to a single-product industries case where the actual economy coincides with Sraffa's 'standard system'. The rate of profits can there be reckoned as the ratio between the physical profits and the physical capital, both consisting of standard commodity. It can therefore be also reckoned in terms of the quantities of labour embodied in the two aggregates. This would undoubtedly validate Marx's determination of *the rate of profits* in the so-called 'transformation problem'. However, Samuelson goes further and writes:

> to *his* values tableau, Marx could in this specially contrived case apply *his* proposed 1894 Volume III algorithm for the transformation problem. The resulting new tableau... happens (in our singular case...) to match exactly the correct Smith–Walras–Dmitriev–Bortkiewicz–Seton–Sraffa competitive prices tableau. (p. 41)

If Samuelson means what he appears to state in the passage, he is not correct; the prices that Marx would obtain by applying the correct rate of

profits to the quantities of labour embodied in the variable and constant capitals cannot generally be the correct prices.[15]

4   The labour theory of value

13. We may now proceed to Samuelson's remarks on the labour theory of value. His present views are in all essential respects identical to those he expressed a decade and a half ago during a well-known debate in the *Journal of Economic Literature* (1971–74) on Marx's economic theory. At the close of that debate he had thrown a challenge to anybody to offer evidence that

[Marx's] novel analytical innovations concerning positive equalized rates of surplus value are other than a detour *to one who would understand 19th-century or earlier-century distribution of income* . . . If I am wrong in my answer to this question – which has been *the* number one question among pro- and anti-Marx analysts from 1867 to the present day – presentation of some new and cogent argumentation controverting my contention can dispose of it. (1974a, p. 69; emphasis added)

Elsewhere (Garegnani, 1986), I have pointed out that the basis for such a 'cogent argumentation' had in fact been advanced by Piero Sraffa in his 1951 Introduction to Ricardo's *Principles*.[16] There Sraffa (pp. xxx–xxxvii) gave textual and logical evidence to the effect that Ricardo had

[15] This can be immediately seen by assuming that constant capital consists of a commodity (a), say 'steel', and variable capital consists of commodity (b), say 'corn'. The relative price p_a/p_b as correctly determined by estimating the capital in price terms is

$$\frac{p_a}{p_b} = \frac{(1+r)(c_a p_a + v_a p_b)}{(1+r)(c_b p_a + v_b p_b)} = \frac{c_a p_a + v_a p_b}{c_b p_a + v_b p_b} \tag{I}$$

whereas Marx's relative price is

$$\frac{p_a'}{p_b'} = \frac{(1+r)(c_a + v_a)}{(1+r)(c_b + v_b)} = \frac{c_a + v_a}{c_b + v_b} \tag{II}$$

Relations (I) and (II) will therefore give the same result only if

$$\frac{c_a + v_a}{c_b + v_b} = \frac{c_a p_a + v_a p_b}{c_b p_a + v_b p_b}$$

i.e. (after simple transformations), only if $c_a/v_a = c_b/v_b$, that is, only with equal organic composition in the production of the two commodities, in which case of course Marx's rate of profits would have been correct even in an economy not coinciding with the standard system and there would have been no transformation problem to deal with.

[16] It is in fact to this unpublished paper (Garegnani 1986) that Samuelson appears to refer his comments on p. 44, since in Garegnani (1987) there is no reference to Samuelson's 'erase-and-replace' explanation of the transformation problem. (The paper has been published in the meantime; see Garegnani, 1991.)

arrived at the labour theory of value of the *Principles* in order to over-come a central logical error in Adam Smith's *Wealth of Nations*. The error was that, in many parts of his great work, Adam Smith had failed to see the constraint that binds the rates of wages, profits and rents and prevents each such rate from varying without affecting the others.[17] If that is so, it is of course natural to think, and is in fact borne out by considerable evidence both textual and logical,[18] that Marx, who placed that constraint at the centre of his theoretical work, developed Ricardo's 'law of value' for that very same basic analytical purpose (Garegnani, 1987, pp. 567–8; also 1984, pp. 305–9): hardly a 'detour' then.

14. Adam Smith's error emerges when, for example, the real wage is determined by subsistence requirements, as described in Chapter VIII of the *Wealth of Nations*, while the rate of profits is left to be determined (in Chapter IX) by the 'competition' of capitals.[19] This error appears to be the result of viewing the price of a commodity as somehow capable of accommodating the change in one of those variables without a necessary change in some of the others.[20]

Ricardo was able to see through those misleading appearances, at first by means of the assumption of a corn wage underlying the principle he used that 'it is the profits of the farmer that regulate the profits of all other trades' (e.g. Ricardo, 1951–73, VI, p. 104, IV, p. 23) – what Samuelson calls the '1815 lost-Atlantis model of Ricardo' (see par. 18 below). As Sraffa has argued, that principle had its rational foundation in the assumption that wages (and therefore, for Ricardo, the entire capital ultimately used there – see Garegnani, 1984, p. 300) consists entirely of corn. Then:

in agriculture the same commodity, namely corn, forms both the capital (con-ceived as composed of the subsistence necessary for the workers) and the product; so that the determination of profit by the difference between total product and

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[17] 'When the stocks of many rich merchants are turned into the same trade, their mutual competition naturally tends to lower its profits, and when there is a ... like increase of stock in all the different trades carried on in the same society, the same competition must produce the same effect in them all' (Smith, [1776] 1960, p. 105). The passage begins with 'the increase of stock, which raises wages, tends to lower profit', but, as the context of the passage shows, the idea is absent that the lowering of the rate of profits is a *logical implication* of the raising of the wage rate. See also Stigler (1952), p. 203; Hollander (1973), p. 181.

[18] Some of this evidence is given in Garegnani (1984), p. 309.

[19] Marginalist theorists avoided the error when with Wicksteed (1894) they faced the prob-lem of the so-called 'exhaustion of the product' once each factor is remunerated at its marginal product.

[20] As Sraffa shows (1951, pp. xxxv–vii), Smith's position was in fact connected with his view that a rise in wages would raise all prices.

capital advanced, and also the determination of the rate of this profit to the capital, is done directly between quantities of corn without any question of valuation ... It follows that if there is to be a uniform rate of profit in all trades it is the exchangeable values of the products of *other* trades relatively to their own capitals (*i.e.* relatively to corn) that must be adjusted so as to yield the same rate of profit as has been established in the growing of corn; since in the latter no value changes can alter the ratio of product to capital, both consisting of the same commodity (Sraffa, 1951, p. xxxi)

If we simplify by assuming that capital consists entirely of wages advanced for a single year, we may see the agricultural rate of profits (and therefore the general rate of profits in the economy) as given by

$$r_A = \frac{P - N}{N} \tag{1}$$

where P is a given amount of corn output and N is the wages (also a quantity of corn) for the quantity of labour required to produce it on the least-fertile land under cultivation (that which yields no rent).[21]

It is then clear that the rate of profits is determined once the real wage (a quantity of corn) is given, since the quantity of labour required to produce the quantity P of corn being given, N will also be given. Smith's 'competition of capitals', is seen to have no influence at all on the rate of profits. Nor can the rate of profits be raised, as Malthus suggested, by an increased demand for commodities from the land-owners. The rate of profits can rise only because of either a reduction in the corn wage or an improvement in the conditions of cultivation of the least fertile land in use.

15. However, it was not long before Malthus pointed out that, once forms of capital other than corn are taken into account, the rate of profits in agriculture must depend on relative prices just as it does in the other sectors. I have dealt elsewhere (Garegnani, 1982, pp. 76–7) with Ricardo's struggle with that problem in the spring of 1815. The conclusion he reached was that exchange of commodities according to the labour required to produce them could be a reasonable assumption from which to begin. To that extent, both the social product P and the necessary consumption N could be taken as known in terms of labour embodied, and the profit rate could then be calculated according to equation (1) above.

Marx inherited this problem and its difficulties from Ricardo. Ricardo had had to assume that, when measured in terms of a commodity requiring

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[21] The position of this land is of course given together with the output of corn for the reasons I indicated in par. 8 above.

a constant quantity of labour, the value of commodities 'in the production of which no additional quantity of labour is required' remained constant as the wage (or the difficulty of producing the wage goods) increased (Ricardo, 1951–73, I, e.g., pp. 100–103). He had, however, also to admit that the resulting fall in the profit rate would affect the relative prices of all commodities, and, therefore, also their value relative to his standard, thus running the risk of contradicting his very premise and of arguing in a circle.

What Marx attempted was to develop and systematize the procedure by which Ricardo himself had been trying to overcome the difficulty – that is, by measuring 'values' in terms of a commodity that would require a constant quantity of labour to be produced and would be an average (a 'medium') with respect to the proportions of capital to labour it required.[22] Measured in terms of such a commodity, the value of the social product would not change as wages changed and would be equal to the quantity of labour it embodies relative to that embodied in the standard. Ricardo hoped that the change in the general rate of profits might then be determined without reasoning in a circle, and might accordingly be used to ascertain the associated variations in the relative prices of the individual commodities. And this is fundamentally what Marx attempted to do in the so-called transformation problem.

16. The labour theory of value of Ricardo and Marx appears, therefore, to have been the analytical tool that allowed them to arrive at the necessary link that binds, one to another, the rates of remuneration of the productive resources (Garegnani, 1984, pp. 302–3; 1987, pp. 566–7); hardly a detour, therefore, especially ' . . . to one who would understand 19th-century or earlier-century distribution of income'. (That link is in fact the same one that Samuelson was to stress, nearly a century and a half later, in terms of the 'non-substitution theorem' referred to above.)

Equally doubtful appears to be Samuelson's 'erase-and-replace' thesis (p. 44) – the one, that is, that he states (1971a, p. 921) when he envisages Marx's 'values' and his 'prices of production' as 'two alternative and discordant systems', and proceeds then to argue that Marx's 'famous

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[22] By the time of the third edition of the *Principles*, Ricardo had in fact come to choose as his 'invariable measure of value' a commodity that would constitute a 'medium' such that 'those commodities on one side of this medium, would rise in comparative value with it, with a rise in the price of labour, and a fall in the rate of profits; and those on the other side might fall from the same cause' (Ricardo, 1951–73, VIII, p. 193).

The difficulty of course was that a 'medium' in terms of which the social product would be invariable would not have the same physical composition as the real wage and, therefore, changes of the real wage would not be measured by it before prices are determined.

transformation problem ... is seen to involve returning from the unnecessary detour taken in volume I's analysis of values' to what elsewhere in the same article is called 'conventional economic theory' (1971a, p. 399).[23]

Here Samuelson overlooks *two* important points. The *first* is that, far from being 'an alternative and discordant system' with respect to 'prices of production', the quantities of labour embodied were seen to be an integral part of the latter system, and as necessary in order to determine the rate of profits without which prices would have remained indeterminate – so far as Marx and Ricardo could see at the time.

The *second* strictly related point is that Samuelson fails to realize how 'the conventional economic theory' of Marx's time was certainly no more aware than Marx of the fact that the price equations were sufficient to bring out the necessary relation between the real wage and the rate of profits. To the extent that the other authors did not refer to Ricardo's and Marx's labour theory of value (or to the conclusions derived on that basis) and instead confined themselves to prices, they went back to ignoring that necessary relation. Far from providing a more correct theory to which Marx could 'return' after his 'detour' on the labour theory of value, they were in fact providing what Samuelson must admit to have been a logically inconsistent account of prices and distribution. Certainly, after Sraffa, Marx could be anywhere, except back to the point where Smith was (Samuelson, p. 27)!

In fact, as I have argued (1987, p. 567; 1984, p. 305), Marx's clarification and development of Ricardo's discussion of the average commodity was an important step forward towards a more exact solution. Marx makes it clear in the manuscripts that became Volume III of *Capital* that he recognized his own arithmetical examples to be inaccurate – in so far as the prices of production are not applied there to the commodity inputs and wages, as well as to the outputs. Thus he anticipated (though he did not himself take) the next necessary step forward, the correctly formulated system of simultaneous price equations (Garegnani, 1984, pp. 307–8).[24]

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[23] Samuelson makes it clear that the 'conventional economic theory' to which he refers is that of Marx's times, or even of 'pre-Marxist' times (see also Samuelson, 1973, p. 66).

[24] Samuelson attributes to me the idea that '[Marx's] mistake was a fruitful one because it led Marx (almost) to a correct Sraffian theory of a competitive price'. However, what I stated was that Marx's error of including a rate of profits determined by the ratio of the social surplus value to the social capital was 'suggestive' because '[it] can be envisaged by us as the result of treating as integral parts of a single method ... what are in fact, when consistently developed, two equivalent methods each of which is sufficient to determine that [rate of profits]' (Garegnani, 1984, pp. 308–9). What is there held to be 'suggestive' is

17. Samuelson is, on the other hand, attempting to crash through a door wide open to welcome him when he tells us of a non-bizarre numerical example in which a small change in distribution causes relative prices to change, quite contrary to what would happen if commodities exchanged according to the quantities of labour embodied. Having dabbled myself with such numerical examples (Garegnani, 1970, pp. 428–35), I am of course far from surprised. In fact, I greet Samuelson's own example with great pleasure, since the 'innocent children' likely to be frightened by it ought to be those belonging to the stable of the 'MIT writers', rather than to that of the 'Palgrave writers' (Samuelson, p. 44).

The fact is that if the relative prices of commodities pertaining to each system of production did not change with distribution, and commodities exchanged in proportion to the quantities of labour required to produce them, the propositions of traditional capital theory associated with the equality between marginal product and rate of remuneration, would all hold.[25] (Indeed, Samuelson himself once argued (1962) for a 'surrogate capital' which rested entirely on the labour theory of value and felt then tempted to assert that the results could be generalized; Garegnani, 1970, pp. 414–16).

Paradoxical as it may at first seem the failure of the principle that commodities exchange according to the quantities of labour embodied is of less consequence for the theory of the classical economists and

therefore the idea of a rate of profits originating from the distribution of surplus ascertainable independently of prices, and originating the rate of profits by its uniform distribution over the corresponding capital – the idea that is the basis of the auxiliary constructions of the standard system and of the integrated wage-goods system (Garegnani, 1984, pp. 313–20; 1987, pp. 570–2).

[25] In this connection we may refer to Samuelson's argument about an incompatibility between the labour theory of value and the possibility of alternative systems of production coming successively into use as distribution changes (pp. 36–40), where Samuelson appears to refer back to his argument in 1974b, p. 292, according to which 'in a regime of values the technique that minimizes "values" at $r = 0$ will minimize them for all r's – a shortcoming of the value model'. That this is not correct is shown by Samuelson (1962), where, when a 'surrogate production function' exists, the consumption good and the capital good in use always exchange according to the quantities of labour embodied. As I have remarked elsewhere (1984, p. 306n), Samuelson fails here to separate two questions. The first is that of the relative value of commodities pertaining to the *same* system of production, in relation to which the labour theory of value has always been used. The second question is that of the costs, estimated at the prices and profits of the system in use of capital goods specific to systems of production other than that in use (see, for example, Garegnani, 1970, p. 411n). It is only if the latter costs also happened to be proportional to the quantities of labour embodied, that the relative profitability of alternative systems of production could not change as distribution changes.

Marx. In fact, the role of the labour theory of value in Ricardo and Marx was fundamentally that of providing a basis for defining an average commodity by which to bring to light the relation between wages and profits. That role is compatible, in principle, with the individual commodities not exchanging according to the quantities of labour embodied. Indeed, Marx himself would have remained quite unperturbed by Samuelson's 'horrendous' example. The ratios in exchange between the commodities as determined by Marx can *also* change quite freely with distribution.

18. Before concluding on the labour theory value, let me deal with what Samuelson calls the '1815 lost-Atlantis corn-to-corn model attributed to Ricardo' (p. 43 and *passim*). The intended implicit reference is probably to the 'lost papers on ... capital' to which Sraffa refers in (1951), pp. xxxi–ii. Unfortunately, the romantic aura Samuelson attributes to those papers is spoilt by the fact that, unlike for the Lost continent, for the Lost Papers we do have a quite down-to-earth record: Trower's letter to Ricardo of 8 March 1814, where Trower say he has seen and read *the actual thing*.

The thesis that seems to underlie Samuelson's references to the 'lost-Atlantis model', or '1815 fabrications' (p. 29, fn.), so far as I can see, finds little support in the results of the literature on the question. I believe it would be fair to say that Sraffa's textual evidence has been strengthened, if anything, in the course of that discussion.[26] But of course, if Samuelson or others have new textual or logical arguments to the contrary, they would be welcome to come forth in an open discussion on this as well as on other aspects of the interpretation of the classical economists. That discussion, I am sure, would be useful for all concerned.

5   Conclusions

19. Finally, before concluding, let me comment on some minor points in Samuelson's paper.

In connection with Sraffa's degree dissertation (which was printed for private circulation, and not 'published'), the statement by Eatwell and Panico, referred to by Samuelson, according to which 'the *normal* value of a currency is completely "conventional", i.e. it can be at *any* level that common opinion expects it to be' (Eatwell and Panico, 1987, p. 447) needs better focusing. What Sraffa holds to be completely 'arbitrary'

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[26] In the by now abundant literature on the question, see in particular Hollander (1973); Eatwell (1975); Hollander (1975); Garegnani (1982); Langer (1982); Hollander (1983); Garegnani (1983b); de Vivo (1985); Prendergast (1986a,b); Peach (1986); Porta (1986); Groenewegen (1986b).

(rather than 'conventional') is the *legal* and not the *normal* value of the currency:

It is supposed that the currency should normally be worth the weight of gold fixed by law...but that value is completely arbitrary and...is no more normal than any other value the currency may assume. (Sraffa, 1920, p. 42; my translation)

As for the true normal value of the currency, Sraffa holds it to be that for which 'no force exists tending to make it change' (ibid.).

The dissertation is evidently the work of a brilliant undergraduate (students in Italy were and still are asked to write a dissertation for the BA degree) who, however, has not yet begun his systematic advanced studies of economics and has therefore all the caution that a serious student has in such conditions. Thus, with respect to a role of the government in the distribution of income, what we find in the dissertation is the remark that, in the processes of war inflation considered there, wages adjust more slowly than prices – and the bright student emerges in the less conventional remark that the same need not be the case in a process of deflation. There the initiative for the adjustment rests with the entrepreneurs who, contrary to the workers, whose standard of living has no absolutely rigid lower limit in the short run, do have the rigid lower limit of the expenses of production which, if not covered by the sale price, make it convenient for them to suspend production, with the result that they will generally be able to force the workers to accept a cut in their monetary remunerations (Sraffa, 1920, pp. 40–1).

20. For the reasons mentioned at the beginning of this Comment, Sraffa is a very difficult author. The difficulty is made even greater than it needs be, because Sraffa's work has in effect been so little discussed on its own terms. Professor Samuelson is therefore to be thanked for what we must hope will be the beginning of a fuller discussion – a beginning that, like all beginnings, is bound to suffer from the fact that the necessary clearing of the ground has yet to be effected.

Comment

Bertram Schefold

FRANKFURT AM MAIN

The participants at the conference will be very grateful to Professor Samuelson for having added a major paper to the proceedings, written in a generous spirit of appreciation for an economist of a rather different persuasion. As an editor, directly concerned by some of Samuelson's observations, I take the liberty to offer some comments in an attempt to broaden the consensus in at least some areas, given that this book is dedicated to the memory of Sraffa.

I

'Mainstream economists of the mathematical or literary persuasion can benefit much from Sraffa's contributions', states Professor Samuelson. Indeed, concepts like basics and non-basics, the maximum rate of profit or the treatment of fixed capital as a joint product are now widely used, but there is less readiness to accept Sraffa's method and only limited agreement has been obtained on the extent to which Sraffa's critique of the concept of capital applies. I should like to start my Comment by advancing the latter argument by a small step, since insight into the critique (where much remains to be developed) may help to gain acceptance for the method.

Let us first agree on properties of long-run equilibria that are accepted by economists of both classical and neoclassical origin: prices, wage rates, rents for lands of the same quality and the rate of profit are uniform. The uniformity of the rate of profit is a long-run equilibrium condition not only in classical but also in neoclassical theory. It may from the start be assumed to hold as a result of competition. The rate of profit is in general not uniform in this sense in *intertemporal general equilibrium* models, but, to the extent that they are not due to permanent scarcities, the inqualities of different own-rates of interest of different commodities even then tend to disappear as a result of a special form of a competitive process, and a unique uniform rate of profit emerges as the time horizon is shifted towards infinity (Bewley, 1982).

Differing own-rates of interest were introduced in Sraffa's critique of Hayek. I wonder whether it would not have been better if Sraffa had

revealed more of his critique of the Austrian theory of capital on that occasion. We do not know how far it had been developed at the time, but he wrote: 'A considerable part of the book is taken up by preliminaries about the relations between the quantity of capital and the length of the process of production and about the proportions in which the flow of money is divided between the purchase of consumer's goods and the purchase of producer's goods. Dr. Hayek as it were builds up a terrific steam-hammer in order to crack a nut – and then he does not crack it. Since we are primarily concerned in this review with the nut that is not cracked, we need not spend time criticizing the hammer' (Sraffa, 1932, p. 45). A critique of Hayek's theory of capital would *eo ipso* also have been one of any supposition of a – barring monetary disturbances, i.e. in the absence of forced saving – spontaneous tendency towards a neoclassical equilibrium at full employment. This might have allowed a more effective attack on the 'rotten core of the Hayek work' than was provided by Sraffa's early critique of the erroneous monetary theory of the cycle.

For those interested in Sraffa's later work the review is important because it helps to elucidate the problematic of the multiplicity of own-rates of interest when relative prices are changing, as in Keynes, where own-rates of interest are used to analyse the short-run equilibrium at less than full employment. I am told that Professor Samuelson, later a pioneer in turnpike theory, dedicated a maiden paper to this connection.

Sraffa interprets inqualities of own-rates of interest of commodities as expressions of differences between the market price and the cost of production (Sraffa, 1932, p. 50). In fact, he considers 'the case of a non-money economy, ... when equilibrium is disturbed, and during the time of the transition, the "natural" rates of interest on loans in terms of the commodities the output of which is increasing [because an initial deficiency of supply is being made good] must be higher, to various extents, than the "natural" rates on the commodities the output of which is falling; and that there may be as many "natural" rates as there are commodities. It will be noticed that, under free competition, this divergence of rates is as essential to the effecting of the transition as is the divergence of prices from the costs of production; it is, in fact, another aspect of the same thing' (Sraffa, 1932, p. 50).

The formal similarity of a classical equilibrium with a uniform rate of profit and a neoclassical one where own-rates of interest are equal has led to the claim that the world of *Production of Commodities* is only a special case of the Debreu world where divergent own-rates of interest are allowed for. Sraffa is said to deal with that special case in which initial endowments happen to be such that relative prices may remain constant

in the intertemporal equilibrium so that own-rates are all equal and we may speak of a uniform rate of profit (Hahn, 1982).

Against this I have argued elsewhere in this book and in Schefold (1985b) that there are profound differences relating not only to the theories about the genesis of such equilibria, but also to the state that is reached. In fact, the domain of application for such long-run equilibria is larger in the *classical* approach because of the different explanation of distribution and employment and different assumptions about effective demand. In neoclassical equilibrium, prices of goods are determined simultaneously with prices of factors, i.e. with distribution, while classical theory proceeds sequentially: prices of commodities in a classical long-period position are determined on the assumption that the real wage rate or the rate of profits is already exogenously given. Several contributions to this volume discuss alternative theories of distribution: they imply that Sraffa does not refer to a full-employment position.

As to a possible genesis of a neoclassical long-run equilibrium, the divergence of own-rates of interest in a Debreu-type intertemporal equilibrium is proof of a peculiar kind of disequilibrium. It is true that there is an equilibrium in so far as arbitrage would not be profitable: a higher own-rate of interest of a particular commodity does not indicate that one should move into the corresponding industry in order to obtain a higher rate of profit because relative prices change. Rather, it indicates a falling price and that the supply of the commodity is already being increased at a rate that is such that neither excess profits nor losses are being made. But the divergence of the own-rates of interest in the early periods of an intertemporal equilibrium reflects the slowness of the adaptation of production with given endowments – which are employed fully or receive zero prices – to demand, which is ultimately derived from demand for consumer goods. The situation implies a disequilibrium at the beginning in so far as the stocks of endowments of capital goods had once been accumulated to be utilized fully, and equilibrium in the capital goods market must mean not only that capital goods receive a positive price but also that their price corresponds to the cost of production. The prices for endowments of capital goods at the beginning of a Debreu equilibrium are just demand prices; the supply conditions as governed by earlier costs of production are, as it were, simply forgotten when time starts. Viewed as a model describing a process of adaptation, the intertemporal model is deficient in so far as it does not take deceived expectations generated by unused initial endowments into account.

If the Debreu equilibrium is thus understood to start from an initial disequilibrium situation which is, with perfect foresight, transformed into an equilibrium, the proof of the temporary nature of the divergence of

own-rates of interest must be that they disappear in the long run – this is precisely what happens according to the results on turnpike properties of intertemporal general equilibria that have been assembled since Bewley's pathbreaking work of 1982. The long-run equilibrium therefore is not only a special case of an intertemporal equilibrium that happens to have endowments in 'correct' proportions, but also the centre of gravitation of an intertemporal equilibrium, if that is formulated as one with a distant time horizon (Duménil and Lévy, 1985), in the special case of permanent market-clearing, full employment and perfect foresight during the process.

Of course, in any neoclassical world with distribution governed by supply and demand for factors of production, relative prices will be constant in the long run only if the relative scarcities of primary factors have no reason to diverge as time goes on – reasons for divergence, which must be excluded, are e.g. the exhaustion of resources (Radner, 1988). Turnpike theorems with recursive utility show that preferences need, on the other hand, not be expressed in terms of discounted utility functions, where the utility function for each period would be assumed to be constant; they are explained directly as evaluations of consumption paths stretching over an infinite horizon, and they are such that the initial endowments of capital goods have no influence on the final stationary state (Epstein, 1987). It has been suggested by Garegnani (1976a) that the old neoclassical notion of long-period equilibrium was replaced, following Hicks and others, by temporal and intertemporal equilibrium concepts. Now, the old concept, in the form of a terminal stationary state with a uniform rate of return, has surfaced again.

What is really at issue is the confrontation of the classical and neoclassical theories of long-period equilibrium themselves (see Garegnani's main paper in this volume [the reference is to Chapter 5 of Bharadwaj and Schefold (1990)]); the intertemporal model provides only a special case of a process of convergence to a neoclassical long-period equilibrium, and, if endowments happen to be in 'correct' proportions from the start, there is no problem of convergence.

However, the attempts to show how the neoclassical long-period equilibrium is generated as a terminal state of an intertemporal equilibrium have the merit of clarifying conditions for the convergence, and here I come to my point: as results from Epstein (1987, p. 341), it appears to be necessary, in order to prove the convergence to the long-run equilibrium, to postulate that the economies are 'regular' in the sense of Burmeister (1980). This means, essentially, the reswitching and Wicksell effects have been ruled out by simple assumption. It indicates that the critique of capital concerns all kinds of truly long-run neoclassical equilibria, not

only Clarkian parables. It had been claimed that the modern versions of intertemporal equilibrium were immune against the critique; now the task is to show how it extends to neoclassical long-period equilibria as soon as it is asked how they can be attained or be stable.

II

Among the criticisms raised by Samuelson, if not against Sraffa then against some of his followers, one looms particularly large: the neglect of the effect of changes in demand on changes of prices in a system of prices of production with a given rate of profit. Mistaken is the 'impression from Part II of Sraffa (1960) that the influence of demand composition on relative prices is absent in Part II in the same way that it can be absent from Part I's model involving labour as the sole factor of production and no jointness of production' (see p. 33 of Samuelson's paper).

I doubt that many of the relevant authors are really guilty of ignoring such essential relationships; Eatwell has clarified the point concerning his own presentation in his Comment. In order to avid unnecessary controversy, I shall simply state how I see the problem with regard to land and joint production.

It may be recalled that the conditions of production and the levels of output are regarded as given in the first ten chapters of the book; it is clear that prices of production would change even in the single-product case if returns to scale are not constant and the level of output was varied. In fact, the analysis starts with technology, outputs, employment and distribution *all* taken as determined, and changes of these data are analysed one by one. Sraffa himself never presented an analysis of the interaction of these factors, and it is doubtful whether a complete theory will ever be devised that would explain all their variations in historical conditions.

The properties of prices which may be considered if distribution changes and outputs are kept constant are few, but essential: the best-known example is the change in the value of capital as a consequence of a *hypothetical* change of the distributive variable. The capital controversy has proved that the thought experiment is relevant although one knows that an *actual* change of the distributive variable may cause switches of technique. An actual change in distribution is also likely to affect the composition of output, in particular of investment. There is therefore only limited, but relevant scope for an analysis with changes in distribution and the levels of output taken as given.

The analysis of prices is supported by *hypothetical* alterations of the quantity system (e.g. in the construction of subsystems) which could

correspond to real variations of output levels only in the case of constant returns.

If *actual* outputs are assumed to change, with constant returns to scale and with the rate of profit being kept fixed, prices of production remain constant in the single-product case. In joint-production systems and/or if land is considered, such output changes usually induce changes in the method of production (entailing price changes) if they are large, but prices of production will in general stay constant if the changes considered are small. It would perhaps be more straightforward to establish this result by considering changes of given levels of *gross* output to be produced (including investment), but I shall argue in terms of *net* outputs (of consumption goods) because the corresponding model has been thoroughly analysed and is better known. I start with land.

Sraffa, following a long tradition, distinguishes two main types of rent: a familiar example of differential rent of the first kind is the Ricardian cultivation of different lands such that the price of the produce is determined on a marginal no-rent land (which pays no rent because it is not fully utilized). Differential rent of the second kind is illustrated by the use of a well-defined piece of homogeneous land, say the Principality of Monaco, which is used fully by means of two methods, say flats in old villas or in sky-scrapers, such that the combination of the more land-using (villas) and less land-using, but more cost-intensive technique (sky-scrapers) allows the satisfaction of a given demand for the produce (flats for people who want to escape the payment of taxes elsewhere). In both cases, needs and production may rise continuously, with the extension of cultivation on the no-rent land or with the displacement of the more land-using method by the less land-using one, while rents and prices rise spasmodically (as soon as new marginal lands or even less land-using, more cost-intensive techniques have to be brought in). The step function so constructed for either type of rent will be called a normal cost curve. It is clear that the level of output is among the determinants of the step reached, i.e. it codetermines the last land used (first kind of rent) or the methods to be combined (second kind), but only sufficiently large changes of that level lead, with a given spectrum of techniques, to a different step, so that prices remain unchanged with small changes. I want to avoid the possible transition to a continuous change of technique and to non-constant returns in order to focus on this conceptually simplest model.

It has been shown how rents and land may be eliminated in those cases from the system at given levels of output in order to express the prices of basics, either by considering the price at the extensive margin or by combining the two methods in the case of differential rent of the second

kind linearly in such a way that rent and land disappear from the combined process ('intensive margin'). By means of this procedure, one can 'get rid of rent' in the consideration of the basic system, *but the exercise is valid only within the stated assumptions*, in particular if only hypothetical price changes are considered and output does not change at all, or little – a familiar application being the proof that taxes on rents fall wholly on landlords (as long as the reduction of the purchasing power of landlords and the increase of that of the state does not affect the methods of production of basics, e.g. through a large change in the composition of output).

In neoclassical theory, land is treated as a primary factor parallel with labour. This leaves room for a determination of rent through demand and supply on land that is cultivated fully by means of one method only, without an explanation of the rent as a differential between the cost of production of this method and some other method, on the same or on some other land. Such a treatment of land also appears in Samuelson's paper, e.g. in his Tableau (p. 33). The assumption seems to be incompatible with a post-Smithian classical theory (leaving aside the special case of monopoly rent and the dubious concept of absolute rent); it would imply that any small change of output may affect rents and prices even if there are constant returns, both for industrial processes and in the expansion of the scale of production of any given piece of land.

I can see two reasons for Samuelson's procedure. One is the repeated combination of a neoclassical approach to rent with Sraffian problems of capital theory in order to extend the critique of the neoclassical theory of capital to the neoclassical hypotheses about land and rent by Metcalfe and Steedman (1972) and other writers.

A more important reason is this. A neoclassical author might cross the normal cost curve with a demand curve falling from the left to the right so that something very similar to the cross of the supply and the demand curve would be obtained. If this intersection was found on a vertically ascending part of the normal cost curve he might say that an equilibrium had been determined such that the price was above the cost of production 'at the margin', and rent or surplus profits correspondingly higher, so that the equilibrium would not be presented by configurations of the Sraffa type. Instead, a separate determination of rent through demand would have been established, as in several of Samuelson's examples. A deeper justification could be based on the following observation: even if we take a classical view of demand, market forces must raise the price of the product, as the expansion of production stops temporarily whenever a new marginal land or a more cost-intensive technique has to be brought

in *before* the switch in the methods of production can take place. Should we therefore not say that 'demand' is the true determinant of rents?

What would Sraffa have replied? Thanks to the changes of land with differential rent of the first kind and thanks to the changes of method in the case of differential rent of the second kind, a 'progressive increase of production on the land' is possible and '*output* may increase continuously although the methods of production are changed spasmodically' (Sraffa, 1960, p. 76, emphasis added). Here we have, in chapter 11 an actual change of output levels, but the rate of profit is assumed to stay constant and no theory to explain output is offered. To provide it, many elements would have to be brought in, in particular the principle of *effective* demand in a form compatible with the explanation of distribution; the determination of consumption should not contradict the critique of the neoclassical theory of distribution and employment.

Most contributions to this volume deal with particular aspects of this research programme. It is clear that the level of output affects the level of cultivation, but the influence of demand on rent could not justify the conclusion that rents ought to be explained in terms of supply of and demand for goods and factors in an interdependent full-employment equilibrium, as derived from subjective preferences. Beyond the critique of marginalism, a different theory of demand has to be provided. As is well known, the classical authors interpreted effective demand for each commodity as a specific quantity demanded at the normal prices. But what determines effective demand? In order to face the challenge, I discuss a special and simplified model that relates output to consumption on the assumption that growth rates (investment) and the rate of profit are given. It is further explained in Section III on joint production.

First, I have proposed elsewhere in this book [the reference is to Chapter 7 of Bharadwaj and Schefold (1990)] that the demand for consumption goods be dealt with in terms of given needs, as Smith, Ricardo and their followers did, and that the responsiveness of the demand for consumption goods to prices be represented in terms of changes in methods of domestic production for the fulfilment of those given needs. Consumption commodities are means of production for household processes; these are added to the system of industrial processes of production. (The responsiveness of demand to income similarly is related to migration between groups of consumers.) The irreversibility of important demand changes, the impossibility of ascertaining what demand would be at prices significantly different from actual prices, and links between socially determined consumption patterns and economic stratification may thus be taken into account. My discussion of this and of the relative

merits of this and of the neoclassical approach to consumer theory cannot be repeated here.

Needs are transformed and distribution changes in the process of development so that our *ceteris paribus* assumptions yield only first approximations, but the formulation of hypotheses concerning the interaction of accumulation, the formation of needs and distribution are beyond the scope of this comment, since Samuelson also takes changes of demand and the rate of profit unexplained as given. This is in keeping with the fact that the discussion is focused on the classical theory of value, but he does not address the question of which theory of demand would be appropriate in the context he considers. Given needs imply that the concept of effective demand is simplified: the normal quantity demanded is determined independently of the price of production, except in so far as there are possibilities of substituting processes and commodities to satisfy the same need.

To the extent that needs are given, the vertical sections of the normal cost curve are then only temporarily met with in the process of accumulation, and this will happen even if the demand for some commodities is price-elastic, provided the explanation of price elasticity of my main paper is used.

The normal or long-period position is found on the horizontal steps. Classical analysis proceeds sequentially. The methods of production are slowest to change. Within that framework, needs and hence normal quantities expand gradually. This is of importance for a (stylized!) interpretation of such an equilibrium as a normal position: actual demand is likely to fluctuate at any time. If the fluctuations are small, they will not lead to a displacement of the equilibrium and a fundamental change in the prices of production. As long as we argue in terms of given needs, whose normal level is fixed or grows slowly relative to rapid fluctuations, positions on the 'steps' are – locally – stable. A major change in effective demand may upset the long-period position itself and cause a change in the course of accumulation.

Second: if demand raises the price beyond the level at which it is 'supported' by the marginal method or combination of methods, the increase is at first to be considered as one of the market price. The normal level of the price of production would then be ill-defined (unless a neoclassical demand curve were used), which is why such a constellation is usually not considered as a long-period position, and a further argument may be advanced to justify Sraffa's assumption. If the constellation is expected to persist for some time, rents are higher than what can be explained through a differential of productivity, and this provides an *incentive* to innovate and to introduce a *new* method of production that

allows a profit to be made according to the normal rate of profit at the elevated level of land rent. There may even be an artificial marginal land, as actually happens in the Principality of Monaco where they are filling up the sea on the shore to push out the coast line, or there may be a compromise and the use of old villas is combined not with sky-scrapers but with the construction of intermediate houses. As a result of such an introduction of a new method, the long-period position will again be established on a new horizontal segment of the normal cost curve, eliminating the formal underdeterminacy of the Sraffa system that occurs if the market price raises rent above the normal level.

The picture of a normal cost curve monotonically rising with output does not always hold. Saucier (1981) has shown that the steps may also go down if several lands and agricultural outputs are involved (contrary to Samuelson, 1987a, p. 458).

III

Under classical assumptions, there is a (not complete!) analogy between the contention that the main forms of rent must be explained in terms of the two kinds of differential rent on the one hand and the proposition that joint-production systems will be 'square' on the other. Let us now stick to the model with given needs. In equilibrium, there cannot be more processes than there are commodities produced; otherwise we have an overdetermination of prices at a uniform rate of profit (which corresponds to the competition between different processes of production at unequal rates of profit in the real world). On the other hand, we cannot, except by a fluke, have fewer processes than there are commodities with a positive price, because needs are given in fixed proportions and, with constant returns, fewer than n processes will not be sufficient to produce n commodities in given proportions. Square systems then emerge if the rule is added that unprofitable activities are not used and overproduced goods receive zero prices (they are not called commodities).

Thus we have the *formal* argument, proposed by Steedman (1976) and rigorously proved by Schefold (1978b and 1988), using inequalities under general assumptions: a given uniform rate of profit and a, in general lower, rate of growth are imposed. (The two rates need not be close to each other, and the uniformity of the rate of growth – analytically convenient but not really classical in spirit – is not essential to the argument; the classical long-period position certainly was not meant as a stationary or steady growth equilibrium.) Then, any vector of final demand will generically result in a long-period position with the number of commodities produced being equal to that of methods used, whatever rectangular

array of methods of production is given initially as a book of blueprints. The square system that thus emerges is a long-period position in which, except for flukes, local and small variations of demand are possible, which do not lead to substitutions of methods, whereas larger ones are feasible only if a new equilibrium is reached.

The *substantive* complement to this formal analysis is, on the one hand, that an *overdetermination* of prices is the rule because 'too many' processes compete. Many applications are possible, e.g. to the explanation of patterns of the price determination of obsolescent machines. The question then is which square technique dominates or is socially necessary and determines normal prices relative to which other methods show surplus profits or losses. If, on the other hand, there is an *underdetermination* of prices, the formal argument assures us that competition will eventually result in a square system. But produced goods, available in excess, that are given away free, apart from charges for handling and transportation, are not encountered often. Leaving aside costs of disposal (as considered in my main paper to this volume), the reason, explained elsewhere since Schefold (1977), is that the falling market price of a good that tends to be overproduced is a strong incentive to use that good as an input in the production of new industrial or domestic activities. In consequence, the long-run solution to underdetermination usually is not that of a bottomless fall in some market prices until we get free overproduced goods and a square solution with fewer commodities and processes but, rather, that new methods are introduced through a form of induced technical progress such that an enlarged square system is obtained and becomes the dominant technique of a new normal position.

To show that there are joint-production systems such that processes and prices do not change at all for all variations of the demand vector, Samuelson reverts to systems that I had called 'all-productive' in Schefold (1971). I should like to point out that such systems are not quite as rare as one might think because they do not presuppose diagonal dominance if there are more than two goods and processes and because it is natural to extend them to the consideration of the activity levels, given a vector of a final demand, on balanced growth paths. It can then be shown (see Schefold, 1978a) that a square Sraffa system emerging from an arbitrary technology set will in general have this property as soon as the rate of growth of the system is close to the maximum rate of growth attainable with this technology set. This means that, generically, the inverse of $\mathbf{B} - (1 + g)\mathbf{A}$ will be positive for the Sraffa system emerging from the choice of technique for rates of growth and profit both close to the maximum.

At low rates of growth and profit, all-productive systems are exceptional. But, as a result of the 'squareness' of the solutions, I have found

that prices are at least *locally* invariant with respect to output changes in joint-production systems and systems with land: if the classical assumption of a given vector of needs is made, those needs may be varied, with the square solution, prices and distribution unchanged, as long as no activity level turns negative.

Finally, the question should be taken up of whether the 'square solutions' remain generic with other assumptions. In this area, much research remains to be done. Square solutions must result, if needs are different for different classes and if the solution is required to be locally stable with temporary fluctuations in demand, as I have shown in my reply to Salvadori in this volume [the reference is to Chapter 7 of Bharadwaj and Schefold (1990)]. The long-run square solutions in themselves are not generic if a neoclassical theory of demand is introduced, as stressed in Schefold (1985a) – I am indebted to Samuelson for this point, which he made on the occasion of a lecture of mine at his Institute. However, there are objections to the non-square solutions even in this case (and in the one of the *ad hoc* example presented in note 2 to Samuelson's paper[1]) if it is assumed to be in the nature of a long-period position that it must accommodate *arbitrary* small short-run changes in the outputs of all commodities. On the one hand, short-run demand changes cannot always be reduced to perturbations of preferences. On the other, there must be room to adapt quantities. Even in Marshall, cattle diseases lead to an

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[1] According to Samuelson's assumptions, we should have one process, producing all commodities in positive amounts (r given)

$$(1 + r)(a_{11}p_1 + \ldots + a_{1n}p_n) + l_1 = b_{11}p_1 + \ldots + b_{1n}p_n$$

with $g = 0$ and demand functions proportional to income

$$c_j(ra_{11}p_1 + \ldots + ra_{1n}p_n + l_1) = (b_{1j} - a_{1j})p_j$$

where c_j, $c_1 + \ldots + c_n = 1$, indicates the share of income spent on net output of commodity j. Of these $n + 1$ equations, n are seen to be linearly independent; they determine prices in terms of the wage rate. A short-run change in demand could be accommodated only by changing all prices and has no clear effect on employment. If these unit elasticity Engel curves were to be retained, it would be better to assume long-run prices determined by a square system $[\mathbf{B} - (1 + r)\mathbf{A}]\mathbf{p} = \mathbf{l}$, long-run relative activity levels $\mathbf{q}$ by

$$(r\mathbf{q}\mathbf{A}\mathbf{p} + \mathbf{q}\mathbf{l})c_j = \mathbf{q}(\mathbf{b}^j - \mathbf{a}^j)p_j; \ j = 1, \ldots, n.$$

If L is employment (how to be derived?), the model might be closed with $\mathbf{q}\mathbf{l} = L$. Short-run activity levels $\mathbf{x}$ could deviate from $\mathbf{q}$, and $\mathbf{x}\mathbf{l}$ from $\mathbf{q}\mathbf{l}$. But, as stated, I should prefer to deduce Engel curves from migrations between groups of consumers with different incomes, with the needs for each group taken as given. A shift of consumers from low- to high-income groups would allow the observation *ex post* of a relationship between a rise in total income and a change in the composition of output (see also Pasinetti, 1981). The solutions would be square.

increased supply of fish in the market in the short run. With joint production, a sudden demand for mutton would have to be choked off by price rises alone if we did not have, as in Henderson ([1921, 1932] 1968, pp. 54–5), two different methods of raising sheep or if more wool results the expansion of a process – possibly a household activity – to use wool. If effective demand fluctuates, the decisive effect has to be on employment in different industries and in the aggregate; it does not operate on long-run equilibrium prices (although provisional changes in market prices may be necessary to *induce* the changes of output levels). Since it is true that the introduction of neoclassical demand theory generates the possibility of non-square long-period equilibria such that alternative square solutions would be less profitable, a dilemma arises, but it is only another indication that neoclassical demand theory is incompatible with the classical theory of prices – a point that may more forcefully be argued with regard to the theory of distribution (Garegnani, 1983a).

IV

Samuelson's observations on Sraffa's article of 1926 furnish an opportunity to return to the neoclassical interpretation of rent as a factor price. I do not think that Sraffa committed an error in his article of 1926, but he may have been misunderstood by some of his readers, and the relevance of his results may be less obvious to modern Walrasian economists than to a Cambridge Marshallian of the 1920s. First of all, the assessment should be based on Sraffa's article of 1925 (Sraffa, 1925) – which is also available in French, German and other translations (Sraffa, [1925] 1975; [1925] 1986) – since the first pages of the article of 1926 are only a summary of the earlier one. Sraffa's real dilemma was that he felt unable to accept the Walrasian approach in economics when he had found, at the end of his article of 1925, that 'These causes of variations of cost, highly important from the point of view of general economic equilibrium, must of necessity be considered to be negligible in the study of the particular equilibrium of an industry' (quoted in Roncaglia, 1978, p. 12).

The Marshallian apparatus of supply and demand curves seemed applicable only under restrictive conditions, the least implausible of which resulted in the postulate of constant costs in a competitive market. Thus Sraffa did not wish to 'fob off on the twentieth century a value taxonomy that was already obsolete in the early nineteenth century' but wanted to get rid of it altogether – a conclusion to which he came close in his earlier critique and which was openly and defiantly thrown at his opponents after softer means of conveying the message had been exhausted in the course of the debate on increasing returns (Sraffa, 1930).

As a matter of fact, Sraffa himself wrote to Keynes on 6 June 1926:

This conclusion has been misunderstood and taken to imply that in actual life constant returns prevail...in reality the connection between cost and quantity produced is obvious. It simply cannot be considered by means of the system of particular equilibria for single commodities in a regime of competition devised by Marshall. (Quoted in Roncaglia, 1978, p. 12)

For the neoclassical theory, with full employment, a first dilemma may be presented in the following terms. *Either*, a general equilibrium is assumed, the solution to which consists in one point in the space of prices and quantities if the solution is unique. Without relaxing at least one constraint, changes in costs can then only be observed if the expansion of the industry under consideration is accompanied by a contraction of other industries due to a change of preferences. This is the point of view taken in Joan Robinson's article, referred to by Samuelson. For, although she speaks of a 'supply price', her equilibrium point moves with changes of preferences. Other causes for shifts of equilibrium points giving rise to equilibrium trajectories, may be changes of technology or of endowments; for example, the changes of prices, and in particular of the wage rate, may be traced as a function of a growing labour force that is always fully employed.

Or the analysis is conducted in terms of demand and supply *curves*. We are here not interested in the demand and supply functions of individual agents or firms that express quantities offered or asked for when all prices vary parametrically, but we are interested in the meaning that can be attached to demand and supply curves in an individual market when other markets are at or near equilibrium, so that an equality of demand and supply in the particular market under consideration implies a general equilibrium. If this approach is chosen, the difficulty is that, as soon as demand and supply are not in equilibrium in the particular market, on points of the supply curve not crossed by the demand curve, there must, because of Walras's law, be at least one other market that is in disequilibrium – a difficulty encountered by Hicks in his stability analysis and dealt with by him using the assumption that the other market in disequilibrium is the money market.

Marshall's assumption, according to Sraffa, is different: he simply assumes that the market under consideration is so small, relative to all others, that a disequilibrium – provided it is not too large – implies disturbances of other markets that are sufficiently small to be ignored. Hence – a point stressed by Marshall and by Sraffa in 1925 – the demand and supply schedules can be drawn only near equilibrium.

Now it is useful to read both articles together. If an expansion of the wheat-growing industry is considered, and if land is not specific to wheat but is also used for other agricultural products, the article of 1926 states on p. 539 (as does, of course, the article of 1925) that, if there are other industries using the same type of land, rents will either not be appreciably affected by the necessary intensification of cultivation in all the industries concerned – this is the case (1) of constant costs, plausible if the other industries are many – or rents rise. In the latter case (2), the change in distribution, if it is sufficiently general, is then susceptible of shifting the demand curves (2a). The article of 1925, however, also stresses another aspect (on p. 324): the supply curves of other industries employing the factor will shift (2b), violating the *ceteris paribus* condition. Joan Robinson's account of the matter in her theory of imperfect competition fails to take the latter point into account, with the result that the argument is not very convincing, and there follows a muddled account of how demand curves might shift with supply (Robinson, 1969, pp. 117–18).

Of course, Sraffa was aware that there were cases in which the use of the Marshallian rising supply curve remained legitimate. It is ironic that the model proposed by Samuelson to provide a meaningful bit of Marshallian analysis in his paper, taken from Samuelson (1971b, p. 367), is precisely the one prescribed by Sraffa in 1925 (but also, more briefly, in 1926): 'These conditions [*ceteris paribus* conditions] reduce the domain to a minimum in which the assumption of rising costs may be applied to a supply curve. They are fulfilled only in those exceptional cases in which all of a factor is employed in the production of an individual commodity' (Sraffa, 1925, p. 323; my translation). In Samuelson's paper there are as many lands as there are commodities, and each land is specialized, each being specifically required for the production of each commodity. This corresponds to Sraffa's assumption that the total amount of a specific factor is used for the production of each commodity.

On the other hand, the example of the paper assumes that each industry uses transferable labour. It is assumed that the labour supply is completely elastic. This could be justified on the ground that the pool of labour used by the group of industries under consideration is small relative to the economy as a whole (this is the assumption proposed in Sraffa, 1925, p. 324). Or there is unemployment, or the disutility of the individual labourer does not change at all with the wage – both not very neoclassical assumptions.

Hence we are, in Samuelson's present paper, exactly where Sraffa stood in 1925. Since the supply of labour has by assumption been made irrelevant to the determination of the supply price, the latter

depends only on the degree of intensification of the cultivation of the lands, each of which is specific to the product grown on it. Nobody could deny, least of all Sraffa, that rising supply curves will be so obtained, and Sraffa's error could only consist in his assertion that the situation under consideration is a more special case than that of constant costs. Of constant costs we may simply speak in those cases where a clear tendency to increasing or diminishing returns is absent. A one-to-one assignment of factors to products seems more particular. In this sense, the constant cost box is less empty. But a general theory cannot be so obtained.

In fact, lands tend to be specialized, but this is the result of an economic process that must be analysed, as can be done using Sraffa's (1960) method of counting the equations; it should not be simply presupposed. And complete specialization, as in the Sraffa (1925)–Samuelson (1989 [the reference is to Samuelson's chapter above]) case, will not result often. Thus, if we have twenty lands, the twentieth being marginal, and three products could be grown on each, there will be nineteen rents and three prices to be determined, so that, of sixty feasible processes, only twenty-two coexist in equilibrium, implying that at least eighteen and possibly nineteen, but never all twenty lands will turn out to be specialized (see Sraffa, 1960, p. 77).

V

I have trespassed already on the space rightfully allotted to me, but I want to take up the challenge of the 'red herring'. I think that a more significant meaning can be attached to the standard commodity than emerges from the paper, although it remains an auxiliary construct. To me (Schefold, 1976, pp. 221–25; Schefold 1986), Sraffa's standard commodity is interesting not so much because of the *result* (a standard such that the wage curve is linear if wages happen not to be advanced, implying the useful and well-known analogy with the corn model) but because of its *derivation*. In this derivation, the point is not to show that prices of production are equal or close to labour values – something that Sraffa does not contend – but to show how and why prices change with distribution for a given technology. To see this, I focus on Chapter III of the book, not on chapter IV.

It is clear to the informed reader that the 'Prelude to a Critique of Economic Theory' cannot be understood independently of the context, i.e. the formal propositions are developed not just in order to obtain abstract propositions but also with a view to questioning traditional economic beliefs, and the formal propositions obtained have, because of their abstract nature, a meaning only within the broader framework

of traditional theory, both classical and neoclassical. Chapter III on the proportions between the input of labour to the inputs of means of production (how are they to be measured?) addresses the question of how changes in distribution affect changes in the prices of the outputs of single-product industries. Nowadays, after the introduction of matrix algebra to input–output systems, it is easy to give a formal solution to the apparent problems of circularity that arises where the tools for measuring the amount of inputs, i.e. prices for the given physical quantities, depend on the result of such measurements. The answer is to calculate prices of inputs and outputs simultaneously. But the calculation obliterates the economic processes leading to the formation of such prices and one is tempted to forget the economic tradition that might explain them.

Having considered simple conceptions of the numéraire (a numéraire commodity, then an arbitrary index) and having alluded to different theories of distribution (subsistence wage, given shares of national income) in the opening pages of his book, Sraffa allows the rate of profit to vary and shows that, if prices remain equal to labour values (they are equal if profits are zero), 'deficit' and 'surplus' industries will arise at positive rates of profit, and with a wage reduced from its maximum; therefore he considers a *dis*equilibrium and asks how it might be redressed. If labour values are $\mathbf{u}$, the input–output matrix is $\mathbf{A}$, the labour vector is $\mathbf{l}$, the vector of surpluses and deficits is $\mathbf{z}$, we have

$$\mathbf{u} - \mathbf{A}\mathbf{u} - w\mathbf{l} - r\mathbf{A}\mathbf{u} = \mathbf{z},$$

where w has been lowered a little from its original value of one and r has been raised a little from its original value of zero (there is as yet no wage curve showing which reduction of w would correspond to a given increase of r). Price changes on the input and the output side are found necessary to reduce each component of $\mathbf{z}$ to zero, and to 'achieve this object it is first of all the price ratio between each product and its means of production that one expects to come into play' (Sraffa, 1960, p. 14). One 'expects' something because there are differing traditions in economic theory. The answer of transforming values into prices of production only on the output side (redistribution of surplus value) is mistaken. (Marx was in fact aware that input prices also ought to have been 'transformed', but he neither had the analytical means to do it, nor did he see the implications.)

If Marx's solution does not help, one may turn to the neoclassical view, which here at first sight seems to be confirmed: high rates of return (surplus industries, with the corresponding component of $\mathbf{z}$ being positive) are associated with low capital intensities. But if the means of production of a capital-intensive industry are produced by labour-intensive industries, the value of the means of production of the capital-intensive

industry may *fall* in consequence of the change in prices (when values are being transformed into prices), so that it is not always necessary for a capital-intensive industry to raise its own price. Sraffa therefore here prepares the reader for Wicksell effects, which are presented in a more striking manner in chapter VI.

Sraffa's main point is that the clue to understanding the reaction of prices to changes in distribution lies not only in the proportions of labour to the value of means of production in the industry under consideration, but also in the corresponding proportions in previous industries that produce those means of production, and further on backwards in 'logical' time. The 'invariable' standard of value is therefore one from which the cause of the price changes in consequence of changes in distribution (namely, the unequal proportions of labour to means of production in the industry itself and in the industries producing the means of production of that industry and further backwards) is *absent*. One and only one industry, the one that produces the standard commodity, has this property of (infinite!) recurrence with equal proportions if the system is regular in the sense of Schefold (1971).

The argument cannot be worked out in greater detail here (see Schefold, 1986, pp. 607–15). Note that causality in this context means that causes are defined relative to traditional explanations in economic theories. *Any* numéraire is invariable in that its price does not change by definition. Sraffa's standard commodity is claimed to be invariable in the more specific sense that the causes that theories of all schools adduce to explain price changes in consequence of changes in distribution are *not* to be found in the standard industry, so that it is appropriate to take the standard commodity as numéraire. It so happens that a linear wage curve results if the wage is not advanced, but, if the wage is advanced, the construction also holds and results in a hyperbola.

The *Hilfskonstruktion* is thus useful for criticizing Marx's attempt to transform values into prices, but it also helps to visualize how prices of production systematically deviate from labour values as the rate of profit is raised from zero to its actual value in a thought experiment. The standard commodity can be constructed only for the system in actual use; it therefore relates actual prices to what prices would be in the same system at a different rate of profit. Since part of its explanatory power is based on the possibility of reduction, it is not only useful for those systems for which a reduction is not possible, i.e. for the majority of joint-production systems (for which it does in fact often not exist). It is obvious that a different standard obtains if the system changes because of technical progress or larger alterations in the actual rate of profit or shifts in demand. I regard it mainly as a didactic concept, i.e. as an introduction

to the logic of the classical theory of value and to puzzles of capital theory. Other uses of it can be made. Whether it was of actual help to Cambridge (UK) in the great debate I cannot say.

There are many other points in Samuelson's contribution that might be taken up, especially the transformation problem, but I do not want to defend the labour theory of value as a basis for an accurate modern analysis; the assessment of its historical function in the works of classical economists is a different matter. 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. Economists working in the classical tradition should be extremely grateful to Samuelson's inquiring comment, not only because he compels them to deal with questions for which neoclassical theorists have well-practised (though not necessarily correct) answers (such as how the demand for consumer goods is to be modelled), but also because he challenges them to develop the broader perspective of the theory of accumulation. In this field the classical authors developed their splendid vision of the period between the earliest phases of the industrial revolution and the century that followed. The question is what their 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 the Walrasian equilibrium either.

Acknowledgements

Reply

Inside every great scholar is a greater one. Albert Einstein, the stubbornest critic of quantum physics, ranked as a giant with Planck and Bohr in the creation of pre-Heisenberg-Schrödinger-Dirac quantum mechanics. Similarly Piero Sraffa, though it was no part of his intention, was led as if by an invisible hand to perfect time-phased mainstream microeconomics – not of course in the narrow corner of it where reside Clark and Wicksteed.

It is of this greater Sraffa that I sing. At the infancy of classical economics its scientists spoke prose and had equations and unknowns which were equal or unequal in number. When we count the number of Ricardo's equations and unknowns, we verify exactly what kind of demand and supply system his model is and learn that he can arrive at no determinate distribution of income that can be freed of the 'complications' of consumer tastes, demands, and time preferences. In the Pantheon along with Walras, von Neumann and Arrow-Debreu, Sraffa has earned pride of place. The critique of Marx, begun by Dmitriev and Bortkiewicz, achieves closure with benefits of Sraffian insights – as exemplified in the expositions of Seton, Steedman and Roemer. The fulfilment of Marx's tableaux of steady and expanded reproduction is achieved by the tools and techniques of Leontief, Sraffa, von Neumann and Morishima.

I welcome the uninhibited discussions of John Eatwell, Pierangelo Garegnani and Bertram Schefold. When scholars feel misunderstood, it is natural for them to wonder about motives. Time-consuming discussion is the only way to sort out the areas of disagreement and agreement. My regret is that an overcrowded research schedule and a tight publisher's deadline necessitated a brevity that makes for a tone of dogmatism and unamiability.

Alphabetical order for the authors is appropriate, with some inevitable repetitions. Because I received more than one version to react to, I cannot be sure I have responded to every important point and avoided responding to points later withdrawn.

Eatwell

(1) I had suggested that a revised edition of the *New Palgrave* rewrite the Eatwell–Panico sentence

Yet the basic structure of classical analysis is preserved [in Part II of Sraffa's book dealing with joint production and land] – the prices, the rate of profit, distributive variables (say, land rents) are determined by the conditions of production, given the wage.

Eatwell quotes the whole of his earlier paragraphs and declares I have misunderstood the intended meaning. Indeed I have, and a revised text seems to me all the more needed. The reader may judge from my account whether the misunderstanding was an 'ideological device' involving removal of 'sentences from their context', misrepresentation of 'arguments', and 'erection of straw men'. Or whether it was a genuine attempt to isolate what is valid in a 'classical' paradigm as against a post-1870 paradigm.

Can the distribution of income be analysed in some classical fashion independently of the composition of demand? Can relative prices of factors and the profit rate have their equilibrium determined by technology independently of subjectivist utility and time preferences?

The answer given by mainstream economics is No.

The erudite Jacob Viner, my mentor at the University of Chicago, counselled: 'Try to read an author for sense, not for error.' I tried. The indicated Eatwell–Panico sentence, if I extended to the word 'wage' the implicit adjective 'real', could be construed *not* to be an untruth in Part I Sraffa – this by virtue of the 1949 non-substitution theorem. In the face of this non-malicious interpretation, what was I to think of the preceding sentences, which said:

In Part II...[of] Sraffa...[with] multi-product industries and fixed capital, and...economies with more than one non-reproducible input...Yet the basic structure of classical analysis is preserved...

and then followed the sentence that I declared to be untrue as applied to Part II.

Was I wrestling with a straw dummy? If readers were to plough through my tedious correspondence files with Sraffians, they would realize how many authors believe that relative prices of joint products are, for a fixed profit rate, invariant to [sufficiently small?] changes in the composition of demand [almost always?].

Since, like Oliver Twist, I am asking for more revisions, I suggest that the two *Palgrave* authors re-examine all these mooted passages. In particular, there will be found to be no valid 'contrast[s] with neoclassical theory, in which determination of prices is dependent upon knowledge of the functional relationships between supply and demand' – in any Sraffa model involving, say, corn produced by labour and land while cloth is produced by labour and corn. When landowners change their tastes for corn and cloth, that changes relative prices and the distribution of rents and wages in Wicksell's demand/supply neoclassical fashion (which is also Smith's and Ricardo's). Etc., etc.

(2) Were generations of readers of Sraffa's 1926 classic right or wrong to believe that the category of increasing cost and rising supply price was demonstrably an empty box? I said they were misled. Eatwell argues that, in 'general equilibrium', a shift in relative demand toward one good does raise its competitive price, but argues that it is an irrelevancy to bring in general equilibrium since Sraffa's critique is exclusively directed at partial equilibrium.

I agree that it was the fuzzinesses of Marshall's partial equilibrium that Sraffa grappled with – and, I would add, that ultimately mired Sraffa

down in a basic non sequitur. But, after I have solved for Marshall and Sraffa *all* the ambiguities of partial equilibrium, why does Mr Eatwell not applaud my debunking Sraffa's claim that 'the old and now obsolete theory which makes it [price] dependent on cost of production alone appears to hold its own as the best available'.

It is not the 'best available'. It was not the best in 1926 (or in 1925). My exact example, which Eatwell says 'fulfils exactly the conditions Sraffa laid down for the construction of a partial equilibrium industry supply curve', refutes Sraffa's attempt to base price on cost alone to the exclusion of *dd* demand curves. Let marginal utility of wine rise relative to that of rye, and the box of rising supply price will be seen to be non-empty – even 'approximately'.

Rereading the *Foundations* (Samuelson, 1947, pp. 75–80) passage Eatwell commends and the Samuelson and Nordhaus (1985, 479–80) passage he finds 'dubious', I find nought to react to in connection with the 1926 Sraffa article or with the expositions themselves.

(3) Eatwell is right that, if textbook writers like me use examples and diagrams that I know to be so oversimplified as to be false representations, it would be churlish to fault him for using Sraffa's *standard commodity* as a simplifying device to explicate how an input–output system with one primary factor has its real prices determined uniquely for each profit rate.

My point is missed. My point is that the *standard commodity* does *not* clarify anything. It does not illuminate 'reswitching'; Wicksell effects; the incidence on relative factor shares of accumulation, innovation, time preference, or tastes changes; the labour theory of value's domain of applicability; the chimera of 'absolute' value;...When more than one technique is competitively viable, no *standard commodity* obtains; under various decomposabilities, none obtains. Its disappearance is no loss; and, in the one-technique, indecomposable single-products case, its appearance is no gain.

My pointing all this out is hardly odd. The reason that the concept does not fade into an appendix on *local* eigenvectors is that Piero Sraffa, late in the task of editing his magisterial Ricardo, bethought to make some sense of notions of *absolute* value: rather than dither between shortest-lived shrimp on the seashore and ancient-trees' masts, or diddle with goods of average time intensity, neo-Ricardians aspired to the objectivity of a Frobenius–Perron matrix's right-hand eigenvector as weights for a market basket. One inessential error in Marx's transformation algorithm could even be lightened when the economy is in a *standard* gross state.

Fun is fun, and heaven knows mathematical economists have their turnpikes and other pebbles on the beach. The only caution is to know the difference between a pebble and a pearl.

Garegnani

(1) I shall concentrate on those points in my critique of Sraffa's classic 1926 paper that have not already been addressed in my reply to Eatwell.

A new point here is that, in Sraffa's longer Italian version of 1925, he is said to have endorsed my position – namely that, as a matter of exact logic, the box of increasing-cost, rising supply is not empty even in an impeccable partial equilibrium model. (Wheat and wine use respective lands specialized for them.) Two people seem to deserve congratulations: Piero Sraffa and I.

How was one to know in the Viner or Schumpeter seminars of 1934–5 that the 1926 author knew better than he wrote? The many merited reprints of the 1926 classic never carried author's alterations to warn of this.

The issue has never been 'Marshall's partial-equilibrium approximations' versus 'classical economics'. It has been Menger and Jevons and Walras versus Ricardo, Mill and Cairnes. It has been Böhm-Bawerk, Wicksteed, Pareto and Wicksell against Marx and Ricardo – with Dmitriev, Bortkiewicz and Seton keeping the score.

What a cleaned-up version of Sraffa (1926) establishes is how *nearly empty* are *all* of Marshall's partial equilibrium boxes. To a logical purist of Wittgenstein and Sraffa class, the *Marshallian partial* equilibrium box of *constant* cost is even more empty than the box of *increasing* cost. I should have said that in *Palgrave* and in my revisionist paper on Sraffa. Piero Sraffa should have said that in 1925 and 1926.

Proof: When all goods use all factors in the same proportions (and a universal non-substitution theorem obtains), which is the most favourable case for 'constant costs', then can Marshall write down *rigorously* a *partial* equilibrium analysis? No. The *ceteris paribus* assumptions he needs do not obtain. QED. As we let Sraffa (1926) fade into history, we are left with no *empirical* primacy for the constant-cost approximation.

(2) Garegnani says I have read Sraffa (1960) with marginalist eyes. I say I have read it with von Neumann eyes, Leontief eyes, Debreu and Koopmans eyes, Dorfman–Samuelson–Solow eyes and Morishima eyes. Later I reread it with Garegnani, Pasinetti and Schefold eyes.

I do reread Ricardo and Smith with all those same eyes and I deny that Ricardo failed to understand how changes in demand and outputs altered

factor prices and relative goods prices. The blindness, if any, is in Mr Garegnani's caricature of a classical economist and there is nothing Clarkianly neoclassical about recognition of this pre-marginalist banality. Who begrudges Mr Garegnani *his* two-stage procedures? But why bind *them* on Mr Ricardo or Mr Mill?

When Ricardo addresses a change in tastes toward labour-intensive goods during the Napoleonic War's need for standing armies, he perceives that this raises the intermediate-run real wage relative to real land rents. He perceives that, in *his* longer run, this adds to the equilibrium population. He perceives that this alters the distribution of income in all runs.

Neither in 1814–15, nor 1817 nor 1823, would Ricardo make the elementary mistake of considering the real wage–profit rate trade-off to be independent of the abundance or scarcity of land. Ricardo's glass is alternately half-empty and half-full. Sometimes his real wage rate cleaves to a specified subsistence level; sometimes it evolves downward toward that asymptote; sometimes the industrial revolution is catapulting it upward. Ricardo's long-run profit-rate plateau is even more weakly hypothesized.

My 1959 *QJE* articles on the Ricardian system showed that I have no marginalist qualms about going all the way to extreme long-run Ricardian poles, culminating in his physiocratic *Land* (not Labour) Theory of Value. But why should I or Garegnani saddle this singular case on the general reader in political economy? That seems antiquarian decadence. It is derogatory toward the classical writers and their system, and for no necessary or even useful purpose.

Garegnani's further remarks about 'squareness' of joint-product matrices and *local* price-ratio invariances are taken care of in my discussion of Bertram Schefold's Comment and can be omitted here.

(3) On the *standard commodity* there is overlap between the Eatwell and Garegnani positions. That vectoral concept, Garegnani reassures us, 'is not there to validate anything in particular...[but] to render "more transparent" what is valid independently of it...' (Garegnani, n. 15, gives as an example the use of x^* in $(1 + r^*)Ax^* = x^*$ to demonstrate that, for $r^* < r$, $P = Wa_0(1 + r)[I - A(1 + r)]^{-1}$ cannot have a positive P/W vectoral solution.)

On p. 59, Garegnani compares the piecewise (real-wage profit-rate) trade-offs over *local* intervals of $1 + r$ with piecewise different *standard commodities*. The logic of the two cases is disparate: there is one and only one trade-off locus no matter (1) how variable the techniques, (2) how decomposable or indecomposable the technologies. By contrast, *no* unique market basket defines a real wage linear in the profit rate when

techniques are variable or decomposable. A locus that is piecewise linear is *not* linear, and as Garegnani well understands the linear pieces do not even belong on the *same* $W / \sum P_j x_j$ vertical axis! It is because, as my critics so well say, the construct does nothing essential for them that its non-existing cases occasion no trouble and so little notice. My Åkerman *Festschrift* positive-profit-rate non-substitution theorem, as I review its explication, does not benefit from Garegnani's proposed vectoral exposition.

I devoted most of my 1983 Alexander Ehrlich *Festschrift* tribute to discussing Marx without matrices, using a one-commodity case to demonstrate how scarcity and plentitude of capital good(s) determine the rate of profit and the real wage. It is a case so singular as to make transparent the superfluousness of the *standard commodity* concept. I therefore am not sure whom the joke can be on if Garegnani somehow infers that I have given the *standard commodity* more than its due. Zero is hardly an excessive rating.[1]

(4) Garegnani's section 4 deals with the labour theory of value. Once again he claims that a corn-only model that Ricardo used in 1814–15 discussions with Trower and Malthus does cogently vindicate the labour theory of value. In this connection Garegnani appeals to Sraffa's 1951 *Introduction* to Ricardo's *Principles* for an alleged cogent demonstration that Ricardo had detected and overcome a basic Smith flaw and had done so by arriving at the labour theory of value. And, he proclaims, Ricardo's Herculean task was creatively buttressed by Marx's 1867–1885–1894 paradigms of equalized *rate of surplus value* and Marx's successful *Weltanschauung* concerning the 'transformation' problem.

I shall rebut by showing that a corn-only model violates any labour theory of value as fundamentally as the general *n*-good case does. I shall show that Smith's lowering of the profit rate as a result of more capitals competing with each other is transparently vindicated as a

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[1] I quite agree with Garegnani that a square $[a_{ij}]$ that is *de*composable may have associated with it a subsistence wage vector, $0 \leq [m_i] \neq 0$, such that its associated 1957 Seton matrix, $[s_{ij}] = [a_{ij} + m_i a_{0j}]$ is *in*decomposable and possessive of a unique (normalized) column eigenvector. Such a Seton *standard commodity* is not Sraffa's 1960 *standard commodity*. Sraffa tried to build on the rock of technology. The Ricardo circle knew well that any subsistence wage vector adjusted endogenously to land/labour scarcities: in densely settled Ireland, the potato superseded meat and grain; Ricardo and Mill, along with the later Marx, reduced their *subsistence* model of wage supply to a virtual convention once they divorced subsistence from a hard physiological basis and could make it fit *ex post* any and all observations. Once we augment the direct labour vector $[a_{0j}]$ by a direct land vector $[a_{-1j}]$, the Seton $[s_{ij}]$ matrix and standard commodity lose their autonomous dependence on the (a_0, m, a) coefficients.

logical and empirical possibility against Ricardo's Smithian strictures precisely in the corn-only case! Garegnani's attempt to defend Marx's detours in the transformation problem will be seen to lack cogency.

Samuel Hollander and Garegnani dispute textual matters. Was there an 1814–15 lost-Atlantis model for Trower, Malthus and Ricardo to debate about? Personally, I'd be overjoyed if a manuscript find in some English country home completely vindicated Garegnani. I shall fabricate the find.

Corn and labour and land inputs can produce corn output after one period. (Cloth is similarly producible but by labour and corn as inputs.) With corn Sraffa's only *basic*, if there is to be a steady state, the profit rate that prevails in autonomous agricultural production must be matched by the profit rate elsewhere.

Our 1815 find has Ricardian arithmetic. 1 of corn can be produced on 10 acres with 1 of labour and 1/10 of corn inputs. Or, if we halve the acres, we can double the non-land inputs – and so forth in the continuous land–non-land mode of the classicals. Also, to give Smith his fair chance, the example permits the following alternative techniques to the (1 labour, 1/10 corn) technique: $(\frac{1}{2}, \frac{1}{4})$, $(2, 1/20)$.

Now we calculate how Ricardo's labour theory of value is neither 100 per cent right, nor 93 per cent right *à la* Stigler. It is simply a wrong one-parameter theory of value when every schoolboy – whether named Adam, David, Léon, Karl or Pierangelo – knows that only a three-parameter theory of value that gives proper scope to rent, wages and interest can properly describe (a) the distribution of income, (b) the interest rate, and (c) the $P_{\text{cloth}}/P_{\text{food}}$ and $(P_{\text{cloth}}/W, P_{\text{food}}/W)$ ratios. All this holds in every run. Before population changes, a new accumulation of corn seed will so compete down the P_{corn}/W ratio as to lower the switching-point profit rate *à la* Sraffa. Despite Smith's archaic language – 'more competition of capital' to describe a rise in the competitive supply of corn seed (within the same regimen of non-monopolistic competition) – Smith is right on target. (We are fortunate that Ricardo was such a fuzzy thinker since we owe his valuable *Principles* to his captious critiques of Smith!) Why did Editor Sraffa not stress that Ricardo understood the necessity for a three-parameter theory of value – at least did so more than 7 per cent of his time?

The reader may investigate how a fall or rise in the corn subsistence wage will alter the long-run interest rate, the population, and the goods' terms of trade. Or, how a change in landlords' tastes toward cloth will alter, by supply and demand, short-run $P_{\text{cloth}}/P_{\text{corn}}$, until population moves to restore the P_{corn}/W ratio. When cloth becomes part of the

subsistence ration, profit in agriculture no longer unilaterally sets the profit rate. Etc., etc.

Now what besides rhetoric are we offered in the way of insight into our 1815 model by its 1867 *Mehrwert* model? In neither short nor long versions of Marx's transformation problem can I find a cogent core. The 1815 model presents him with his opportunity. I can report to the reader that I put it through the 1867, 1885 and 1894 Marx paces and they failed perfectly to provide a single valid insight. (I wrote down volume II *Mehrwert* tableaux above, below and at switchpoints. Always irrelevancies.) The reader should judge whether Ian Steedman's *Marx after Sraffa* needs any revisions in the light of the Garegnani contentions.

Let me add that I have gone back to reread exactly what Piero Sraffa wrote in 1951 and 1960 and find no words that purport to reduce a three-parameter theory of value down to a one-parameter theory of value (save possibly for the passage in his *Introduction* where he without comment refers to Ricardo's (mistaken) attempt to get rid of the complication of land for exchange theory by considering goods as produced (endogenously?) on marginal zero-rent land.)

(5) Mr Garegnani has served us all well in describing more fully Sraffa's Italian undergraduate thesis.

I completely agree that the merits of a scientist's assertions are independent of the political party he votes for or the Church he attends. When a great scholar has passed into history, those of us who knew him and his works form part of the enterprise that constitutes his scientific biography. If Isaac Newton gave me as a reference for a job at the Mint, I would not deem it necessary to mention his penchant for alchemy; but, if that penchant had aught to suggest for his theory of optics, his scientific biographers will want to weigh contemporaries' impressions of the matter.[2]

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[2] One of Keynes' biographers, feeling a need to justify his own voyeurism, quoted Schumpeter's disparagement of Keynes as a policy adviser influenced by his childless status. This early instance of Bloomsbury-bashing no doubt plays into the prejudices of conservative anti-Keynesians. But, if it were the case that Schumpeter's arrow hit its target, we could not rule it out of civilized court. Actually, though, as I had to argue at Harvard's 350th gathering, Schumpeter was wrong on this Keynes issue. The man who wrote (in quite another 1923 connection), 'In the long run we are all dead', was a sentimental English patriot in the Edmund Burke style, a loyal son of Eton and King's and of the intellectual middle class, with a concern for posterity that Joseph Schumpeter and I could well envy. Sociobiologists like Hamilton and Wilson know that even Popes have nephews and I am not engaging in *double entendre*.

Schefold

From no one have I learned more than from Bertram Schefold. At bottom we are in agreement, not disagreement.

(1) Schefold's first comment deals at some length with the undisputed difference between *steady-state* regimens and *transient* regimens when relative prices foreseeably are altered by evolving scarcities of produced-goods endowments (possibly on a rendezvous course with an asymptotic steady state). Mainstream theorists, more lacking in interest in Sraffian matters than I, would recognize all this as part of mainstream economics: Schumpeter, Fisher, Ramsey, von Neumann, Samuelson, Malinvaud, Koopmans, Hicks, Morishima, McKenzie, etc. Dozens of the chapters in *Collected Scientific Papers of Paul A. Samuelson* (1966c, 1966d, 1972, 1978b, 1986) deal with precisely such *transient* and *steady-state* contrasts. I hail Sraffa (1932) for early stimulus, which enabled a maiden paper of mine to identify and correct a *General Theory* blemish on own interest rates by showing that the same no-excess profits are earned on all goods when relative prices are foreseeably changing – and this *independently* of the numéraire-good used and the choice among heterogeneous own-rates of interest. Heady stuff for a 21-year-old fledgling to correct a Keynes!

Nothing in Schefold's wording should deter his readers from realizing how sharp was my revisionist scalpel and how deep into neo-Ricardian arteries it cut. My censure of the labour theory of value (of Ricardo's editor; of the Garegnani belief that the lost 1815 corn-only model of Ricardo somehow saves the face of the labour theory and defends Marxian uniform-rate-of-*Mehrwert* analysis as a non-detour; and of what I had regarded as the Eatwell–Panico belief that Sraffian joint production leaves relative prices invariant under taste and demand changes) – all these critiques were directed to Schefold's present case of *permanent* rather than transient scarcities.

(2) In a long section, Schefold deals with joint products and with land as a primary factor along with labour. Here is one such permanent scarcities case. Claret is produced by labour largely on cool vineyard lands; vodka by labour largely on potato-bearing plains. Then 1926 Sraffa or 1989 Sraffians will err if they think a shift in tastes from wine to spirits will involve unchanged terms of trade between them in the *longest* run.

Every point in my revisionist critique can be purged of *all* neoclassical elements. Discrete activity analysis *à la* von Neumann and not Clark or Cobb–Douglas marginal products can push my scalpel inward. My contentions do not need to rely on indifference-contour utilitarianism; but

like Adam, David and Stuart I would not want to deny that, when goods get dearer, people alter their consumption of them.

Even when the same *permanently* scarce lands can produce all the different goods, Ricardo, his editor and approving reviewers of the editor such as Stigler are simply dead-wrong to think that the complication of Smithian rent can be removed as a deviation from the labour theory of value by utilizing the *no-rent margin* for goods' relative cost comparisons. As Thünen, Wicksell, Frisch, Robbins and I have many times demonstrated, where the 'margin' (extensive or intensive) falls is an *endogenous* unknown dependent on the composition of demand and tastes. Ricardo himself occasionally lapses into good sense on the point: when land comes to be occupied by greater population, corn price rises relative to hair cuts; a permanent state of warfare, with its relative intensification of derived demand for labour-intensive soldiering, raises the population density of a region as the fruits of rent are given over to sustaining more people to be cannon fodder; in any time-run when labour and land are relatively fixed, their factorial terms of trade and shares are demand dependent.

My 1815 example of Garegnani type has already shown that a three-parameter theory of value is irreducibly needed. None of the above facts is possible under a true labour theory of value. Nor are they only 7 per cent possible under realistic conditions, as Ricardo lamely claimed with the support of one arbitrary numerical example that went down better with Stigler's (1958) computer than it did with mine.[3]

(3) Schefold is no dogmatist, and not even much interested in antiquarianism. He has a mathematical point to make in connection with joint products and land-and-labour scenarios. He admits that large enough changes in needs (or in tastes) can invalidate the non-substitution

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[3] Schefold asserts that Saucier (1981) has shown that something I said in 1987 *Palgrave* was wrong. I deny error. The mathematics of maximization endorses what I wrote. The own competitive supply response to a single change in output price can never reverse sign. Not only is this spelled out in the 1983 enlarged edition of *Foundations*, but before the birth of Dantzig's linear programming I had proved in Samuelson (1946) from the logic of maximizing that Giffen sign reversals could not occur in a linear programming problem even more general than Stigler's least-cost diet problem. My *QJE* 1959 Ricardo Appendix, written in ignorance of Frisch's exploration of the 1930s, showed precisely how many qualities of land affect Ricardo's rents. Whatever Saucier validly established, it was not error in my 1987 sentences. The general supply inequality,

$$0 \leqq (\Delta p_1)(\Delta q_1) + (\Delta p_2)(\Delta q_2) + \ldots$$

is valid and entails, when all Δp_j vanish except for a rise in p_1, that q_1 cannot fall even when (in Schefold's words) 'several lands and agricultural products are involved'.

theorems that make equilibrium price ratios independent of the composition of demand. *Globally*, Schefold subscribes to a three-parameter theory of value. But, *locally*, he insists that *ss* supply curves can be regarded generally as flatly horizontal.

Speaking loosely, we can say that shifts in *dd* curves can cause intersections with *ss* curves to occur at different P_i (or P_i/P_j) levels – as the Mill–Ricardo trade theory and the Jevons–Walras paradigm stressed. But, Schefold insists, almost all vertical *dd* curves find their equilibrium intersections on the flats of the *ss* steps and not on the risers. *Locally* [my words, not his], the labour-only invariances do hold.

To a mathematician, the issue involves 'squareness of the effective submatrix', whether it is generally true that the number of goods and processes (counting land as a joint product) endogenously turn out to be equal. Such a contention is almost correct. If it were completely correct, that would not suffice to save the labour theory of value; but it would be an interesting fact.

First, I stress fundamentals. It was a well-known Frank Graham fallacy in the second quarter of this century that *n*-good and two-country comparative advantage models – or *n*-good and *m*-country models – had no need (*No* need? Virtually no need...) for demand functions to determine equilibrium terms of trade. The same linear programming calculus of rank of submatrixes of rectangular matrixes was involved. In the end, Graham was seen to be quibbling. The reader can predict that, in the end, just before everyone admits that land rents do affect relative prices, it will be argued that 'At least in proper post-Smith models, such and such holds...'.

Thirty years ago in the *QJE* I worked out the linear programming of discrete Ricardian models with differing land qualities. Even in the timeless case or the zero-profit-rate case, the constancy of relative prices entailed by the labour theory of value or the land theory of value was denied. Dantzig's fundamental theorem of linear programming – that extrema can always occur on vertex points – was utilized

Digression on 'squareness'

We are all in Bertram Schefold's debt since he has been early and preeminent in working out Sraffian joint-production relations. Still that literature has a long way to go to catch up with the general von Neumann–Koopmans inequality–equality analysis of the following type:

$$[P_1 \ldots P_n] \begin{bmatrix} b_{11} & \ldots & b_{1m} \\ \vdots & & \vdots \\ b_{n1} & \ldots & b_{nm} \end{bmatrix} \leqq W[a_{01} \ldots a_{0m}] (1+r)$$

$$+ [P_1 \ldots P_n] \begin{bmatrix} a_{11} & \ldots & a_{1m} \\ \vdots & & \vdots \\ a_{n1} & \ldots & a_{nm} \end{bmatrix} (1+r)$$

$$W, \ P_j \geqq 0, \ b_{ij} \geqq 0, \ a_{ij} \geqq 0, \ 1+r > 0, \ m \lesseqgtr n \qquad (1a)$$

$$[b_{ij} - a_{ij}] \begin{bmatrix} x_1 \\ \vdots \\ x_m \end{bmatrix} = \begin{bmatrix} C_1 \\ \vdots \\ C_n \end{bmatrix} \geqq 0, \qquad [a_{0j}] \begin{bmatrix} x_1 \\ \vdots \\ x_m \end{bmatrix} = L > 0$$

$$[P_j] [C_i] = WL + r[P_j] [b_{ij} - a_{aj}] [x_i] > 0$$

$$C_i \geqq 0, \ r \geqq 0, \ x_i \geqq 0. \qquad (1b)$$

Even when generalized Hawkins–Simon conditions are assumed for the (b, a) matrices, we are infinitely far from a determinate unique solution for our steady-state system. Schefold proposes his own version of a theory of consumer 'needs'. Whether a quorum of modern economists will find it of interest and worth does not have to be pronounced on here: it consists of specifying that the (C_i/C_1) ratios be *exogenously* specified at any and all possible non-negative levels; as we shall see, it is a non-generic specification in the sense that it is embeddable in a more general manifold in such a way that (some of) its qualitative properties are lost by an epsilon-small deviation from its Schefold stipulation.

The purpose of all this was to try to show that, *almost always*, the competitive solutions to (1) will involve 'square' Sraffian production: thus, even when $m \gg n$, $m - n$ of the x intensity levels will be competitively non-viable and the number of x-activities used positively will equal the number of goods produced positively.

Specify six b_{ij}'s, six a_{ij}'s, three a_{0j}'s, one Schefold's needs variable $C_2/(C_1 + C_2)$ or c, and one r, all of which can generically be supposed to be positive. The remaining dozen variables can be specified to obey *any* positive joint-probability density function:

$$(b_{11}, \ b_{12}, \ b_{13}, \ b_{21}, \ b_{22}, \ b_{23}, \ a_{11} \ldots a_{23}, a_{01}, \ a_{02}, \ a_{03}, \ c, \ r)$$

Schefold's c can take on any value in the $[0, 1]$ interval and r any value in a neighbourhood of zero.

I now enumerate the possible results.

First, part of the 17-variable space will not permit of any steady state with positive net consumptions. Never mind: redefine the measure so that the feasible points in the Hawkin–Simon region add up in probability to unity with r permitted to be positive in some interval above zero.

Second, part of the space falls into that 'special domain' where, at $r = 0$ and its neighbourhood, one of the three processes is competitively dominated: say, x_3 is the zero one. In the following special domain, Schefold and I are quite agreed that the non-substitution theorem of Sraffa's Part I is in effect satisfied and the C_2/C_1 ratio does *not* affect the P_2/P_1 equilibrium at each specified r. This special domain is where

$$\begin{bmatrix} (b_{11} - a_{11}) & (b_{12} - a_{12}) \\ (b_{21} - a_{21}) & (b_{22} - a_{22}) \end{bmatrix}^{-1} = \begin{bmatrix} J_{11} & J_{12} \\ J_{21} & J_{22} \end{bmatrix} > 0 \tag{2}$$

Incidentally, Schefold believes I underestimate the 'probability' contained in this special domain: I don't know how to decide whether he should be agreed with in this (in the case of either $n = 2$, or $n > 2$ where 'diagonal dominance' becomes a more intricate concept).

Third, since the above special domain has only fractional probability, we face positive probability for the traditional cases of joint production described by Smith, Longfield, Mangoldt, Marshall, Hubert Henderson and a horde of pre-Sraffa writers. It is sufficient to contemplate the hoary wool–mutton example, or the taxicab-worked-hard-or-easy-when-new example, to demonstrate the following:

There is positive-probability measure, not zero-probability measure, that competitive production is *not* 'square' in the Schefold universe. Square production is *not* 'almost always' true.

I have published examples like the following: 1 labour produces 1 red sheep that contains 3 of wool and 3 of mutton. Or: 1 labour produces 1 blue sheep with 4 of wool and 1 of mutton. Or: 1 labour produces 1 green sheep with 1 of wool and 4 of mutton.

When people want wool and mutton in exactly equal Schefold proportions, a singular case that almost never happens, only red sheep are produced. This deviation from squareness is trivially rare and I forbear to count it against Mr Schefold.

Now consider the positive measure in which people 'need' a bit more of wool than of mutton. Mr Schefold gets his squareness. Red and blue sheep are both produced and only them. *Local* swings in his C_2/C_1 ratio

leave P_2/P_1 invariant at $1/2$. The blade of vertical dd demand is weak relative to the ss supply blade (on the flat step).

Schefold well understands that a shift of *his* demand to wool being newly needed in fractional amount compared to mutton again produces squareness at first: only red and green sheep are produced; and, so to speak in neoclassical fashion, the newly prized wool has its relative price coaxed up from $1/2$ to 2 (the increasing-cost case!).

So far squareness? Yes. But further shifts in the Schefold needs parameter c, below $1/3$ or above 3, loses squareness with positive measure.

For C_2/C_1 above 3 only green sheep are competitively producible and we have lost squareness. With similar positive probability C_2/C_1 will be below $1/3$, only blue sheep will be producible, and squareness will be lost. With these same probabilities one of the goods will have zero price – or, if disposal costs are unavoidable, even a negative price.[4]

Indeed, if red sheep had their productivities grow from (3 wool, 3 mutton) to above $(4, 4)$, for *almost all* Schefold's C_2/C_1 ratios, squareness and all-positive prices would be lost.

Economic squareness's universality is even more limited by the consideration that extreme price changes realistically modify even the neediest of needs. Schefold sees the point, even in exaggerated form, when it is a question of technology and innovation. For him, Necessity is indeed the mother of invention, and Nature indeed abhors the vacuum of a redundant harvest and a free good. But, beyond the thin line of his needs psychology, diplomatic recognition is withheld.

Schefold and I agree that, when C_j demanded is affectable by price, the probability measure of non-square equilibria is definitely positive even when all P's are stipulated to be positive. By stipulating *vertical dd* demand curves, almost all his intersections with the ascending ss stairs are on the flat of steps and not on the risers. The Schefold construction is not *generic*, but rather infinitely unrobust. When he lets his dd's be tilted ever so little from the vertical – and whose theory is so exact as to permit no perturbation at all? – Schefold will encounter positive measure of non-squareness cum positive prices and intersections of dd on ss risers.

Does much of this smack to the reader of Middle Age casuistry concerning how many angels can dance on the end of a pin? If so, join with

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[4] *Editorial note*: After some hesitation, I use my discretion as an editor to clear up a possible misunderstanding. According to the example, we either have an additional disposal process or only one process and one good with a positive price so that the system that actually emerges *is* square according to *my* definition. I have consistently proposed that only goods with non-zero prices and processes with non-zero activity levels should be counted (p. 196, p. 309); my zero-probability theorem rests on this assumption. (BS)

my complaints and address them to the appropriate post-Sraffian authorities.

In any case the *global* truth that subjectivist demand alters Part II price ratios save in the special domain described above can no longer be in dispute and is independent of the angels-on-pins squareness calculi. Should one laugh or cry when a commentator admits that *local* invariance is compatible with global lack of invariance but excuses not using the local/global distinction because it has become 'controversial in the Sraffian literature'?

Approximation theory can save no argument or the face of any arguer. Consider a Clark–Solow paradigm in which joint production never involves as many techniques as there are goods, in which positive prices are determinable but only by demands–needs considerations, and in which everyone agrees that land invalidates the labour theory of value and *all* nonsubstitution theorems. Piero Sraffa would turn over in his grave to contemplate such a marginalist's orgy. Nonetheless, Mr Schefold and Mr Garegnani will agree with me that it is a trivial theorem (of approximating an arc by broken line segments) that permits us to specify Sraffa-discrete models that come as closely as we like to having *all* the qualitative and quantitative properties of the Clark–Solow heresy! (As tit for tat, Clark–Solow models come as close as we like to having double-reswitching, 'perverse Wicksell' effects, and plateaus of consumption that both rise and fall with the interest rate. Philip Wicksteed and Bates Clark had a lot to learn from Piero and Joan!)

(4) The reader's patience and my time are by now exhausted. Concerning Schefold's final effort to find a useful role for the *standard commodity*, let me be over-brief. In the single-products case with a single set of $[a_{ij}, a_{0j}]$ coefficients, P_i/P_j ratios are known to be able to vary with $1 + r$ like the ratio of an n'th degree numerator to an n'th degree denominator. Many ups and downs are, in general, possible when n exceeds 2. If the reader learns something further from the *standard commodity* construct that is deemed interesting, I applaud from a distance.

However, as soon as we admit the realism of *alternative* viable techniques at different profits rates, *all is lost*. Literally any (and *all*!) qualitative patterns for P_i/P_j can occur and none of the multiplicity of local-piecewise straightline-segment loci puts limits on what can qualitatively obtain.

There is a paradoxical turnaround. I used to have to counsel my neoclassical friends to give up the oversimplification that Garegnani, Pasinetti and Morishima showed them does not exist. Now I must nag my Sraffian friends to give up untenable *standard commodity* oversimplifications.

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Returns to scale, demand, money and interest, and the classical tradition

Sraffa's hits and misses

Paul A. Samuelson

Pressure of other research at first prevented me from responding favourably to the Editor's request for a new paper on Sraffian economics. However, I was glad to have my 1990 revisionist paper included in this colloquium, and did welcome the suggestion that a new Addendum be included. If a scholar in his ninth decade is to record his considered opinions on an important topic, it had better be a matter not of *when* but of *now*. So, reconsidering, I do offer here some further informal analyses.

Dr. Samuel Johnson said that being hung in the morning greatly clarifies the mind. Nonsense. It is more likely to paralyze coherent thought. True though that as the days grow shorter, one does dispense with nice diplomacies and ancient jockeyings for victories. Knut Wicksell (1919) at long last wrote out exactly what he faulted and admired in Gustav Cassel's work. To exaggerate a bit, it was a case of then or never, and on a take it or leave it basis posterity is the richer for this.

1   The one basic novelty

What did I learn from Piero Sraffa's 1960 classic? One thing. An important thing.

Here, too briefly, is that one special thing I learned from Piero Sraffa. In much of this section I quote from my paper in honour of Pierangelo Garegnani (see Samuelson, 1999).

A. Long before 1960 one understood that, in general, no scalar magnitude can denote what is the 'accumulation of capital' when a society abstains from present consumption to effectuate a permanent rise in potential future consumption. The capital/output ratio, as Joan Robinson (1956) demonstrated, is a treacherous guide because of

'Wicksell' and other effects. No reliable independent meaning can be given to 'more or less roundaboutness', or to 'degree of mechanization' and other measures of capital 'intensity'.

B. Even when capital is intrinsically vectoral rather than scalar, its real return, as measured by its *steady-state* or stationary-state rate of *interest : profit*, is indeed a *scalar* parameter in equilibrium. For each rate of interest, r, there is a determinate maximal level of sustainable '*consumption*', c, vectoral (or as a *scalar* once the market-basket composition of consumptions is specified).

C. Around 1960, one could therefore still describe the following process of capital accumulation: when society is not already at a golden-rule state of technology, by sacrificing some vectoral amounts of current consumption over a finite time period, it can achieve a permanently higher time path of (vectoral) consumption forever. In a convex technology (of Sraffa or von Neumann type, or of Clark–Walras neoclassical type), an *intertemporal law of diminishing real returns* invariably obtains for the vectors involved. None of that is vitiated by possible 'reswitching', Wicksell effects, joint products or anything else.

D. Although any close reader of Irving Fisher's 1907 *The Rate of Interest* should have known better, I hoped around 1960 to be able to summarize the essence of C above by asserting that, as r rises above the golden-rule rate (of, say, zero), c of consumption must if anything fall in the entailed stationary state equilibrium. If we can write c as a function of r, $c = f(r)$, then

$$\text{Max } c = \underset{r}{\text{Max}} f(r) = f(0), \qquad f'(r) \leq \text{ near } r = 0$$

There is no error in the above *local* relation. Where my thinking went wrong was in believing that $f'(r)$ and $(\Delta c)\,(\Delta r)$ had to be (if not zero) negative.

Although I may never have put such a false conjecture into explicit print, it was from brooding over Sraffa that I learned the truth, that

$f(r)$ can rise – but not to above $f(0)$ – for ranges of r a finite distance above the golden-rule r^*.

It may be added that Liviatan–Samuelson (1969) had, by another route, fabricated a one-capital-good joint-product model for which $f(r)$ is a single-valued, falling for r near 0 but recovering part way for an intermediate interval of r, and then falling indefinitely. It is evident that convexity of technology does not imply convexity of steady-state $[f(r),\ w(r)]$ loci.

When I chanced to write the above to a French savant, he objected: 'But that is nowhere in Sraffa! Never did he speak of golden ages. And too rarely did Sraffa leave the realm of price dualities in an input–output model to elaborate on its quantity dualities.'

I replied: 'Each of your words is true. But you are too young to recognize the innuendo of the author. Long before Joan Robinson (1956, pp. 109–10 on the Ruth Cohen phenomenon), Piero had proved to himself that there can (in general) exist no objective way to decide that Technique A, in comparison with Technique B, is more "capital-intensive", "roundabout" or "durable". The critique of Eugen von Böhm-Bawerk by Irving Fisher (1907, pp. 351–55) might earlier have convinced me of this, but I was playing tennis the mornings that Jacob Viner and Joseph Schumpeter lectured on those subjects.' To the trained ear, the 1960 Sraffa book whispers the relevant hints.

'What, only one thing learned from a classic? You must be pretty dumb', readers may aver.

No. There were indeed many theorems and lemmas to be learned from *Production of Commodities by Means of Commodities*, but for the savvy youngsters in the Leontief Circle, the important ones were pretty much old hat before Sraffa. At the Corfu International Economics Association meeting in September 1958, Piero told me: 'Now that I've finished the Ricardo editing, I've taken up my old notes on capital matters. You know, I find nothing has changed. Soon I'll bring out a book on the subject.' A book from Sraffa! I was enchanted, but I said to myself that the post-von Neumann explosion of game and programming theory had evidently not reached the inner walls of Trinity College!

Later, in the spring of 1960, I received the page proofs of the 1960 book from the Cambridge University Press. To their question, 'Shall we bring out a separate American publication?', I replied in enthusiastic affirmation. I recalled G. H. Hardy's romantic 1915 recognition of the genius of Ramanujan from an unsolicited letter from a poor clerk in a poor region of a poor colony. As Hardy (1940) proudly boasted, the dozen-odd infinite-series expansions in that letter he could recognize were riches of genius. Having this in mind, I wondered to myself: 'What if I got this in the mail, not from Cambridge, but from an anonymous graduate student at East Arizona Tech? Would I have the acumen to recognize its quality?'

One tells anecdotes in order to make a scientific point. The Sraffa work is *outside* normal cumulative science in the sense of Thomas Kuhn's 1962 *Structure of Scientific Revolution*. It is a work in mathematical economics by an amateur, an autodidact. It has the properties of

such. The book has more in it than the author knows. It is not the better for its imperfections. (As Hardy came to admit, Ramanujan could have been even more incredibly original if he had been well grounded in rules of proof and in frontier mathematical knowledge.) However, we can be gratified that Sraffa was not inhibited from publishing his innovations by any conscious feeling of ignorance concerning the Frobenius–Minkowski theory of non-negative real matrices, and he did benefit from Cambridge world-class mathematicians.

Let me be concrete. Chapter 1 begins with a subsistence economy where produced inputs suffice barely to produce themselves. In 1960, a Ph.D. candidate at Stanford, Rochester, MIT, Harvard or Berkeley would be obliged to cite John von Neumann (1937, 1945), Wassily Leontief (1941, 1953), of course, and most important of all the Hawkins and Simon (1949) conditions that precisely identify when an input–output system is *net productive* or is on the borderline of subsistence (see Technical Note 1).

Pendantry. Pedantry. Pedantry. No. The inefficient bifurcation of the literature into two streams has not generated Kuhnian breakthroughs of supernormal science. The whole is less than the sum of its dishevelled parts.[1]

After Joan and Piero had shown that feasible per capita stationary consumption can be cut rather than raised by a drop in interest rates, I had to learn for myself that a J. B. Clark system with genuine smooth marginal productivities can be as capable of *per capita consumptions that sometimes rise when the profit rate rises* as discrete-technology von Neumann–Sraffa systems can. Marginalist models can come as close as you like to reswitching, and in any case reswitching is a red herring, being a sufficient but not a necessary condition for the phenomenon that matters. Thus Böhm-Bawerk (1889) *cum* marginal products can encounter (normal!) cases where lowering the interest rate kills off some stationary-state consumption and production! Post-1960 researches, mainstream and heterodox, add to our knowledge of conditions sufficient to banish reversals in the (profit rate, per capita consumption) relation, and of conditions necessary or sufficient to produce reversals.

Warning: this which Sraffa taught me has essentially nought to do with production of commodities by means of themselves as commodities. Nought to do with existence of Sraffa's basics – where coal needs iron,

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[1] I have mentioned in print that neither Wassily Leontief nor Piero Sraffa has seemed ever to cite the other's work in print. That makes things even? No, it is two warts on the face of science.

iron needs coal, and *all* goods need one and both of these two.[2] My Footnote 2 on Böhm-Bawerk's triangular system *sans* basics makes this independence clear. *A fortiori*, all this has nought to do with successful or unsuccessful critiques of marginalism.

The Böhm-Bawerk example, and *every* behaviour of neoclassicism, can essentially be reproduced up to the thousandth decimal place of accuracy by strict examples of discrete technology à la von Neumann–Sraffa and also, as we shall see, vice versa: strict neoclassical systems of infinite alternative techniques can come as close as we like to *any and all* behaviours of Sraffian finite paradigms. This I did not have to learn from 1960 Sraffa, which indeed obscured the matter. Study of Walras's second edition of *Eléments* (1889), in comparison with his 1896 third edition's marginal products, made that obvious to anyone who realized that smooth curves can always be arbitrarily closely approximated by straight-line chords and vice versa (see Technical Note 2).

2 The doomed critique of marginalism: constant returns?

An honest audit of a purported scientific revolution must record, along with its hits, its misses. What did it fall short of perceiving? Which elements of empirical fact and of normative truth about Pareto optimality did it tend to obscure rather than illuminate?

When giving guest lectures to students during the rebellious late 1960s and early 1970s, I learned that what they considered important in Sraffian economics was his promised future critique of marginalism. After a third

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[2] Consider Böhm–Bawerk's Austrian case, where $Q_t = F[L_{t-1}, L_{t-2}, L_{t-3}]$ and where the partial derivatives of marginal products, $\partial F/\partial L_{t-j}$ and $\partial^2 F/\partial L_{t-1}\partial L_{t-j}$ nicely exist to provide us with the kind of neoclassical distribution theory that a Garegnani (1960) or post-1960 Pasinetti would find displeasing. For this paradigm, the stationary-state per capita consumption is the following function of the equilibrium interest rate of $1 + r^*$:

$$c[1 + r^*] = Q^*/\Sigma_1^3 L_j^* = F[L_1^*, L_2^*, L_3^*]/\Sigma_1^3 L_j^*$$

where

$$\frac{\partial F[L_1^*, L_2^*, L_3^*]/\partial L_2}{\partial F[L_1^*, L_2^*, L_3^*]/\partial L_1} = 1 + r^* = \frac{\partial F[L_1^*, L_2^*, L_3^*]/\partial L_3}{\partial F[L_1^*, L_2^*, L_3^*]/\partial L_2}$$

The Jacobian matrix of this system permits $c'[1 + r^*]$ to change from its negative sign near $r^* = 0$ to a positive sign even when F has every neoclassical property of being first-degree homogeneous and strongly quasi-concave! Sraffa and Joan Robinson taught us more than they dreamed of in their philosophy. See Samuelson (1966, 1994) for more on this. Note that when F has only the two arguments $[L_{t-1}, L_{t-2}]$, $c'[1 + r]$ cannot be positive and the simplest mainstream parable remains valid.

of a century of exploration and reflection on that issue, I have considered opinions that ought to be put in the amber of published discussion. They may be the most important part of my present recorded reflections.

Sraffa's book, he tells us (1960, pp. iii, v–vi), is a 'Prelude to a critique of economic theory'. More specifically, a critique of *marginalism* (call it neoclassicism, if you wish) is to be the next step. 'If the (1960) foundations hold, the critique may be attempted later, either by the writer or someone younger and better fitted for the task.' Extrapolating Piero's speed of composition, we cannot be surprised that he never provided such a critique. I once nursed the hope that among his papers at Trinity, or in Italy, treasures would turn up. That happy eventuality I must now doubt on the basis of all we know about the scholar, but personality traits aside, the Bayesian probabilities of cogent Sraffian fragments on marginalism seem low based on the disappointing *quality* of the few remarks the 1960 author does provide us. The reference to Philip Wicksteed (1914, pp. 18–20; 1933, pp. 790–96) seems a confused citing of a confused and confusing text. Wicksteed for once makes mountains out of trivial hills, and he does not succeed in climbing up and down those mole hillocks.[3]

Sraffa is correct that, in steady states of equilibrium where only one set of input proportions are maintained, any marginal products that exist cannot be *identified*. That tells nothing about when they do and do not exist, and therefore *that* cannot be an analysis *cogently* 'designed to serve as the basis for a critique of that theory' ('the marginal theory of value and distribution', 1960, p. vi). Fortunately, the 1960 book is better than its 78 pages of Parts I and II alone (with their postponing consideration of alternative feasible techniques).

In cautioning (p. v) against readers 'mistaking spurious "margins" for the genuine article', the author seems to overlook that much of his first 78 pages themselves do involve shifts in the 'scale of an industry' – as, for example, in working with specified *standard* market baskets of productions, or in supposing that demand and taste shifts do not alter real prices in a no-joint-product world, and as, for example, in Chapter I's crucial sole footnote.

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[3] When corn is produced by a first-degree-homogeneous function of homogeneous labour and homogeneous land, there are no terminological perplexities. When heterogeneous lands and homogeneous labour are alternative ways to produce homogeneous corn, there are no terminological or logical perplexities – as the 1960 Chapter IX on Land could have clarified if only its few pages had used the space devoted to the topic of the standard system in favour of the programming inequalities–equalities of competitive arbitrage. See, for example, a modern treatment of the Ricardian economy in Samuelson (1959, Appendix, particularly pp. 28–35).

This brings me to the state of ambiguity, scandalous after a third of a century, on the question of whether input–output analysis can be content with a position of agnosticism on the question of an axiom of 'constant returns to scale'. As I hope to demonstrate mathematically, the author wants to play in a poker game where he has not put up the ante. No one need play in a specified game, but if you do play, you must not tolerate self-contradictory rules. A single contradiction in a logical system of axioms makes nothing provable in it (because *any*thing and its negation are implied theorems in it).

My purpose is not to conduct a one-sided debate with a dead scholar. My plaintiff brief, which must stand on its merits not on anyone's ideology, is against a generation of Sraffian writers who are very much alive and have not done their duty in proving that they are entitled to have their cake and eat it too. Constancy of returns to scale (or non-constancy) is crucial for its own sake. It is not crucial at all for a cogent rejection of neoclassical marginalism in favour of some claimed alternative classical paradigm (of distribution, pricing and dynamic growth). Thus, if increasing returns to scale obtains in the real world, so as to entail Chamberlin's (1933) imperfect competition, post-Kaldorian and 1867–1894 Marxian paradigms are as much impacted as are Clark–Solow models.

We can begin with page 1 of the book, then follow up with Chapter II and with Chapter IX on land, and end with the final seven pages that constitute the novelty in the work's contribution.

Wheat and iron outputs (p. 3) are each producible out of themselves as inputs, à la von Neumann (1937, 1945). By definition of this as being a Sraffian subsistence economy, these commodities can just barely reproduce themselves in the stationary state. From out of the blue, Sraffa gives the reader a single-instant picture, describable in the following modern production–function language:

$$\text{Wheat output at } t+1 = f^1 \text{ [wheat input at } t, \text{ iron input at } t]$$

$$\text{Iron output}^{t+1} = f^2 \text{ [wheat-for-iron}^t, \text{ iron-for-iron}^t] \quad (1)$$

Here $f^1[\ ]$ and $f^2[\ ]$ are Sraffa's production functions. His snapshot reveals the following arithmetical numbers:

$$Q_1^{t+1} = f^1[Q_{11}^t, Q_{21}^t] = f^1[280 \text{ qr. wheat, 12 t. iron}]$$

$$= 400 \text{ qr. wheat}$$

$$Q_2^{t+1} = f^2[Q_{12}^t, Q_{22}^t] = f^2 \text{ [120 qr. wheat, 8 t. iron]}$$

$$= 20 \text{ t. iron} \quad (2)$$

Note that each period's outputs are specified to provide just enough inputs as are needed to reproduce the equilibrium indefinitely:

$$Q_1^{t+1} = Q_{11}^t + Q_{12}^t = 280 \, \text{qr.} + 120 \, \text{qr.} = 400 \, \text{qr. wheat}$$

$$Q_2^{t+1} = Q_{21}^t + Q_{22}^t = 12 \, \text{t.} + 8 \, \text{t.} = 20 \, \text{t. iron} \tag{3}$$

Theorem: *If(!), and only if(!), production obeys the law of constant returns to scale, we can write the $f^j[Q_{1j}, Q_{2j}]$ functions more specifically in the following* anti-neoclassical *fashion:*

$$Q_1^{t+1} = 400 \, \text{Min} \, [Q_{11}^t/280, \ Q_{21}^t/12]$$

$$= \text{Min} \, [Q_{11}^t/(280/400), \ Q_{21}^t/(12/400)]$$

$$\equiv \text{Min} \, [Q_{11}^t/a_{11}, Q_{21}/a_{21}]$$

$$Q_2^{t+1} = 20 \, \text{Min} \, [Q_{12}^t/120, \ Q_{22}^t/8]$$

$$= \text{Min} \, [Q_{12}^t/(120/20), \ Q_{22}^t/(8/20)]$$

$$\equiv \text{Min} \, [Q_{12}^t/a_{12}, Q_{22}^t/a_{22}]$$

$$0 \le \begin{bmatrix} a_{11} & a_{12} \\ a_{21} & a_{22} \end{bmatrix} = \begin{bmatrix} 280/400 & 120/20 \\ 12/400 & 8/20 \end{bmatrix} = \begin{bmatrix} 0.7 & 6 \\ 0.03 & 0.4 \end{bmatrix} \tag{4}$$

By convention, if an input–output technical coefficient a_{ij} is zero, we agree to omit its Q_{ij}^t from the $\text{Min} \, [\ldots, Q_{ij}^t/a_{ij}, \ldots]$ expression. Also, no a_{ij} can be negative under free disposability conditions.

Sraffa never writes down the above production functions but, as will be demonstrated, if they are denied the vast corpus of post-Sraffian literature collapses like a soufflé.

Under the same 'If, and only if' proviso stated above, the actual production functions of the system photographed at one instant could just as well be Cobb–Douglas neoclassical rather than the above Walras *Eléments* (pre-third edition) version written here in Equation (4). Once you tell me they are to be Cobb–Douglas, they definitely are thereby 'identified' as having to be the following neoclassical production functions:

$$Q_1^{t+1} = 400 \, (Q_{11}^t/280)^{0.7}(Q_{21}^t/12)^{0.3},$$

$$Q_2^{t+1} = 20 \, (Q_{12}^t/120)^{0.6}(Q_{22}^t/8)^{0.4} \tag{5}$$

Why (0.7 and 0.3) and (0.6 and 0.4)? There is no black magic involved, merely recognition that the relative shares of wheat in the unit costs of Equation (4)'s two goods can be shown to be 0.7 and 0.6, respectively. Remark: other Cobb–Douglas choices could have produced the snapshot, but obviously Sraffa wants his data to represent the *best* that the system can do and still be barely reproductive, which narrows the choice down to Equation (5).

We are beginning to see that the author was misguided to believe that his expositional departures from the literature [from, I suppose, Vladimir Dmitriev (1898), Ladislaus von Bortkiewicz (1907a), Leontief (1928, 1941, 1953), Tjalling Koopmans *et al.* (1951), Robert Solow (1952), Dorfman, Samuelson and Solow (1958), Michio Morishima (1959), Paul A. Samuelson (1959), . . .] were *well* 'designed to serve as the basis for a critique of that [marginalist version of value and distribution] theory' (p. vi). Only in his last chapter does he begin to analyse how to handle alternative techniques if they exist – as they realistically will.

The arithmetic example on the first page is useful to understand this. The 2-good subsistence system there, Equations (1) here, is declared to define a unique set of relative prices, (P_2/P_1) when $n = 2$ or in general $(P_2/P_1, \ldots, P_n/P_1)$, 'which if adopted by the market restores the original distribution' (p. 1), ' . . . which ensures replacement all round' (p. 2), . . . 'which if adopted restores the original position . . . ' (p. 2). What is this language about the market choosing to adopt this or that definition of price? The market has no mind of its own. Only under strict specifications will Darwinian competition *enforce* certain price–cost inequalities–equalities – as Sraffa would learn if he tried to exercise his imagined freedom to assume *increasing* returns to scale of (say) second-degree-homogeneity type. Thus, let a Sraffian try rewriting Equation (4) as

$$Q_j^{t+1} = \left\{ \text{Min} \, [Q_{1j}^t/a_{1j}, \ldots, Q_{nj}^t/a_{nj}] \right\}^2 \bar{Q}_j, \qquad j = 1, \ldots, n \tag{6}$$

or try rewriting Equation (6) with the exponent 2 changed to 9/10, as with *diminishing* returns to scale, or let each *j*th commodity have a different exponent: some above 1, some below 1, some at 1. A true agnostic will let the exponent differ according to scale for each commodity, but the

author's coyness about commitment makes him no source to go to for factual knowledge about any of this.[4]

Does it matter? Of course it does as soon as the author hazards assertions about how the prices of standard or of other market-baskets of goods will vary with the profit rate.

If all this sounds complicated, it is. That is why I devoted most of an MIT semester in the early 1960s to exploring whether useful sense can come from *explicitly denying* constant returns to scale. When I had exhausted all efforts, we were left with an empty set of results. To my knowledge no Sraffian hitherto, or since, has had better luck. How many thirds of centuries must go by with the matter being treated as if unresolved?

Now suppress all t superscripts which become, in stationary states, for $i, j = 1, \ldots, n$,

$$Q_j^{t+1} \equiv Q_j^t \equiv Q_j, \qquad Q_{ij}^t \equiv Q_{ij}, \qquad a_{ij} \equiv Q_{ij}/Q_j \tag{7}$$

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[4] Paolo Varri (1987, p. 380), in a *Palgrave* piece on Fixed Capital, illustrates the uneasiness and mysticism about Sraffian prices as a new kind of prices, saying:

> The meaning of these prices has nothing to do with marginal or neoclassical theory. They represent a more fundamental [sic] concept: the exchange rates which ensure the reproduction of the economic system.

This seems like science fiction. It is the production equalities of $280 + 120 = 400$ and $12 + 8 = 20$ that 'ensure' the reproduction of the stationary state – provided the 400 and 20 harvests are properly allocated between industries. The book's author dictates that, and without indicating what algorithm of tâtonnement is to bring it about (i.e. to convert transitory Equation (8) here to Equation (1) of Sraffa). If entry is free, knowledge is ubiquitous, and inputs are dispersely owned – and if technology is minutely divisible among sub-firms of any size, so that people will stay being 'price takers' in self-sustainable auction markets – then market-clearing competitive markets can be the mechanisms for providing society's appropriate stage directions of behaviour. However, if returns are increasing so that collusion of owners is entailed, price takers become price namers and Sraffa's asserted terms of trade, '10 qr. of wheat for 1 t. of iron' (p. 1) is not at all realized. The defining matrix relation of $P = Pa$, when $[a_{ij}] = [Q_{ij}/\sum_i Q_{ij}] > 0$ and when $\det[I - a] = 0$, can be asserted by Humpty Dumptyism, but we are interested in such Ps only to the extent that they bear a relation to some real economic drama? This, we see again and again, comes when and only when the axiom of constant returns to scale obtains. Incidentally, the 'negative prices' that raise controversies in Sraffians' dialogues on joint production arise as artifacts only when Sraffa's special equalities are respected instead of the proper duality equalities–inequalities of market-clearing behaviour. If axioms of *free disposability* and *divisibility* of goods obtain, then all competitive prices that arise will be non-negative. The defects in Sraffa's Part II on joint products are touched upon by Samuelson (1990) and will not be further treated here. They are easily handled by von Neumann inequality dualities and ought to become standard in the post-Sraffian literature.

This suggests that our relative prices be time-invariant too. Sraffa is shy, or coy, about saying that his prices are to be competitive market prices, never greater than the respective goods' minimized unit costs. (In Robinson's East Anglia, for a time, *simultaneous equations* were considered viciously circular if P_j's unit cost depended upon P_i (and possibly P_j) that was considered somehow unkosher.) Thus, Marx (III, 1894) preferred the term 'prices of production' to '(minimized) unit costs of production', and Sraffa eschews going beyond speaking of his basics' prices as those that enable advances to buy inputs while being able to earn the system's (specified) rate of profit and still have receipts sufficient to compensate for the advances *ad perpetuum*.

There is in any case no way of avoiding simultaneous equations, which Sraffa recognizes. As we shall see, his prices are in every case precisely those of perfect-competition's arbitrage: its inequalities, equalities and dualities. All this applies equally to his defined basics and non-basics, and my conscience as a teacher bothers me that our seminars have to waste so much student time on that not very important distinction. If the sterile quest for the chimera of Ricardo's absolute measure of value had been abandoned stillborn, the Sraffian literature would gain in relevance and appeal. Later I say more on this.

Even in the subsistence economy, incapable of sustaining a positive interest rate, suppose Sraffa's snapshot had been the following instead of my Equation (1) above:

$$350 \text{ wheat} + 15 \text{ iron} \rightarrow 500 \text{ wheat}$$

$$90 \text{ wheat} + \ 6 \text{ iron} \rightarrow \ 15 \text{ iron} \tag{8}$$

It was then *not* in *its* stationary state. Not to worry. The author says (p. 5, n. 1): '...every system of the type under consideration [i.e., just barely productive] is capable of being brought to such a state merely by changing the proportions in which the individual equations enter it.' Oops! Only in constant returns to scale technologies do proportions matter and *alone* matter! Otherwise scale and proportions interact to deny the quoted claim. To see this let the snapshot data of Equation (8) come from Equation (6)'s allegedly admissible Sraffian form. Then Sraffa can never succeed in arriving at his Equation (1) by specifying appropriate relative inputs into Equation (6)'s proposed form for Equation (8). QED.

We can gain further insights from this devastating rebuttal. Suppose that half the inputs in Sraffa's example of Equation (1) become specified *not* to be needed any more. Then each and every output could be twice the sum of itself used as inputs, and thus the system could grow exponentially, doubling every period in accordance with what Sraffa (p. 6)

asserts would be its 100 percent profit rate per period. Who can believe *that* if constant returns to scale is in any way *denied*? Von Neumann knew better.

Suppose the folk on Sraffa's Island X acquire the technical knowledge to be observed on two other subsistence islands:

Island Y

$$140 \text{ wheat} + 6 \text{ iron} \rightarrow 200 \text{ wheat}$$

$$60 \text{ wheat} + 4 \text{ iron} \rightarrow \quad 10 \text{ iron}$$

Island Z

$$200 \text{ wheat} + 16 \text{ iron} \rightarrow 400 \text{ wheat}$$

$$200 \text{ wheat} + \quad 4 \text{ iron} \rightarrow \quad 20 \text{ iron}$$

For anyone not in a Pickwickian mood of nihilism concerning any and all returns to scale, Island Y will be of no *new* interest. It looks to be the same technology as Sraffa's, happening to be sampled at half his scale. Would it be useful for a Sraffian to disagree with this interpretation?

Now turn to Z. It gives us *new* technical options: along with Island X's $(a_{11} \ a_{21})$ of Equation (4), we also have $(a'_{11} \ a'_{21}) = (200/400 \ 16/400)$ and also, along with old $(a_{12} \ a_{22})$, we have $(a'_{12} \ a'_{22}) = (200/20 \ 4/20)$. Peeking into all the chapters of the book, we realize that our own island is no longer a subsistence economy. At the zero interest presupposed in the old subsistence state, Darwinian competition will lead us as if by an invisible hand to produce wheat with Island Z's technique and iron with *our* technique. The same efficiency now goes for autarkic Island Z. When our subsistence state betters itself, it becomes a net production (or surplus) state. It can grow for ever at some positive exponential rate $1 + g^* > 1$ (in this example, $g^* = 2\frac{1}{3}$ per period). We can pay any positive profit rate less than g^* and can still afford to pay needed primary labour and primary land positive wages and rental rates.

Do you believe that? It is nonsense to do so if production functions are homogeneous of degree 2 or of degree 1/2! All of Part III's nice rules about switchpoints are inapplicable nonsense under the same licentiousness. Taking a linear blend at critical *switch* interest rates $\bar{r}$, where two alternative techniques are indifferent, is quite unwarranted (unfeasible!) if returns to scale are variable.

The young Sraffa's original instinct in the 1920s to presume (with Keynes) constant returns to scale was thus not gratuitous. I suspect he abandoned it for two or more reasons. (1) The unimportant conjecture is that Sraffa, at times in 1925 and after, may have used *constant returns* as

a loose equivalent to *constant cost*, and used *decreasing returns* as loosely *increasing cost and supply*. In any case, after the heat of debate, his 1926 brief for constant cost may well have lost self-esteem. (It should have, in my reiterated view.) (2) More importantly, he never worked through the literal consequences for his 1960 book of departures from the returns conditions that market-clearing competition depends upon.

I should add at this point that my (unreported) attempts to make a defence for Sraffa's agnosticism by regarding his prices as planner's prices in an efficient non-market society all failed. The marginalist shadow prices of such a scenario lack the *average*-price properties that are intrinsic to Sraffa's equations in the book, except of course under special explicit constant-returns axioms.[5]

In sum, if a Sraffian denies constant returns to scale, the one-hundred-page 1960 classic evaporates into a few paragraphs of vapid chit-chat.

3   Mathematical heart of Sraffa

Now combine Chapter II with Chapter XI and Part III. Here is how a 3-good, 2-primary-factor Sraffa paradigm will look when (for simplicity) each good can be produced with two alternative techniques and without joint intrinsic products or durable machines. I write $(a'_{Lj}\ a'_{Tj}; a'_{1j}\ a'_{2j}\ a'_{3j})$ and $(a''_{Lj}\ a''_{Tj}; a''_{1j}\ a''_{2j}\ a''_{3j})$, where a_{Lj} stands for direct primary labour requirements and a_{Tj} stands for direct primary land requirements. Labour and land are each homogeneous with stipulated total supplies. Stationary states obtain

$$L'_1 + L''_1 + L'_2 + L''_2 + L'_3 + L''_3 \leq \bar{L} > 0, \qquad L_j \text{ non-negative}$$

$$T'_1 + T''_1 + T'_2 + T''_2 + T'_3 + T''_3 \leq \bar{T} > 0, \qquad T_j \text{ non-negative}$$

$$\text{For } j = 1, 2, 3, \tag{9}$$

$$Q_j = \text{Min } [L'_j/a'_{Lj}, T'_j/a'_{Tj}, Q'_{1j}/a'_{1j}, Q'_{2j}/a'_{2j}, Q'_{3j}/a'_{3j}]$$

$$+ \text{Min } [L''_j/a''_{Lj}, T''_j/a''_{Tj}, Q''_{1j}/a''_{1j}, Q''_{2j}/a''_{2j}, Q''_{3j}/a''_{3j}]$$

$$= \sum_{k=1}^{3} Q'_{jk} + \sum_{k=1}^{3} Q''_{jk} + C_j, \qquad 0 \leq C_j = \text{consumption of } j$$

$$\tag{10}$$

[5] Neo-Ricardian Sraffian models of Smith and Ricardo make no sense if the constant returns to scale that they presumed under competition gets explicitly denied. The classicists did not realize they 'spoke prose', but that prose had to be for the most part first-degree homogeneous.

Write the nominal wage rate as W, the nominal rent per acre (each paid at the *beginning* of the period) as R and the interest rate as r. Then real steady-state prices and distribution involve

$$\text{Min} \left[(Wa'_{Lj} + Ra'_{Tj})(1+r) + \sum_{i=1}^{3} P_i a'_{ij}(1+r) \right.$$

$$\left. (Wa''_{Lj} + Ra''_{Tj})(1+r) + \sum_{i=1}^{3} P_i a''_{ij}(1+r) \right]$$

$$= P_j, \qquad j = 1, 2, 3$$

$$= Wa^*_{Lj}\{R/W, \ 1+r\} + Ra_{Tj}a^*_{Tj}\{R/W, 1+r\}$$

$$[I - a^*\{R/W, 1+r\} (1+r)]^{-1}$$

$$= WA^*_{Lj}\{R/W, 1+r\} + RA^*_{Tj}\{R/W, 1+r\} \tag{11}$$

where the starred a's are competition's chosen least-cost methods, and the starred A's are total (dated!) labour and land requirements. (The choice is from the 2^3 matrices that can be formed by *independently* using for each good either its ( )$'$ or ( )$''$ technique.)

None of this Sraffa–Leontief wisdom applies if returns are essentially non-constant to scale. Unlike Sraffa, von Neumann knew that his growth model had to obey constant returns to scale.

In the smooth neoclassical case, the $(a_{Lj} \dots a_{3j})'$ and $(a_{Lj} \dots a_{3j})''$ vectors are replaced by an infinite variety of alternative $(a_{Lj} \dots a_{3j})$ coefficients connected by each good's relation(s):

$$1 = F^j[a_{Lj}, a_{Tj}, a_{1j}, a_{2j}, a_{3j}], \qquad j = 1, 2, 3 \tag{12}$$

where each $F^j[$] is a concave, smooth, first-degree-homogeneous production function. Always, at each $(R/W, 1+r)$, an optimal $[a^*_{Lj}\{R/W, 1+r\} \ a^*_{Tj}\{R/W, 1+r\} \ a^*_{ij}\{R/W, 1+r\}]$ set of coefficients will be ground out by Darwinian competition.

As we go from ( )$'$ and ( )$''$ choices to a rich variety of techniques, we can approach qualitatively and quantitatively step-function approximations to smooth curves of market-clearing supply and demand (again, see Technical Note 2).

Always, in these single-product Clarkian or Sraffian technologies, a well-behaved *factor–price frontier* obtains for each good:

$$1 + r = \Phi^j(R/P_j/W/P_j), \qquad j = 1, 2, 3 \tag{13}$$

where $-\Phi^j($) is a quasi-concave function that is monotone-increasing. Reswitching or the mentioned permitted reversals in the $(1 + r$, consump-

tion menu) relationships do not affect the good behaviour of Equation (13)'s factor–price frontiers, whether technologies are discrete and finite à la von Neumann–Sraffa or uncountably infinite as with Clark–Solow–Meade.

I have written out explicitly some things Sraffa did not write out in his book. This way we can see precisely what Ricardo's (1) labour-cum-land, (2) time-phased technologies and (3) subsistence-wage paradigm look like through correct 1960 Sraffian spectacles. Call it a neo-neo-Ricardian theory, but recognize that it is quite different from what those who call themselves neo-Ricardians usually talk about when they compare modern and old-time paradigms.

Equations (9), (10) and (13), which eschew smooth Clarkian production functions, have exactly the essential properties of a Haberler–Heckscher–Ohlin–Fisher *post*-1870 paradigm.

1. Far from giving comfort to a *labour theory of value* as an approximation to reality, the model teaches us that Ricardo's complications to the labour theory of value from problems of time can be much more than the Ricardo–Stigler seven percent (see Stigler, 1958). With outputs as inputs, the aberration can easily be 70 or 99 percent.

2. Ricardo could not avoid perceiving the 'time' complication, but neither he nor his editor took proper note of the *irreducible negation of the labour-only dogma that is introduced by land.* When goods differ in their land/labour intensities (for positive-rent and endogenous zero-rent lands!), changes in tastes for corn and cloth completely destroy the hope of relating relative prices to an invariant ratio of respective embodied-dated-labour contents of the goods. Where the external margin for land falls, and how big or small Ricardo's marginal-labour cost will be, these become *endogenous* not exogenous variables – thereby emasculating all meaningful content of a labour theory of value formulation.

The Sraffian model of my Equations (9), (10) and (13) clinches the point.

3. When the real world offers *alternative* techniques,

$$(a'_{jL}, a'_{jT}, \ldots, a'_{ij}, \ldots), \qquad (a''_{jL}, a''_{jT}, \ldots, a''_{ij}, \ldots),$$
$$(a'''_{jL}, a'''_{jT}, \ldots, a'''_{ij}, \ldots), \ldots,$$

then what are smooth demand and supply curves in smooth neoclassical technologies become step-function loci in Sraffa land. In a Gerald Shove (1930) jigsaw puzzle world, where catalogues offer a variety of alternative items and where suppliers are prepared to insert inbetween variants whenever demand warrants, the lengths of the steps and of their risers

shrink in importance and the von Neumann *inequality bounds* become tighter and tighter around the system's equilibrium variables.

Query. If Pero could be brought to life, or if followers would volunteer to field questions on his behalf, what would be the answer to the following questions?

Are there not observable 'margins' (observable equalities or bounds) here? Are such margins '*spurious* margins' or the 'genuine article'?

My answer to these questions is manifest. Under the conditions specified (and with no pretence toward *aggregation* of *scalar* capital), Wicksteed and I would understand this model to have the general qualitative properties of Walras (1896), multi-commodity J. B. Clark (1899), Wicksteed (1894) and Arrow-Debreu (1954). Wouldn't it be nice if Sraffa had left us in an old trunk an outline of precisely these truths? (Of course I wryly jest.)

Figure 1 illustrates neoclassical versions of neo-neo-Ricardianism, and various Sraffian approximations to them. In Figure 1(a), AA′ is the neoclassical production-possibility frontier in the short run when supplies of labour, land and capital are fixed. Figure 1(b) shows the three factor prices (W/P_{corn}, R/P_{corn}, $1 + r$) depicted by their respective distances from the sides of the equilateral triangle and standardized so that their sum is unity. (The top point betokens high profits; the right-hand point means a high corn wage; the left-hand point means high rent.) The locus aa′ traces out induced changes in distribution as consumer tastes change from much cloth at a to much corn at a′.

OUTPUT TRADEOFFS   DISTRIBUTION OF INCOME

(a)   (b)

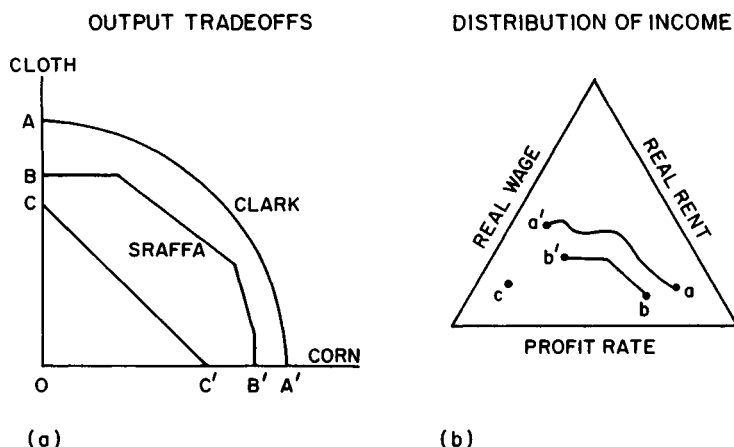Figure 1

In Figure 1(a), BB' is the Sraffian counterpart to AA'; in Figure 1(b), bb' is the Sraffian counterpart to aa'. (Explanation: corn happens here to be relatively land-intensive and with a relatively high wage/profit ratio; cloth is the reverse.) The reader can construct a pair of new diagrams to handle the longer run where (say) population size adjusts to a subsistence corn real wage and accumulation acts to preserve a fixed $1 + \bar{r}$. (Remarks: in the 1960 Parts I and II limiting case of a *single technique*, factor returns are indeterminate when their totals are in fortuitous balance; for factor supplies generically in *any* proportions, one of Part I's factor share will be *zero or all* under ruthless short-run competition. CC in Figure 1(a) is included to portray the Santa Claus case where all goods happen to require all factors in the same proportion. Only the face of the labour theory of value is then saved by the implied invariance in the P_{cloth}/P_{corn} ratio since, as shown in point c in Figure 1(b), virtually 90 percent of the national income can go to land rent rather than to wages! CC' can be either Sraffian or Clarkian.)

4   The futility of Sraffa's standard commodity

My 1990 revisionist paper on Sraffa devoted paragraphs 10–11 to demonstrating the irrelevance and lack of usefulness of his standard commodity. No need to repeat here the argument that it cannot help defend Ricardo's attempted labour theory of value or Marx's formulation of the transformation problem. Here I ought to move on to show why Sraffa's standard does not cogently interpret and effectively help out any Ricardian's (misguided) hankering for an absolute or invariable measure of 'value'. In the 1993 Luigi Pasinetti *Festschrift*, Heinz Kurz and Neri Salvadori have provided a truly valuable survey of Ricardo's wanderings and Sraffa's proposed innovation. Analysts today and antiquarians in the next century will benefit from their efforts. They confirm my view that Ricardo's itch for absolutes was psychosomatic, and that the Sraffian construct does not succeed in scratching *it*.

Begin in 1810, when Ricardo was a rich broker beginning to study economics and when the Napoleonic Wars' expansion of the currency was having the usual inflationary effects on prices (including the prices in paper currency of precious metals such as gold and silver). Practical people sensibly tried to estimate how much prices rose for particular goods and for collections of goods. (Half a century before Jevons, primitive index numbers of prices were glimpsed.) Instead of welcoming this attempt to separate 'real' changes from 'non-real', what Keynes called the

subtlest mind that ever came to economics said in effect at the time of the *Bullion* controversy (I paraphrase Kurz and Salvadori, 1993, p. 96):

No. Rather than measure average price changes, one will better separate the real and the unreal by measuring how price(s) change relative to some [single?] reference commodity whose purchasing power is constant or changes little in the short run. Experience has indeed taught '... that the *value* of gold or silver... for short spaces of time their value is tolerably fixed' [High Price of Bullion, *Works*, III, p. 64n., Ricardo's emphasis]. Therefore, compare individual or means of price changes relative to an ounce of such gold stuff [my wording].

Ricardo's goal is the *intertemporal and interspatial comparison* of price vectors, which tries to separate out *real* and *un*real changes. In *balanced* inflations, for example, the vector (P_j/P_{gold}) (or P_j in ounces of gold) might be virtually constant. By contrast $(P_j/P_{\text{strawberries}})$ will be contaminated by seasonal shifts in tastes and weather. Since Ricardo was building up toward an exaggerated confidence in the labour theory of value, one wonders why his 1810 proposal is to be preferred to concentration on the (P_j/wage) vector itself – or, we might add, the $(P_j/[\frac{1}{2}W + \frac{1}{2}\text{rent}])$ vector?

Seventy-five years ago the American philosopher John Dewey was asked what he thought of IQ measurements. Flippantly he replied: 'It's like trying to decide which of two people is heavier by looking in a pasture of heterogeneous rocks for the items you think most nearly match the individuals. And then *guessing* the weight of those rocks!'

Anyone who swallows a commodity theory of money must have peculiar ignorance about the technology of gold mining to expect particularly low standard deviations and zero mean-trend values in short-run (P_{gold}/W) time series of costs (quantity theorists do less badly), but at least Ricardo in 1810 is operating in the real world of economic history and policy debate. By 1817–21 Ricardo (1951–73) has turned theological and terminological. Now a good's 'value' is ever its labour content or purchasing power over labour. The vector $(P_j/W, P_{\text{gold}}/W)$, or for that matter $(P_j/W, P_j/\text{rent})$, could be better examined item by item, or by market basket, to see how real inventions, real changes in consumers' tastes, real changes in population and required subsistence-wage rates, and real changes in interest rates will affect ratios of elements in such vectors. My Sraffian-like equations presented here are useful to do precisely that and, except for the complication that iron may need coal and coal need iron, Ricardo displayed full powers to handle such equations. The effects of a wartime issue of currency could be contrasted with the comparative statics of these equations.

Why the itch for an absolute or invariable measure of VALUE? Kurz and Salvadori mention the 'time-honoured problem of distinguishing between "value" and "riches"' of Sir William Petty (1690), Adam Smith (1776) and other pre-1821 writers. *That* covers a can of disparate worms. Thus, Smith worried that our welfare would be much more hurt if all the water or air were taken from us than if all the diamonds or silks were, while at the same time each unit and all the air and water do command much less in the marketplace than do diamonds and silks. After 1870, the distinction between marginal and total utility properly explicates the puzzle. Despite the puffery for David Ricardo by Alfred Marshall (1890; 1961, p. 814), David cuts no heroic figure in *this* resolution. What counts here is that *theological and terminological insistence on absolute and invariable measures impede rather than induce clear thinking of these 'real' matters.* (In chasing down citations to Ricardo's *Principles*, I was struck anew with how muddled are some of Ricardo's wordings and joustings with J. B. Say. Editor Sraffa chastely desists from all normative comments.)

On reflection, Ricardo came to realize that exogenous *and endogenous* changes in any economic system must necessarily and always be capable of changing any and every commodity's $(P_j/W, P_j/R, P_j/P_i, P_j/P_{\text{gold}})$ ratios. Instead of this causing him to drop the search for the Dewey-rock unicorn, he narrows his focus to *one* kind of endogenous change: a drop in the interest rate (somehow occasioned) and a rise in the return of the primary factor(s) in terms of labour alone. (One would have thought it better for him to have contemplated *all* changes in the vector of real $(W/R, r, L/T$, tastes) and worked out their effects on $(P_j/P_i, P_j/W, P_j/R)$. The hole in the doughnut of Ricardo's labour theory of value haunts his guilty conscience.)

Now Ricardo looks for a rock, for a good, whose P_j/W is raised by a rise in the $1 + r$ interest rate that is intermediately normal between that of 100-year trees and one-minute shrimp gathered on the seashore. Why that 'mean' is golden or useful as a comparison rock for measuring absolute or invariable 'value' is simply and gratuitously taken for granted.

One who devotes decades to editing Ricardo is prone to take his every preoccupation seriously. Sraffa comes to notice that a set of basics, in a no-joint-product, labour-the-only-primary-factor, SINGLE TECHNIQUE scenario possesses a unique vectoral market-basket of goods which has *its* real wage (paid at end of the period) drop *linearly* as the profit rate rises from *zero* to its technological *maximum*. (Frobenius theorem: every non-negative $[a_{ij}]$ matrix that is *indecomposable* has a right-hand characteristic vector $[Q_i]$ that is positive and unique

but for scale, so that $a(1 + r_{\max})\bar{Q} = \bar{Q}$. *Ergo,* $W(1 + r)/\Sigma_1^n P_j \bar{Q}_j = \alpha[1 - (r/r_{\max})].)^6$

One notices that whenever the basic goods differ in their direct and indirect labour intensities, some of them have real wage rates (have loci of $W(1 + r)/P_j$) that are pushed downward by a specified $(1 + r)$ rise in degree that locally *exceeds* the fall of Sraffa's STANDARD vector real wage; and necessarily some other basic must have its $W(1 + r)/P_k$ fall curvilinearly *slower* than the STANDARD's.

This Sraffian offering to Ricardo: what does it accomplish? How does it compare with, say, a market basket of goods constructed along Etienne Laspeyres, Hermann Paasche, or Fisher ideal index lines? How inferior is looking at it to studying the observable change in $(1 + r)$ [wages' fractional share] induced by all degrees of permissible $(1 + r)$ rise?

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[6] Here is one way, an alternative to the 1960 way, to bring out the economic meaning of the standard vector. An indecomposable, net-productive, single technique can grow at a maximal rate, $1 + r^*$, if all is ploughed back as inputs, and the *positive* vector of productions (*and* of net ploughbacks) are in the proportions of the right-hand characteristic vector $\bar{Q}$. This is a special case of von Neumann's balanced-growth vector when several techniques are feasible.

For this standard vector, a non-spurious marginal productivity interpretation of $1 + r^*$ holds. The vector of inputs $\bar{Q}$ at t will *produce* at $t + 1$ (incrementally, totally and on average) exactly $(1 + r^*)$ times itself, r^* being the scalar intensity of the vectoral augmentation. Here is the story, followed by the scalar (non-vectoral) 'neoclassical' story:

$$[(1 + \epsilon)\bar{Q}^t - \bar{Q}^t]/\epsilon = r^*\bar{Q} \text{ for } 0 < \epsilon \text{ and as } \epsilon \to 0 \tag{6.1}$$

$$K^{t+1} = (1 + r^*)K^t \text{ for my bank account} \tag{6.2}$$

where

$$\partial K^{t+1}/\partial K^t = 1 + r^* \tag{6.3}$$

For a Clark–Ramsey–Solow neoclassical story, let

$$K^{t+1} + C^{t+1} = \alpha(L^t K^t)^{1/2} \tag{6.4a}$$

$$L^{t+1} = (1 + g^*)L^t = 2L^t \tag{6.4b}$$

$$C^t \equiv 0 \tag{6.4c}$$

If and only if $\alpha = 2$ will r^t be constant through time at $r^* = 1$, and $1 + r^*$ will then satisfy the scalar marginal-productivity determination

$$\partial K^{t+1}/\partial K^t = \tfrac{1}{2}\alpha(L^t/K^t)^{1/2}$$
$$= \tfrac{1}{2}\alpha(1)^{1/2} = 4/2 = (1 + 1) = 1 + r^* \tag{6.5}$$

Any fixed proportions for the (K_t/L_t) ratio other than unity will fail to achieve (maximal) feasible balanced exponential growth.

When does the Sraffian construct *not* exist? When is it not even an *internal mean* of *all* $[W(1 + r)/P_j]$ items? How does the real-world existence of land and other non-producible natural resources affect Sraffa's brainchild? How is the concept impacted by real-world jointness of production?

All of these questions have been discussed somewhere in the literature, some of them by me and generations of MIT students. Kurz and Salvadori, as befits a sympathetic account, provide a useful survey of most of these issues. Here are some abbreviated comments.

1. In real life, when Leontief's students study census data on two-digit and three-digit classification of industries, they can 'identify' indecomposable $[a_{ij}]$ matrices *only after aggregating* sectors. Such aggregation can introduce *spurious* indecomposability when no one of the 50,000 commodities can be found with the property of being needed by *every* industry.

In other words, outside of the mathematical economics seminar room where we use indecomposable matrices as simplifying expositional devices for stating Frobenius–Perron matrix theorem, BASICS probably do not exist. (I do not insist on this, but it is noteworthy that *no* system of basics could ever have got started *after* the Big Bang. Realistically, innovators would have to have fabricated by *decomposable* labour-intensive activity the first inventories of basics that could thereafter be competitively viable to reproduce themselves.) I believe in a plethora of independent *sub*-systems that are indecomposable. This denies BASICS.

If exogenously supplied labour is a needed primary factor along with the $(\bar{Q}_{ij})$ and if workers are the only units that redundantly 'consume', then consuming their positive income share will slow down the growth process. If and only if they oddly choose to consume in $[C_j]$ proportions proportional to the technical $\bar{Q}$ vector will there be self-sustaining *exponential* (balanced) growth at $(1 + \sigma r^*)$, where $\sigma r^* < r^*$ is the ruling interest rate and σ is non-consuming rentiers' fractional share of national income.

Duality theory enables us to define $[a_{ij}]$'s existent left-hand eigenvector, $\bar{P}a = \bar{P}/(1 + r^*) > 0$.

Clearly, $\bar{P}a\bar{Q} = \overline{PQ}/(1 + r^*)$,   gross aggregate cost

$$\overline{PC} = \bar{P}[I - a]\bar{Q} = r^*\bar{P}Q, \qquad \text{national income}$$

$$= \text{profit share} + 0 \text{ wage share}, \qquad \text{when } W/\bar{P}_j \equiv 0 \qquad (6.6)$$

Note that all of this has taken no notice of competitive prices. All of it is subject to the same limitations arising from (i) non-indecomposability, (ii) land as a primary factor limiting labour's productivity, (iii) alternative techniques somewhere viable – as belaboured above.

Marxians handicapped themselves when concentrating on zero or near-zero r evaluations. Sraffians will handicap themselves when concentrating on zero or near-zero wage configurations, which is part of what concentrating on standard commodities involves.

2. Related to the above point, but distinct from it, is the observation that a set of basics which exists could well be of minor fractional importance in the national income. Basics sound basic; non-basics sound like frills and luxuries. There is no warrant for this. Once we go beyond believing that water, earth and fire constitute the raw ingredients of everything, we contemplate cases like the following extreme: sugar needs a pinch of itself along with primary labour and land as inputs. Every other good needs a pinch of sugar among its inputs. The set of basics is then not empty: it consists of the one good sugar and, for dramatic exposition, suppose that expenditure on sugar never reaches one-thousandth of the national income.

$W(1 + r)/P_{sugar}$ does fall *linearly* as the profit rate goes from zero to its maximum of $1 + r^* = 1/a_{sugar,sugar}$. So? Little comfort for Ricardo's gratuitous itch here.

Therefore, let us add salt to the basics. Sugar and all goods now also need a pinch of salt as input. Now sugar and salt are basics, and let their total in the national income never exceed say one-seven-hundredth. Now a Sraffa basket of, say, 1 sugar and 0.01 salt defines a real wage that falls linearly – while one of the pair $[W(1 + r)/P_{sugar}, W(1 + r)/P_{salt}]$ has a concave profile and the other has a convex profile, thereby bracketing Sraffa's straight line.

Cui bono for Ricardo's purpose or anyone's purpose? It could well be that *every* other good has a $[W(1 + r)/P_j]$ profile that *lies outside* either and both of the basics' profile(s).

At the least, some Laspeyres or Divisia index of goods can provide a better reference mean than the new Sraffa tool.

3. Dramatic cases alert one to the generic possibilities. Suppose all goods, $j = 1, \ldots, n$, are always consumed in such a way that invariant proportions of individual's income and of NI are $(k_1 \ldots k_n)$ constants. Suppose the first s goods are basics. Their $\sum_1^s k_s$ can be a large or small fraction of unity. Moreover, Sraffa's linear $[W(1 + r)/P_{standard}]$ could well have little resemblance to the behaviour of $[W(1 + r)/\sum_1^n P_j k_j]$ or $[W(1 + r)/\sum_1^n P_j C_j]$ that statistician Simon Kuznets would record.

If compelled to address Ricardo's psychosomatic itch, I would seriously propose the plain-person's Kuznets calculation of how $W(1 + r)/\sum_1^n P_j C_j$ deterministically drops as r rises from zero to Sraffa's r_{max}. (C_j is the net consumption of good j in our stationary state.) Even where every commodity is a basic, it will generally *not* be true that raising r to halfway on its admissible range will result in exactly or approximately a 50% drop in measured real post-factum wages. (Why should *that* be true, and why care when it generally is not?) One can still harmlessly babble: interest-rate increases lower real wage reckoned in long-lived trees more than they

lower real wage in haircuts or shrimp-gathering, and sophisticates can still warn that goods A and B cannot always be reliably ranked in terms of 'time intensity'.[7]

Not only does the Sraffa construct deviate from the mean–aggregate ratio, I would not be surprised if Monte Carlo experimentations with randomly sampled a_{ij} and a_{Lj} coefficients revealed a definite bias in the standard vector. To test this, play with my sugar and salt world, where only sugar is the basic and where most other goods are produced primarily by labour and a pinch of salt. When $W(1 + r)/P_{\text{sugar}}$ falls *linearly*, then precisely because P_{pepper} has *in it* the interest-bearing P_{sugar} component, W/P_{pepper} may tend to drop faster at first than W/P_{sugar}. Concretely,

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[7] Kurz and Salvadori (1993, p. 120, n. 11) point out what they identify as an obvious error in Mark Blaug (1987). Then, in a left-handed compliment, they gratuitously absolve Samuelson from having made that error. (When the small-town editor was reproached for reporting 'John Smith was drunk last week', he changed the headline to 'Smith was sober last week'.) I come into their 1993 Footnote 11 for asserting in *Palgrave* (1987, p. 456) that 'Sraffa...thought that [W/P_{standard} linearly declines with r] somehow provided Ricardo with a defence for his labour theory of value.' For this, the authors say:

> [1] There is no evidence whatsoever in support of this interpretation. [2] Sraffa...emphasized that the Standard commodity is 'a purely auxiliary construction'...and [3] cannot alter its [the system's] mathematical properties. (1993, p. 120; my numberings)

Before I agreed to reformulate what I now guess was in Sraffa's mind during 1927–1960, and appraise how close post-1960 writers are to *his* understandings, it must be noted that what I have numbered [2] and [3] is not cogent rebuttal to my alleged error of [1]. If I erred in attributing to Sraffa interest in defending what I regard to be erroneous Ricardo infatuation with the labour theory of value, it was in no degree because I believed Sraffa to make the Blaug error. Why drag that into appraising *my* critique?

I indict Ricardo (and Sraffa) for not explicitly following Smith in formulating a *tripartite* model of relative prices, real prices and distributive shares based on the *three*some of labour, land and time-phased produced inputs. (Ricardo wrongly missed out in understanding the complications engendered by land(s); for all his complaining about Smith, Ricardo did recognize that his own 'values' paradigm entailed time-phasing deviations, but through some 8 years of dithering he persuaded himself that the deviations were quantitatively minor – viz. the Ricardo–Stigler 93% labour theory of value. See Coleman (1990) for argumentation that 93% could well be 3%.) Ricardo's preoccupations with absolute and invariable measures of value are part of the indictment that post-Smithians like me cogently include in our brief. I agree with Kurz–Salvadori that Sraffa's pages on the STANDARD commodity provide no shred of cogent defence for the defendant(s) indicted. (*That* was my *Palgrave* point, and I need not have complicated it by pronouncing on what Sraffa *thought* his standard commodity had to do with this.) I hope they agree with me that some representative Sraffians have taken a less unsympathetic attitude on *this* matter.

when expenditures on the goods consumed are in proportions invariant to $1 + r$, I would want to explore whether a rise of r halfway to r_{max} will cause Kuznets to observe *more* than a 50% drop in empirical wage share; Sraffa's benchmark in such cases would give a biased upward wage share. Eager readers might work out 'random' choices of coefficients and check whether a systematic bias does exist. Even if the characteristic vector is found to err as much in one direction as the other, why should Ricardo tolerate the gratuitous variance from the Kuznets data which comes from Sraffa's proposal?

Indecomposability and basicness is not a metric character of quantitative relevance. It depends qualitatively on a shibboleth: drop that pinch-of-salt requirement and you have not perceptibly changed anything in the real economic world, but you have perpetrated a tempest in Sraffa's teacup, wiping out half of all his basics!

Before leaving this point, I should take up von Neumann's case of cancerous exponential growth *sans* limiting land supply. For it, sugar and any other basic can grow most rapidly in the proportions of Sraffa's STANDARD. (Non-basics grow in entailed proportions, including possibly their coming to have *infinite* or *zero* relative price!) The standard vector to me is more importantly the von Neumann vector than the Sraffa vector. With multiple independent sub-basics, no standard exists!

4. To cut short a possibly boring topic, consider how to illuminate idle questions like the following: How many inflection points can $(1 + r)W/P_n$ have when the number of goods is given as $n = 3, 4, \ldots$? How many double-switching points can the eight-technique model of Equation (10) possibly have? Etc. These are all part of the P_i/P_j dependences upon $1 + r$. The theory of equations, Sturm's tests and more complicated

On what is a different issue, as I write now in 1993, I would not be surprised or distressed if some back of Sraffa's envelope turned up in the future that was found to say:

My studies have convinced me that the single-technique, labour-only model with an indecomposable core, and which defines a unique standard vector, speaks not at all to the empirical and theoretical usefulness of that standard concept or to the merits and demerits of Ricardo's preoccupation with *labour values.*

Piero's was a subtle mind, which had thought long and hard on these (mathematical!) relationships. His pen writes as if a lawyer were at hand to ensure that no vulnerable sentence appears. I honour him for that, and with my own students felt obligated to point out the subtlety of the text that in one place uses indefinite articles such as 'a' and in another uses definite articles such as 'the', or 'the unique'. What all of Sraffa's readers can agree on is that in the 1960 classic there are no passages like the above back-of-the-envelope fragment or its negation. (So to speak, nowhere does he say, 'I have stopped beating my horse'.)

extensions to ratios of polynomials would be what we must study if these questions were not too frivolous for us to try to answer. If Sraffa's construction were a useful auxiliary for that purpose, it might deserve a modest paragraph in the comprehensive treatises, but is it? Toward what is it an 'auxiliary'?

5. Up until now I have played along with the supposition of but one single $[a_{Lj} \, a_{1j} \ldots]$ technique. As in Part III (1960), let there now be more than one competitively viable technique. Ricardo has now lost the linear reference proffered to him. (Who steals my purse steals trash.) Now, for $0 \le r \le r_{\text{switch}}$, one STANDARD market basket serves; for some other r, it is irrelevant. The King is dead, long live the King, a drama replayable a few or a hundred times as selfish competitors are induced by changes in interest rates to switch their orders from machine-tool catalogues.

It is fortunate that there was no previous usefulness in the standard concept, since that would be lost in any scenario which was at all realistic.

6. Staying with no jointly produced goods, how does the realistic intrusion of Ricardian land affect the Sraffa offering? On the extreme supposition that one technique $(a_{Lj} \, a_{Tj} \ldots a_{ij} \ldots)$ obtains always, and that from somewhere the wage/rent ratio is held invariant while $1 + r$ rises from unity to its maximum, the device works as well (or as badly) as in the labour-only case, but when W/R varies generically *and systemically with* $1 + r$, all is lost.

Ricardo and I have to realize that optimal proportions of land to labour are affected by changes in the interest rate. When vectors of capitals $(Q_{1j} \ldots Q_{nj})$ differ at different $1 + r$ levels, depending on whether one of them is 'more complementary' to land than labour – as is expressible in non-classical Sraffian discrete technologies – there are no *linear* paths in the $(W/P_j, R/P_j, 1 + r)$ loci described in Equation (13) here.

Distribution is complicated in Ricardo's world of labour, land and time-phasing. Had Sraffa developed his critique of marginalism further, he might have come to see how preliminary his Prelude still was.

7. To conserve space, I conclude with a few words on joint products and Sraffa's standard concept applied to them. Preoccupation with it entails preoccupation with the unrewarding definitional complexities of *indecomposability* for such systems. These conquered, we need to flesh out the treatment of inequalities and dualities that Sraffa's Part II never properly addressed.

Let all this be properly done. We are then left with the anticlimax that, for admissible non-negative *rectangular* matrixes $[b_{ij}]$ and $[a_{ij}]$, and admissible von Neumann $[b_{ij} - a_{ij}]$ matrixes, there may exist only in the

complex number system $\alpha + \beta\sqrt{-1}$, characteristic vectors. No one seriously wants to make STANDARD market baskets of say two Basics, with weights of $(0.1 + 0.9\sqrt{-1})$ and $(0.9 + 0.1\sqrt{-1})$. As Carlo Manara (1980, pp. 9–11) has shown, there may exist *no* real characteristic vectors to serve as a standard commodity for admissible *single*-technique joint-product systems.

A catastrophe? No, no catastrophe. There was little of value (to me, to Ricardo, to Sraffa) to be lost and no tragedy in the Manara finding that some *b–a* matrixes lack *real*-number characteristic Sraffian vectors.

My 1990 paper, preliminary to this one, makes it unnecessary to elaborate here on the fact that, even when there exist as many usable activities as there are goods, so the locally relevant sub-system is 'square', it will still be generically true – almost generically so – that competition chooses *endogenously* to go from one square principal-minor to *another* square principal-minor as the result of changes in tastes alone. Constant costs and invariant price ratios (which are not even mandatory when production is not joint but primary factors are more than one) will obtain only in severely limited cases of joint production and when labour is the only primary factor.

5   How limitations of land and capitals get underplayed

Steady states of equilibrium are subsets of the dynamic paths that economic systems can and do follow. These steady states are, in the nomenclature of politics, minority states rare in comparison with the totality of states. The exceptions to this truth occur in the special circumstances of heavily dampened systems that *rapidly converge* to their asymptotes, and which are only rarely perturbed by further exogenous shocks. Keynes recognized this when he said, 'In the long run we are all dead.' He did not mean by this, be cavalier in taking account of the future in comparison with the present. Instead he was reminding us that each future grows out of present presents.

The banalities of the previous paragraph must be reasserted to make the point that the post-1959 Sraffian literature lamentably has shifted undue attention to long-run equilibrium relations. When a Dobb thinks about China or Russia, he ought (like Kuznets) to concentrate on the primitive vectors of capital goods that these societies possess. They should analyse what sacrifices of current consumption may be required if capital vectors are to be built up. They cannot expect different goods to have common own-rates of interest along the transient paths of compe-

titive arbitrage. Piero Sraffa (1932), when criticizing Friedrich Hayek's 1931 *Prices and Production*, insisted on all this in an innovative way. Joan Robinson, to her dying day, expressed scepticisms concerning the usefulness in the real world of exponential paths of equilibrium. However, when you examine the 1960 Sraffa book, you are hard put to find a single passage grappling with dynamic trajectories of induced $P_i(t)/P_j(t)$ changes. If, as I did cursorily for the present effort, you sample a score of post-Sraffian writings in *Palgrave* or elsewhere, you will verify that the 1960 preoccupation prevails.

Why does that matter? It matters because the scarcity of capitals is hidden from view through steady-state spectacles. When Nicholas Georgescu-Roegen (1951) and Samuelson (1951) prattle about non-substitution theorems in Leontief systems, we do not dramatize for readers how a shift of tastes from ballet to bourbon will (at each somehow prescribed interest rate) require a vast reduction of some elements of society's capital VECTOR and a vast increase in some other elements – with no Clarkian neutrality of *net* effect being conceptually definable. Students from a Marxian tradition of *Mehrwert* are not bothered by this: they have been taught that constant capital or 'dead labour' is sterile anyway in comparison with vital direct (or 'live') labour. Any planned utopia that fails to emancipate itself from these notions fatally handicaps its own efficiency and progress.[8]

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[8] Samuelson (1975) has demonstrated the 'intertemporal Pareto-optimality' of competitive arbitrage pricings, *statically* and *dynamically*. Also, Samuelson (1994), in a discussion of new elegant German reproductions of Böhm-Bawerk's 1889 *Positive Theory of Capital* and Irving Fisher's 1907 *Rate of Interest*, calls attention to the Bernard Shaw, V. I. Lenin and Joan Robinson view that once capitals have been accumulated, their returns are rents like Henry George land rents and are therefore available for confiscation by an egalitarian society. I am not a besotted admirer of Friedrich Hayek's *laissez-faire* views, but I do salute his deep 1945 refutation of this naive viewpoint as applied to real life, where knowledge is seriously incomplete in the marketplace.

An important Sraffian 'hit' is that, as Ian Steedman's *Marx After Sraffa* (1977) points out, his 1960 classic is the Trojan Horse in the Marxian seminars on the so-called 'transformation problem'. See Marx (1895), Dmitriev (1898), von Bortkiewicz (1907a), Seton (1957) and Samuelson (1971). Sraffa, the friend of the Italian Marxian communist Gramsci, quietly debunks Marx's paradigm of *Mehrwert*, in which only direct-wage outlays earn an exploitative mark-up. For Sraffa's cost-of-production relationships, constant capital is not dead labour product that needs receive a positive interest yield as surplus. 1960 Sraffa jettisons the *labour theory of value* and replaces it by the 'dated-labour theory': if his chapter on land had been properly made explicit, Sraffa's would have been a land–labour–interest (or time-phased) theory of value, with what Marshall would call 'normal prices' rather than with classical natural price constancies.

6   Conclusion

I have concentrated here more on Sraffa's misses than on his hits. Good wine needs no bush. Like Wicksell on Cassel, I want to nominate for the record some nagging doubts. Peer groups can in the end elect or reject nominated viewpoints, and although I love Wicksell and have some contempt for Cassel's scholarly manners, I judge some of Wicksell's 1919 criticisms to have been wrong. Examples: Cassel is not in error to believe that *numerical* utility is not needed for (or identifiable from) non-stochastic demand data; again, Cassel's early 1918 version of the Harrod–Domar multiplier–accelerator exponential process is valuable despite Wicksell's exaggeration of the importance for early twentieth century Sweden of diminishing returns due to land scarcity. (My own insistence on 'land' in Equations (9)–(13) is motivated by more than land's deserved importance in GNP. Ricardo without land is Hamlet without the Prince. Besides, lands stand for and dramatize the *realistic* lack of homogeneity of the important *primary* factors in the real world: women vs. men; high IQ DNA vs. low; prime vineyard lands vs. scrub pastures. Smith's one-third for labour, one-third for rent, one-third for interest and profit seems better factually than zero for natural resources, 75 percent for wages (heterogeneous workers' rents) and 25 percent for profits.)

Wicksell's misses do not impair the worth of his hits. I hope the same can be said of my effort, whose fruits need to be tested and weighed. Actually, my half a dozen articles purporting to question some Sraffian doctrines have not, to my eye, made palpable dents in the beliefs of contemporary Sraffians. By contrast, and this is only proper and to be expected, my few stumbles in this rough terrain have not gone unnoticed.

As I read the 1960–1993 literature, I sense that mathematical Marxianism of the Paul Sweezy (1942) type has paradoxically been undermined by Sraffa's prices-of-production alternative paradigm to equalized *Mehrwert*. I have in mind such Trojan horses (not pejorative appellations) as Ian Steedman, *Marx After Sraffa* (1977) and John Roemer (1977).

Not less paradoxical is my finding that Editor Sraffa's compilation of David Ricardo's *Works* has resulted in modern microscopes being put on them to reveal a rich pasture of warts rather than beauty marks. When I began to study economics some six decades ago, none of us read Ricardo but we took for granted that there were subtle treasures therein. Our teachers had lost interest and involvement, but their teachers, our grandparents, had argued endlessly about whether Ricardo did

or did not believe in a labour theory of value. (When I put that question to Piero Sraffa in 1948 on the Cambridge Backs, he shrugged his shoulders and replied Delphically: 'He did and he didn't.' I understood and I didn't.)[9]

Of the many post-1960 doubts aired here, a brief summing up would run as follows.

1. Without constant returns to scale, the Leontief–Sraffa matrix apparatus is virtually without economic content and interest. If the axiom is violated at the industry level, price-and-unit-cost correspondences must be replaced by Chamberlin–Cournot monopolistic–competition alternatives. *External*-economy increasing returns won't refute my point.

2. The existence or non-existence of basics is of limited empirical and theoretical importance even in the absence of joint products and non-labour primary factors. When basics do exist and constitute a small fraction of the GNP, constructions based on them are of fractional interest. Whatever their weight in the total, as soon as more than one viable technique exists, there is a plethora of standards. In the most favourable case for Sraffa, the 'auxiliary' knowledge about $(\partial/\partial r)[P_1/W \ldots P_n/W]$ contributed by this 'auxiliary' concept of Sraffa is, to my mind, virtually zero. Ricardo's pathetic hankering for an absolute or invariable measure of value (or price or ...) remains as pathetic after 1960 as before, and it was a pathetic fault in Piero Sraffa as editor not to point this out cogently.

3. No single homogeneous primary factor of production obtains in real life. When we add land(s) (or multiple grades of labour) to a Sraffa–Leontief system, price ratios and the profit rate (W/R, P_j/P_1, P_j/W, $1 + r$) are competitive *endogenous* unknowns subject to supply and demand in

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[9] It is part of the intellectual history of our times that Piero Sraffa helped propel Ludwig Wittgenstein from his *Tractatus* phase to his ultimate phase by introducing into their railway station discussion on the language game, 'Then what do you make of this [Sicilian hand gesture]?'. The late Alexander Gerschenkron, Harvard's erudite economic historian, mentioned to me that there is a similar colloquy in Thomas Mann's 1924 *Magic Mountain*. 'Could Sraffa have been remembering, consciously or unconsciously, that passage?' Gerschenkron asked. 'Why not write to him at Trinity,' I suggested. Gerschenkron hesitated to do that but, on my urging, wrote to Maurice Dobb to put to Sraffa the question if he thought that acceptable. Gerschenkron reported: 'Dobb replied that Piero confirmed he had never read *Magic Mountain*'. Another time, I was puzzled about whether Sraffa meant by his words 'constant returns' (1) constant returns to scale, or (2) 'constant costs' as the special case of (1) where factor proportions happen to be uniform? I was hesitant to press him in correspondence, so I enlisted Joan Robinson to ask him. She reported that Piero asked what else could he have meant than 'constant returns *to scale*?'.

multiple markets – markets which clear in every run in time with equilibria that depend on tastes, endowments and relevant factor-supply relationships. Qualitatively, the resulting inequalities of comparative statics – $(\Delta P_i)(\Delta Q_i) \geq 0$ and all that – are precisely the same whether the discrete-technology system has many or few alternative techniques and/or has much or little variability in proportions. *All* the qualitative intertemporal properties of a Sraffa–von Neumann discrete technology can be mimicked in a smoothly differentiable technology, and vice versa. (In both paradigms, a bunching of techniques *near* each other will create the same sensitivity of factor shares in GNP to minute changes in input endowments, etc.) See Samuelson (1949, 1987, 1991a, 1991b).

4. I strongly believe, on the evidence, that Smith, Ricardo and J. S. Mill used essentially *the same logical paradigm* as did Walras and Arrow and Debreu. (Edward Chamberlin is another matter, as is Ralph Gomory's (1958) integer programming.) Until missing papers surface in the Sraffa files with *new* devastating critiques of 'marginalism', or until living Sraffians produce such new critiques not yet to be found in the literature, there will seem no need to qualify the first two sentences of this paragraph.

Years ago in a Presidential AEA address I scolded the public for taking John Kenneth Galbraith too seriously, and scolded us professionals for not taking him seriously enough. Maybe I was at least half right.

Today, if I need to scold Sraffians for taking *Production of Commodities by Means of Commodities* too seriously, I must scold mainstream economists for not taking it seriously enough.

It is a beautiful work for all its idiosyncrasies. Piero Sraffa was a marvellous personality and personage. Joan Robinson (1933), Roy Harrod, Michal Kalecki (1971) and Nicholas Kaldor (1937, 1960a, 1960b) – individually and collectively – added to our understanding of mainstream economics and its limitations and to our understanding of the world. My Nobel medallion would have a greater lustre to my eye if *their* just rewards had been justly recognized.

Technical notes

1. Hawkins–Simon and Sraffa's subsistence technologies. The traditional subsistence economy of Malthus and Darwin, applicable to men, rabbits and sagebrush, contemplates stationary states with a population density relative to fixed land at a critical ratio where output per capita is just adequate to keep populations from either declining or increasing. Sraffa's Chapter I has its own, related but distinguishable, definition.

For Sraffa a technology is a (barely) subsistence one, where by definition the stationary levels of total outputs, $(\bar{Q}_i)$, are just adequate to provide the (Q_{ij}) inputs of themselves needed for their total production and reproduction. He begins with *all* produced inputs strictly positive – as in (p. 3)'s

200 qr. wheat and 12 t. iron *produces* 400 qr. wheat

120 qr. wheat and 8 t. iron *produces* 20 t. iron

or

$$Q_{11} \text{ and } Q_{21} \rightarrow Q_{11} + Q_{12} = \bar{Q}_1, \ Q_{21} \text{ and } Q_{22} \rightarrow Q_{21} + Q_{22} = \bar{Q}_2 \tag{1.1}$$

Notationally, I write total outputs as $(Q_1 \ Q_2 \ldots)$, inputs of goods $(1, \ldots, n)$ needed to produce Q_j of good j as $(Q_{1j} \ldots Q_{nj})$, and the technical a_{ij} coefficients giving the needed inputs normalized to produce *one* of good j as $(a_{11} = Q_{11}/Q_1,$ $a_{12} = Q_{12}/Q_2, \ldots, a_{ij} = Q_{ij}/Q_j, \ldots)$. Sraffa's adequate but self-handicapping notation translates as $(Q_1 \ Q_2 \ldots) = (A \ B \ldots)$; $(Q_{11}, Q_{21}, Q_{12}, Q_{22}, \ldots) = (A_a, B_a, A_b, B_b \ldots)$. Also $(a_{11}, a_{21}, a_{12}, a_{22}, \ldots) = (A_a/A, B_a/A, A_b/B, B_b/B, \ldots)$, etc.

Equation (1.1) is *one snapshot* of the technology. That same technology, Sraffa presumes (p. 5, n.1), would be capable of showing a *second snapshot* such as

100 wheat and 6 iron *produces* 200 wheat

120 wheat and 8 iron *produces* 20 iron $\qquad\qquad$ (1.2)

In his words (p. 5, n.1): '... every system of the type under consideration [such as Equation (1.2)] is capable of being brought to such a [self-replacing] state [proportional to Equation (1.1)] merely by changing the proportions in which the individual equations enter it.' Thus, by his third page, the author has answered in the affirmative his own question: Am I *necessarily* assuming *constant returns to scale*? Yes, his own logic tells us, for the quoted sentence is the necessary and sufficient condition for one to convert any single snapshot, of the type

$$\bar{Q}_1/400 = \text{Min} \ [Q_{11}/200, Q_{21}/12], \qquad \bar{Q}_2/20 = \text{Min} \ [Q_{12}/120, Q_{22}] \tag{1.3}$$

regarded as valid for the one special case of $(Q_{11}, Q_{21}, Q_{12}, Q_{22};$ $Q_1, Q_2) = (200, 12, 20, 120; 400, 20)$, to be *necessarily valid* for *any* positive Q_{ij}. If this first-degree-homogeneous formulation of Equation (1.3) were not valid – and, say a two-degree-homogeneous, or a 1/3-degree-homogeneous, or a varying-degree-homogeneous function were assumed valid – then it would be inadmissible for Sraffa to be able to convert Equation (1.2) into Equation (1.1) or its scale equivalent. QED.

Chapter I's definitional condition for Sraffian subsistence, written as

$$Q_{i1} + \ldots + Q_{in} + 0 = \bar{Q}_i > 0 \qquad i = 1, \ldots, n \tag{1.4}$$

is equivalent in matrix terms to saying that $(\bar{Q}_i)$ is *a positive characteristic right-hand column vector* of the $a = [a_{ij}] = [Q_{ij}/\sum_k Q_{ik}]$ matrix

$$\begin{bmatrix} a_{11} & \ldots & a_{1n} \\ \vdots & & \vdots \\ a_{n1} & \ldots & a_{nn} \end{bmatrix} \begin{bmatrix} \bar{Q}_1 \\ \vdots \\ \bar{Q}_n \end{bmatrix} = (1) \begin{bmatrix} \bar{Q}_1 \\ \vdots \\ \bar{Q}_n \end{bmatrix}, \qquad aQ = Q \tag{1.5a}$$

$$[I - a][\bar{Q}] = \begin{bmatrix} 1 - a_{11} & \ldots & -a_{1n} \\ \vdots & & \vdots \\ -a_{n1} & \ldots & 1 - a_{nn} \end{bmatrix} \begin{bmatrix} \bar{Q}_1 \\ \vdots \\ \bar{Q}_n \end{bmatrix} = 0 \tag{1.5b}$$

If Equation (1.5b) is to have a non-zero vector solution for $(\bar{Q}_i)$, we know $I - a$ must be singular with a zero determinant:

$$\det[I - a] = \begin{vmatrix} 1 - a_{11} & \ldots & -a_{1n} \\ \vdots & & \vdots \\ -a_{n1} & \ldots & 1 - a_{nn} \end{vmatrix} = 0 \tag{1.5c}$$

Actually, unknown to Sraffa publishing in 1960, David Hawkins and Herbert Simon (1949) gave a classic proof for a technology to be *net-productive* or to be *barely* so. See Robert Dorfman, Paul Samuelson and Robert Solow (1958, pp. 253–64) for a discussion of many equivalent Hawkins–Simon conditions: necessary conditions, sufficient conditions, necessary-and-sufficient conditions.

For brevity, I note that if $[Q_{ij}]$ and $[a_{ij}]$ are all positive, then Equation (1.5c) is assuredly both necessary *and* sufficient. In Sraffa's terms, *all* goods are then basics (*each* needed directly or indirectly to produce *every* good; in this overstrong case of positivity, *directly*).

Sraffa (pp. 4–5) notes that some Q_{ij}'s can be zero rather than positive. Page 8 says, correctly says, that a subsistence a cannot be of the form that includes a non-basic along with basics, but little definite is given about what a_{ij}'s can be zero for Sraffa. It is understandable that Sraffa in his sixth decade would not know of Hawkins–Simon (1949) and Dorfman–Samuelson–Solow (1958), but in view of Kaldor, David Champernowne (1945) and the Cambridge discussions of John von Neumann (1937, 1945), it was self-indulgent of him not to relate his subsistence technology to the von Neumann closed growth model capable only of zero growth and a zero interest rate. On the issue of a's being indecomposable, so that all the goods are to be basics in the Sraffa zoo, von Neumann's over-strong condition for irreducibility boils down in Chapter I's no-jointness-of-production case to the following anticlimax: *Any* diagonal Q_{ii} or a_{ii} may be zero, but *all* off-diagonal a_{ij}'s or Q_{ij}'s must be positive. Even for Sraffa, this would be gratuitously over-strong. (Von Neumann was not nodding but he was in an over-hurry.)

Actually, any of the following sign patterns for a are legitimate subsistence economies, satisfying the spirit of Equations (1.4) and (1.5), but only a subset of

them satisfy Sraffa's gratuitously special requirement (1960, p. 8) that only systems possessing basics are to be discussed in his book.

$$[1], \quad \begin{bmatrix} + & + \\ + & + \end{bmatrix}, \quad \begin{bmatrix} + & + & + \\ + & + & + \\ + & + & + \end{bmatrix} \tag{1.6a}$$

$$\begin{bmatrix} 0 & \frac{1}{2} \\ 2 & 0 \end{bmatrix}, \quad a_{12}a_{21} = 1; \quad \begin{bmatrix} + & + \\ + & 0 \end{bmatrix}, \quad a_{12}a_{21} = 1 - a_{11} \tag{1.6b}$$

$$\begin{bmatrix} 0 & \frac{1}{2} & 0 \\ 2 & 0 & 0 \\ 0 & 0 & 1 \end{bmatrix}; \quad \begin{bmatrix} + & + & 0 \\ + & + & 0 \\ 0 & 0 & 1 \end{bmatrix}, \quad \begin{vmatrix} 1 - a_{11} & -a_{12} \\ -a_{21} & 1 - a_{22} \end{vmatrix} = 0 \le 1 - a_{ii} \tag{1.6c}$$

$$\begin{bmatrix} 0.9 & 0 \\ 0 & 1 \end{bmatrix} \tag{1.6d}$$

Why does it matter that the real world can often have *no* set of basics? Why not humour Piero Sraffa's idiosyncratic refusal to contemplate technologies, like those in Equation (1.6c)? It matters because the general is always to be preferred to the (gratuitously) special. It matters because so much of the 1960 book, which is in any case less than 100 pages, is literally wasted on verbiage concerning basics. (For example, the palaver about standard commodities.) Remove the pages dealing with an irrelevancy and you have a very small book indeed, one with gaping vacuums that (to mix a metaphor) now stand out. The five-page Chapter XI on land is a glaring example. Any work calling for a repudiation of mainstream paradigms in favour of a return to pre-1870 classicism should have a long and deep chapter on land. Instead we have a trivial preoccupation with how to fit land into the mould of joint production, and how to define for such models the definition of basics. What we lack are recognitions of how a 2-primary-factor-cum-time-phasing paradigm vitiates Ricardian labour-theory-of-value approximations, and how joint production paradigms necessitate going beyond Sraffian equalities (with their bizarre negative prices in a universe of free disposability!) in favour of Dantzig–von Neumann inequalities–equalities.

I return now to point out that in subsistence economies of the Equation (1.6c) type, which possess no non-empty set of basics, Sraffa's Chapter I artifact of 'exchange-values' (that 'restore[s] the original distribution of the products' and makes the process repeatable) simply does not *uniquely* exist. Where 1 of wheat by itself produces 1 of wheat, and 1 of iron by itself produces 1 of iron, Sraffa's p_2/p_1

is *any* positive number, and the same holds for vectors of prices in multi-good subsistence systems that split into independent parts.[10] That such indeterminacy does not matter reveals that unique determinancy (when it obtains) does not really matter after all!

The Hawkins–Simon analysis can assure Sraffa of the following:

If and only if all goods in the subsistence economy are basics, so that

$$I + a + a^2 + \ldots + a^{n-1} > 0 \tag{1.7a}$$

and a is assuredly indecomposable, a will possess both a right-hand characteristic column vector $\bar{Q}$ and a left-hand characteristic row vector $\bar{P}$, which are both positive and unique save for arbitrary scale

$$0 < \bar{Q} = a\bar{Q}, \qquad 0 < \bar{P} = \bar{P}a \tag{1.7b}$$

P/P_1 and Q/Q_1 unique vectors; also, every $(n-1)^2$ minor of $I - a$ is positive and $I - a$ is of rank $n - 1$.

However, when a subsistence a has no basics, as in Equation (1.6c), the correct necessary and sufficient Hawkins–Simon conditions for a to be a barely subsistence technology is that

$$\det[I - a] = 0 \tag{1.8a}$$

Every principal minor of $[I - a]$ to be non-negative \hfill (1.8b)

My example in Equation (1.6d) illustrates the inadequacy of Sraffa's 'equalities approach' in comparison with the more general von Neumann (1945) equalities–inequalities approach. Suppose a technology can produce autonomous exponential growth of wheat but only steady-state reproduction of iron. Then modern students of non-linear programming, as in Tjalling Koopmans (1951), will consider this to be a subsistence economy. (A chain is only as strong as its weakest link. The most slowly growing autonomous sub-economy determines the maximum growth rate of the system, which is zero in this case. Von Neumann's minimum interest rate is here zero, and the steady-state price(s) of the redundantly growing sub-sector(s) is zero in virtue of those goods' redundancy.) Hawkins–Simon's Equation (1.8) still applies.

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[10] The lack of uniqueness of Sraffa's (P_j/P_1) characteristic vector in Chapter I when (1.6c) occurs and no basics exist is a bit reminiscent of the Kurz and Salvadori (1991 [1992a], 1995, pp. 155–6) *curiosum*, in which alternative choices of techniques exist in a subsistence economy of the type that can lead to some indeterminacy of (P_j/P_1) prices. It is to be noted, though, that (1.6c) here involves solely one (a_{ij}) matrix. My same indeterminacy would also hold for the (1.6c) pattern applied to a net productive case like $a_{11} = 1/2 = a_{22} = 1/2$, $a_{12} = 0 = a_{21}$. At $1 + r^* = 2$, no primary factor could be paid a positive return, and the P_2/P_1 ratio would be indeterminate. For $1 < 1 + r < 2$, the wage and rent rates could both be positive and P_2/P_1 could be determined from (wage/rent, r) parameters alone. As $r \to 1$, P_2/P_1 would approach a determinate limit, but that is only one point on the continuum of P_2/P_1's that are admissible at $r^* = 1$ (when needed, labour and land stay conveniently available at zero factor prices).

I conclude this discussion of Chapter I subsistence with the generalized Hawkins–Simon analysis of technologies that are net productive, or in Sraffian language are 'surplus' technologies. In Chapter II (1960, p. 7), Sraffa increases his subsistence example's wheat harvest by seven-sixteenths, or 43.75 percent. Now that some a_{ij} is reduced, what was barely self-reproducing becomes capable of positive exponential growth. He does tell us that his new steady-state prices are

$$P_2/P_1 = 15\text{qr. wheat per ton of iron} \tag{1.9a}$$

$$\text{Profit or interest rate} = 25\% \text{ per period} \tag{1.9b}$$

Page 6 defines his post-subsistence prices as the following positive left-hand characteristic row vector of the new a, and $1 +$ the profit rate as a's real-and-positive *eigenvalue*:

$$\bar{P}a = (1 + \bar{r})^{-1}\bar{P} > 0, \qquad \bar{r} > 0 \tag{1.10a}$$

When Sraffa's a is stipulated to be indecomposable, $\bar{P}/\bar{P}_1$ is unique and positive and so is $\bar{r}$. In the usual Marxian *Weltanschauung*, Q_{ij} capital (so-called 'constant capital') is sterile. A self-critical Marxian will notice that the $\bar{r}$ eigenvector of Sraffa (1960, p. 6), of Equation (1.10a), and of von Neumann generally is a pure-productivity rate of profit – as Nicholas Kaldor (1937) discussed in his polemic with Frank Knight.

Although Sraffa does not mention it, the system could grow at any uniform exponential rate of less than exactly 25 percent per period (and at the same time choose to be consuming one or another exponentially growing vector of basics). Sraffa, a critic of Walrasian competitive pricing, paradoxically neglects the new $\bar{Q}$ vector of uniform maximal growth to concentrate on the new $\bar{P}$ vector of better-than-subsistence a.

It follows that the positive vector mode of maximal growth, $\bar{Q}$, is the uniquely positive column eigenvector, and the growth rate equals Sraffa's same $\bar{r}$:

$$a\bar{Q} = (1 + \bar{r})^{-1}\bar{Q} > 0, \qquad \bar{Q}/\bar{Q}_1 \text{ unique} \tag{1.10b}$$

Now, however, the net productive a can have its basics supplemented by so-called luxuries (or, better, non-basics, since the oxygen needed for life itself could be a non-basic that is hardly a mere luxury). Now Sraffa's typical case can be written to involve m basics and $n - m$ non-basics, as in the block matrix.

$$0 \leq a = \begin{bmatrix} a' & a'' \\ 0 & a''' \end{bmatrix} \tag{1.11a}$$

a' m-by-m, $m < n$; a'' m-by-$(n - m)$ and not all 0's; a''' $(n - m)$-by-$(n - m)$

$$I_m + (a') + (a')^2 + \ldots + (a')^{m-1} > 0 \tag{1.11b}$$

Every principal minor of a''' to be *positive* $\tag{1.11c}$

If $[I - a''']$ has a real characteristic $\bar{r}''' <$ than the $\bar{r}'$ of a',

    straightforward complications arise $\tag{1.11d}$

2. The truly classical 'subsistence' state. When Sraffa's original 'subsistence' economy reports that wheat and iron alone produce wheat and iron each, and in amounts of total outputs that respectively just equal total inputs, no explicit mention is at first made of labour as a cooperating input. However, by Chapter II, it is made clear that needed labour is getting its subsistence wage of wheat (and possibly of iron) in the background. Thus, when 280 of wheat is needed to produce 400 of wheat, that 280 might already include (say) 100 of wheat for (say) 100 workers' needed subsistence – along with the residual 180 of wheat needed as seed input. Notationally, call $a_{\text{wheat,wheat}}$ or a_{11} the technical input of wheat needed for one unit of wheat production: in the example, $a_{11} = 180/400 = 0.45$. Add to a_{11} what Francis Seton (1957) aptly calls the 'feeding coefficient' of $100/400 = 0.25$; then that gives Sraffa's reported $a_{11} = f_{11} = 0.45 + 0.25 = 0.70 = 280/400$.

There is no room in that exposition of Sraffa for the positive land rent and (possible) positive interest rate that characterizes the conventional Malthus–Darwin subsistence stationary state. As in many a Marx tableau of reproduction, Sraffa here ignores land as a constraining input – until the brief Chapter XI where land is given a walk-on part in the second act of joint products – instead of being treated as a primary input like labour. Along with the a_{ij} and f_{ij} technical coefficients, in a single-product scenario, one specifies needed-land coefficients $(a_{\text{land},1}, a_{\text{land},2}, \ldots)$.

Taken literally, Sraffa's ecological scenario is a special and odd one. We can envisage three independent planets. On A, the only technique can keep no positive stationary state alive. Any initial endowments of wheat and iron will erode away, because their use as technical and feeding inputs yield less output than themselves. On Planet B, with Goldilocks' just-right, not-too-hot-not-too-cold technology, one reportable scale of stationary state can occur: for any jury who concludes that constant returns to scale is the only interesting case that obtains, Planet B can be magnified a trillion-fold in scale or can be shrunk down a trillion-fold. For Planet C (which is *net* productive à la Hawkins and Simon (1949)), exponential self-growth is suggested to be possible *ad infinitum* and/or positive net consumption can be pulled out forever from the initially endowed system.

Classical economics if anything overstressed constraining land. Where post-Newton man is concerned, the same is true of Darwinian paradigms – as in the *logistic* model of Verhulst (1838), Lotka (1925) and Pearl (1925), where environmental scarcity is what determines the evolutionary equilibrium capacity. It would be fortuitous if land-augmenting technical change permitted realistic ignoring forever of natural-resource constraints. That the Second Law of Thermodynamics grinds exceedingly fine would be apparent were it not for post-Newtonian scientific breakthroughs that play no role in static microeconomic models.

To use later terminology, Sraffa is in a Roy Harrod (1939, 1948) world where limited environmental resources do not constrain. To realize the more common 1750–1870 *Weltanschauung*, all three planets are on the technological menu, and endogenously, the subsistence-state equilibrium is found at population densities

that select from the broad menu a Planet C item. If only the Planet A choice is realistic, we have deserted islands. If only Planet B is realistic, a non-generic razor's edge case of probability zero, except under egalitarian socialism, no positive population is viable. With a continuum of Planet C-feasibilities, involving $[a_{ij} + f_{ij}]$ coefficients which are at least lower than Sraffa's

$$\begin{bmatrix} 280/400 & 12/400 \\ 120/20 & 8/20 \end{bmatrix}$$

coefficients, a non-property-owning working class can reproduce itself inside of C only at one bare-subsistence wage-consumption level and at a scale that will depend on the taste allocations of the property owners. (If they change to consume more iron, whilst workers subsist only on wheat, the equilibrium of the population will become higher than when property rentiers demand much wheat.)

I think it a pity that the 1960 classic did not give the reader a few pages on this core of *classical* economics. To do so would not have weakened any valid future critique of 'marginalism'. Among early writers Cantillon (1755), Quesnay (1758), Thünen (1826–1850) and Marx (*Capital*, Vol. II, 1885) gave some signs of sensing the circular interdependence problem entailed when iron as output needs, directly or indirectly, some of the iron itself, but no one seems to have pointed rigorously to the analytic solution until Dmitriev (1898, 1904). Bortkiewicz (1907b) reported on Dmitriev's brilliant work, but neither Leontief nor Sraffa seem to have known of it until after 1940. (I owe to Heinz Kurz's researches on the Sraffa papers the suggestion that only in the 1940s did Sraffa become aware of the writings of Bortkiewicz, Dmitriev and von Neumann. One who can be nameless here suggested to me that Leontief, as a student of Bortkiewicz, must have known of, and been able to read, the Dmitriev Russian-language breakthrough on this point. To check up on this, I quizzed my old master when he was in relaxed mood and learned that indeed, while he knew Marx, he did not know the Dmitriev item in his days at St. Petersburg, Berlin and Kiel.)

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Comment

Heinz D. Kurz and Neri Salvadori

In his paper, Paul A. Samuelson focuses attention especially on the following problems: (i) can Sraffa (1960) do without constant returns to scale; (ii) what is the use of the Standard system and Standard commodity; (iii) is there a 'classical' alternative to the 'marginalist' or 'neoclassical' approach to the theory of value and distribution? In passing he also comments on a couple of other issues including (iv) the importance or otherwise of the distinction between basic and non-basic commodities, (v) Sraffa's treatment of joint production, and (vi) the question of simultaneous equations. In what follows we shall point out in which respects and why our reading of Sraffa and some other authors differs from that of Samuelson. We try to be as brief as possible. The interested reader may want to consult Kurz and Salvadori (1995) for a more detailed exposition of the arguments sketched.

(i)   Returns to scale

Samuelson introduces the part of his paper entitled 'The Doomed Critique of Marginalism: Constant Returns?' with the assessment that it may be 'the most important part of my present recorded reflections' (p. 116), and concludes it with the statement: 'In sum, if a Sraffian denies constant returns to scale, the one-hundred-page 1960 classic evaporates into a few paragraphs of vapid chit-chat' (p. 123). Samuelson attempts to 'demonstrate mathematically' that Sraffa has to assume constant returns to scale and attacks 'a generation of Sraffian writers who are very much alive and have not done their duty in proving that they are entitled to have their cake and eat it' (p. 117).

As it well known, in his criticism in the 1920s of the Marshallian analysis of variable-cost industries within the framework of partial competitive equilibrium (cf. Sraffa, 1925, 1926), Sraffa argued that the Marshallian analysis has to assume that variable costs are due to economies of scale and are *internal to the industry and external to the firm*. The former condition is a requirement of partial analysis, the latter of the assumption of free competition. In the 1926 paper, Sraffa suggested retaining partial equilibrium analysis. This was possible at the cost of abandoning the concern with the free competition form of markets: in order to be able to preserve the partial framework, the analysis had to be limited to the study of economies internal to the firm. Yet this is not the only possible way of coping with the critique of the Marshallian analysis. There is an alternative route that could be followed, which consists of retaining the concern with the free competition form of markets but abandoning partial analysis. This involves assuming that variable returns are a consequence of economies *external to the firm*. This is the route followed by Sraffa (1960).[1]

Assume single production and let **q** be the *n*-vector of quantities of commodities produced in the different industries. For a given **q** the processes available to firms are given irrespective of the question of returns with respect to industries, returns within firms being constant.[2] The choice of technique can be carried out so that a unique price vector **p** and a unique wage rate w (in terms of some numéraire) can be determined for each given level of the rate of profit r. Since this operation can be

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[1] According to another interpretation, Sraffa's approach can also be considered to cover the case of contestable markets. It is argued that in this case the issue of whether variable returns are connected with internal or external economies does not arise.

[2] Therefore, we do not agree with Samuelson, who contends that 'If the Axiom [of constant returns to scale] is violated at the industry level, price-and-unit-cost correspondences must be replaced by Chamberlin–Cournot monopolistic-competitition alternatives' (p. 139).

performed for any (feasible) vector $\mathbf{q}$, the analysis will determine a function[3]

$$(\mathbf{p}, w) = F(\mathbf{q}, r)$$

where a vector of gross outputs $\mathbf{q}$ is feasible if there are techniques available that allow its production.

Clearly, the above function (or correspondence) depends on the kind of returns prevailing in the economy. Thus, with constant returns to scale throughout, the function is a constant function with respect to $\mathbf{q}$. If returns are decreasing because certain qualities of land are in short supply and the technology is such that only extensive rent arises, then $\mathbf{p}$ is an increasing step-function with respect to $\mathbf{q}$, provided that labour is used as the numéraire, that is, $w = 1$. In the general case, the above correspondence has not yet been fully explored, but it is known that if intensive diminishing returns are allowed for, then prices can locally go up or down with respect to changes in $\mathbf{q}$. Hence, we cannot agree with Samuelson's claim that if constant returns to scale are not explicitly assumed in Sraffa's analysis, we are left with an 'empty set of results' (p. 120).

To conclude this section, we wonder what is the factual basis of Samuelson's 'suspicion' that Sraffa may have abandoned the assumption of constant returns to scale because 'he never worked through the literal consequences for his 1960 book of departures from the returns conditions that market-clearing competition depends upon' (p. 123). At any rate, we are not aware of any evidence from Sraffa's own writings that could lend support to this view.

(ii)   Standard system

In the part entitled 'The Futility of Sraffa's Standard Commodity', Samuelson reiterates his 1990 view as to 'the irrelevance and lack of usefulness' of Sraffa's respective concept. To this he adds:

No need to repeat here the argument that it cannot help defend Ricardo's attempted labour theory of value or Marx's formulation of the transformation problem. Here I ought to move on to show why Sraffa's standard does not cogently interpret and effectively help out any Ricardian's (misguided) hankering for an absolute or invariable measure of "value". (p. 127)

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[3] This is so only if all commodities are produced. If only some commodities are produced that relationship between $(\mathbf{p}, w)$ and $(\mathbf{q}, r)$ is a correspondence (cf. Kurz and Salvadori, 1995, Section 3 of Chapter 5).

We do agree with each of these statements. At the same time we insist that none of them contains anything that could be seen as a criticism of Sraffa for the simple reason that Sraffa nowhere used the Standard commodity in order to accomplish what (according to Samuelson and other interpreters) cannot be accomplished. While some interpreters of Sraffa may be criticized for having attributed to the Standard commodity properties which it does not possess and for which it was not designed, this criticism cannot be levelled at Sraffa. Having said this, we do not agree with Samuelson that the Standard commodity is irrelevant and useless. We also do not agree that Ricardo's search for an invariable measure of value is 'theological' (p. 128), 'psychosomatic' (p. 127) and 'pathetic' (p. 139).

As regards Ricardo, the search for an invariable measure of value is but an expression of Ricardo's awareness of the difficulties of the theory of value, a major difficulty being due to compound interest. In Ricardo, the concept of an 'invariable' measure of value was meant to single out the determinants of value, that is, those factors which, if changed, would affect the prices of commodities with the exception of the price of the standard of value. Hence, already at Ricardo's hands the search for an invariable measure of value was at least partly an analytical tool designed to render the theory of value and distribution precise and simple. While Ricardo's search for such a measure indeed turned out to be a search for a will-o'-the-wisp, given the properties he required the measure to possess, this does not mean that his efforts were totally futile. In the course of his investigation, and despite his fruitless wanderings, he was able to illuminate the intricacies involved and render more precise than presumably any author before him the factors affecting relative prices.

As regards the Standard commodity, we have argued elsewhere that 'Sraffa, for perfectly good reasons it seems, saw only a single analytical purpose of the Standard commodity, i.e. to simplify the analysis of the effects of changes in the division of the product between profits and wages on prices' (Kurz and Salvadori, 1993, p. 118). In this view the Standard commodity is an *analytical tool* useful in the study of the dependence of relative prices on income distribution. We added:

It deserves mention that these results [i.e., those obtained by Sraffa in regard to single-product systems] can also be obtained by using the Perron–Frobenius Theorem. In fact, Sraffa's demonstration of the existence and uniqueness of the Standard commodity can be considered a (not fully complete) proof of this theorem. Yet Sraffa does even better, simultaneously providing an economic rationale of the analytical tools he uses. (ibid., p. 111)

Hence, in our view the Standard commodity is relevant and useful, but not indispensable. In one place Samuelson appears to come close to this interpretation: he stresses that 'we can be gratified that Sraffa was not inhibited from publishing his innovations by any conscious feeling of ignorance concerning the Frobenius–Minkowski theory of non-negative real matrices' (p. 114; see also p. 129). It was indeed precisely the elaboration of the ingenious concept of the Standard commodity which enabled Sraffa to accomplish a task which otherwise would have required a knowledge of the relevant parts of linear algebra. Sraffa himself forged the tools of his analysis. In addition, it should be noted that he was very clear about the limited scope of the tool under discussion: 'The Standard system is a purely auxiliary construction. It should therefore be possible to present the essential elements of the mechanism under consideration without having recourse to it' (Sraffa, 1960, p. 31). The mechanism referred to is the adjustment of relative prices consequent upon a change in distribution. This should suffice to answer Samuelson's question: 'Toward what is it an "auxiliary"?' Moreover, it should be clear that in contradistinction to his claim the Standard system does not 'involve shifts in the "scale of an industry"' (p. 116) in any real sense: its construction is a pure thought experiment preserving the technical characteristics of production (per unit of output) of the actual system under consideration.

Samuelson expresses the view that our criticism of his statement 'Sraffa, for reasons not easy to understand, thought that $[w = 1 - (r/R)]$'s truth somehow provided Ricardo with a defence for his labour theory of value' (Kurz and Salvadori, 1993, p. 120) is somewhat mistaken. He clarifies that he 'in no degree' intended to attribute to Sraffa a view similar to the one expressed by Blaug that prices can be made independent of distribution by an appropriate choice of the numéraire. To this he adds: 'I indict Ricardo (and Sraffa) for not explicitly following Smith in formulating a *tripartite* model of relative prices, real prices, and distributive shares based on the *three*some of labour, land, and time-phased produced inputs.' In brackets follows the adjunct: '*That* was my... point, and I need not have complicated it by pronouncing on what Sraffa thought his Standard commodity had to do with it.' He concludes: 'I hope they agree with me that some representative Sraffians have taken a less unsympathetic attitude on *this* matter' (pp. 133–4, fn. 7). In this latter regard we do indeed agree with him. It is not clear to us what Samuelson means when indicting Ricardo and Sraffa, because these authors corrected logical flaws and deficiencies in Smith's analysis of value and distribution.

(iii)   On alternative approaches to the theory of value and distribution

Samuelson confirms that Sraffa's work 'is *outside* normal cumulative science in the sense of Thomas Kuhn's 1962 *Structure of Scientific Revolution*' (p. 113; Samuelson's emphasis). At the same time he deplores what he calls an 'inefficient bifurcation of the literature into two streams;' (p. 114). In his view there is no 'alternative classical paradigm' to 'neoclassical marginalism' (p. 117). Sraffa's analysis is rather envisaged as exhibiting 'the general qualitative properties of Walras (1896), multi-commodity J. B. Clark (1899), Wicksteed (1894) and Arrow–Debreu (1954)' (p. 126). Samuelson stresses: 'I strongly believe, on the evidence, that Smith, Ricardo, and J. S. Mill used essentially the *same logical paradigm* as did Walras and Arrow–Debreu' (p. 140). He attempts to demonstrate the alleged family resemblance of the different theories in terms of production possibility and factor price frontiers.

On this we agree and we do not. We fully share Samuelson's view that '[t]he time-phased input–output system has many of the regularities enjoyed by a maximum system, provided that the competitive solutions are correctly treated. . . . Whether one is neoclassical or not, or Marxian or not, the logic of such systems fits well into the Weltanschauung that permeates this book [i.e., the *Foundations of Economic Analysis*]' (Samuelson, 1983, p. 584). Therefore, we agree if Samuelson wants to say that *any* long-period theory of prices must satisfy Sraffa's equations of production (cf. Kurz and Salvadori, 1995, pp. 22–33). However, we do not agree if he wants to say that the different theories referred to all belong to the demand and supply 'paradigm'.

Scrutiny shows that the contributions to the theory of value and distribution of 'classical' derivation share a common feature, the many differences between different authors notwithstanding: in investigating the relationship between the system of relative prices and income distribution they start from the same set of *data* or rather *independent variables*. These independent variables concern the 'system of production' in use, characterized, as it is, by:

(i)   the set of technical alternatives from which cost-minimizing producers can choose;

(ii)   the size and composition of the social product;

(iii) the ruling wage rate(s) (or, alternatively, the rate of profits);

(iv)   the quantities of different natural resources, in particular land, available.

The treatment of wages as an independent variable and of other distributive variables, especially profits, as dependent residuals exhibits a fundamental *asymmetry* in the classical approach to the theory of value and distribution.

In correspondence with the underlying *long-period* competitive position of the economy, the capital stock is assumed to be fully adjusted to these data, in particular to the levels of output. Hence the 'normal' desired pattern of utilization of plant and equipment would be realized and a uniform rate of return on its supply price obtained. It turns out that these data are sufficient to determine the unknowns or dependent variables, that is, the rate of profits (or, alternatively, the wage rate(s)) and relative prices. No additional data are needed to determine these unknowns. Thus the classical authors separated the determination of profits and prices from that of quantities, taken as *given* in (i) and (ii) above. The latter were considered as determined in another part of the theory, that is, the analysis of accumulation and economic and social development.

In contradistinction, the data or independent variables from which marginalist or 'neoclassical' theory typically begins its reasoning are the following. It takes as given:

(i) the set of technical alternatives from which cost-minimizing producers can choose;
(ii) the initial endowments of the economy including labour, land(s) and capital and the distribution of property rights among individual agents;
(iii) the preferences of consumers.

As regards the specification of 'initial endowments', we have to distinguish between the 'original' factors of production, such as different kinds of labour and different kinds of land, and a factor called 'capital'. While the former are generally given in kind and measured in terms of the respective factor's own natural unit, there are two different treatments of the economy's 'endowment' with 'capital'. First, there is the treatment of 'capital' as a single item; second, there is the treatment of 'capital' as a given set of physical stocks of capital goods. Major representatives of the first alternative include Jevons, Böhm-Bawerk, Marshall and Wicksell, whereas Walras and Arrow–Debreu adopted the second alternative. While the first alternative preserved the classical economists' concern with the *long-period* equilibria of the economic system, characterized by a *uniform* rate of profits and uniform rates of remuneration for all primary factors of production, the second alternative, in order to avoid the difficulties encountered with Walras' capitalization equations, deliber-

ately did away with this concern: starting from a vector of concrete capital goods in given supply implied that only *short-period* equilibria could be studied.

Sraffa's approach shares all the characteristic features of the 'classical' approach. In particular, he does *not* start from given endowments of capital goods. Therefore, all attempts to interpret Sraffa's analysis as a 'special case' of neoclassical analysis appear to be mistaken. (For a more detailed exposition of this argument, see Kurz and Salvadori, 1995, pp. 451–5.) We wonder in particular what is the 'Sraffian counterpart' of the 'neoclassical production-possibility frontier in the *short run* when supplies of labour, land, and *capitals* are fixed' (p. 126; emphases added).

Finally, it ought to be recalled that Sraffa effectively demolished traditional neoclassical analysis, which starts from the assumption of a given 'quantity of capital'. As he pointed out, reswitching and capital reversing 'cannot be reconciled with *any* notion of capital as a measurable quantity independent of distribution and prices' (Sraffa, 1960, p. 38; Sraffa's emphasis). Samuelson himself has repeatedly paid tribute to Sraffa for this; see, in particular, Samuelson's 'A Summing Up' of the 1966 symposium on capital theory organized by the *Quarterly Journal of Economics* (Samuelson, 1966).[4]

(iv)   Basics and non-basics

In Samuelson's view, the distinction between basic and non-basic commodities is 'not very important' (p. 121). At first sight this judgement is difficult to appreciate because basic commodities exhibit various properties which non-basics do not. Yet what Samuelson really maintains is that 'outside of the mathematical economics seminar room where we use indecomposable matrices as simplifying expositional devices for stating Frobenius–Perron matrix theorems, BASICS probably do not exist' (p. 131). A few lines further down he conjectures that 'a set of Basics which exists could well be of minor fractional importance in the National Income. Basics sound basic; Non-Basics sound like frills and luxuries. There is no warrant for this' (p. 132). Hence, in his opinion it is doubtful that there are basics in the real economy, and if there are, that they are important.

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[4] It is unclear to us why Samuelson calls reswitching 'a red herring' on the ground that it is 'a sufficient but not necessary condition for the phenomenon that matters' (p. 114).

Whatever the names given to different kinds of commodities, what matters are the different roles performed by them in production and consumption. Sraffa focuses attention exclusively on production. His distinction between basics and non-basics is *purely* technological, that is, whether or not a commodity enters (as a means of production) directly or indirectly into the production of all commodities. Therefore, the distinction says nothing about the importance of a commodity in consumption. A non-basic may very well be an essential (even indispensable) consumption good. Sraffa makes a single assumption, namely that there is at least one basic (Sraffa, 1960, p. 8), that is, at least one basic in altogether k commodities, however large k is! This assumption does not seem to be excessively strong. It is equivalent to the assumption that no commodity can be produced without a material input, and that the economy cannot be divided in parts which are totally separated from each other (as in Samuelson's examples (1.6c) and (1.6d), p. 143) in the sense that they produce totally different commodities, and each part trades with the others just for the purpose of consumption since there is no other need to trade. On the other hand, such a weak assumption allows for a rich harvest of results. For instance, it implies that there is a maximum rate of profits, that there is a (composite) commodity which, if used as numéraire, implies that the wage rate as a function of the profit rate is a straight line, and a number of other properties. Hence, we cannot agree with Samuelson that 'Existence or non-existence of Basics is of limited empirical and theoretical importance.... When Basics do exist and constitute a small fraction of the GNP, constructions based on them are of fractional interest' (p. 139).[5]

(v)   Joint production

Samuelson argues that

the 'negative prices' that raise controversies in Sraffians' dialogues on joint production arise as artifacts only when Sraffa's special equalities are respected instead of the proper duality equalities–inequalities of market behaviour. If axioms of *free disposability* and *divisibility* of goods obtain, then all competitive prices that arise will be non-negative. (p. 120, fn. 4)

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[5]We obviously agree with him that 'Realistically, innovators would have to have fabricated by *decomposable* labour-intensive activity the first inventories of Basics that could thereafter be competitively viable to reproduce themselves' (p. 131). We are sure that he will also agree with us that no process used for the production of a prototype is a process used in the long run.

In addition, in his 'Technical Note 1' he maintains that 'joint production paradigms necessitate going beyond Sraffian equalities (with their bizarre negative prices in a universe of free disposability!) in favour of Dantzig–von Neumann inequalities–equalities' (p. 143).

While we do agree that Sraffa's approach to joint production is not fully satisfactory, we think that Samuelson's assessment of it is difficult to sustain. Sraffa's assumption that the number of independent processes in the system is equal to the number of commodities produced cannot be sustained in general. His justification of this assumption in terms of the 'requirements for use' (Sraffa, 1960, p. 43, fn. 2) is valid only in some circumstances. This does not mean, however, that his analysis of joint production is without value.

The starting point of Sraffa's respective argument is the observation that while with single production no price can become negative as a result of the variation of the wage rate between zero and its maximum value, given the ('square') system of production, with joint production this is no longer true. He comments on this:

This conclusion is not in itself very startling. All that it implies is that, although in actual fact all prices were positive, a change in the wage might create a situation the logic of which required some of the prices to turn negative: and this being unacceptable, those among the methods of production that gave rise to such a result would be discarded to make room for others which in the new situation were consistent with positive prices. (ibid., p. 59)

Hence, Sraffa is aware of the fact that the positivity of prices cannot be guaranteed if there is no choice of technique. As to the substance of Sraffa's suggested way out of the impasse arising from the negativity of the price of a joint product, it is tantamount to the *ad hoc* assumption that there is always one or several processes of production such that the phenomenon of negative price disappears. Sraffa in fact adopts this assumption rather than von Neumann's assumption of free disposal. Clearly, the former assumption is no more *ad hoc* than the latter, which is equivalent to the assumption that for each process producing a given product there is another process which is exactly identical to the first one except that the product under consideration is *not* produced (cf. Kurz and Salvadori, 1995, p. 228). Therefore, it is not clear what Samuelson means when he speaks of 'bizarre negative prices in a universe of free disposability'. In Sraffa there are *neither* negative prices *nor* is there free disposal.

In the 'real world' disposal is never really free, nor can it always be counted upon that the set of alternatives from which cost-minimizing producers can choose is such that none of the joint products will ever

be overproduced, that is, 'requirements for use' will be exactly matched. Hence, both the von Neumann and the Sraffa approach to joint production involve strong abstractions. An alternative would be to allow for *costly* disposal (see, for example, Kurz and Salvadori, ibid., pp. 202–3). There is nothing wrong or bizarre with a *negative* price, because the price of a product which *must* be disposed of in a costly way must be negative if nobody is interested in taking it for free.

(vi)   On simultaneous equations

Finally, we should like to remark on the simultaneous equations approach in the theory of value and distribution. In one place Samuelson maintains that 'Sraffa is shy, or coy, about saying that his prices are to be competitive-market prices, never greater than the respective goods' minimized unit costs.' In brackets he adds: 'In Robinson's East Anglia for a time *simultaneous equations* were considered viciously circular' (p. 121). To avoid possible misunderstandings the following points should be stressed. (i) Sraffa emphasizes that under competitive conditions the choice of technique 'will be exclusively grounded on cheapness' (Sraffa, 1960, p. 83). (ii) The idea that simultaneous equations are 'viciously circular' was widespread in economics (and still is in some circles): Böhm–Bawerk, for example, chastised simultaneous equations as 'a mortal sin against all scientific logic'. (iii) Sraffa is explicitly opposed to this view: he decides to avoid the use of the term 'costs of production', as well as that of 'capital', precisely because these terms could wrongly give the impression that the problem of simultaneous determination could be circumnavigated. These terms, he points out, 'have come to be inseparably linked with the supposition that they stand for quantities that can be measured independently of, and prior to, the determination of the prices of the products.... Since to achieve freedom from such presuppositions has been one of the aims of this work, avoidance of the terms seemed the only way of not prejudicing the issue' (ibid., p. 9).

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Reactions to Kurz–Salvadori's Comments

Paul A. Samuelson

I am blessed by the thoughtful and deep and candid comments on my *Sraffa's Hits and Misses* by Professors Heinz Kurz and Neri Salvadori. Using their numbering system, (i) to (vi), I try to react constructively and advance the good cause of judging the 1960 Sraffa classic. Readers are in their debt when they force me to explicate more fully my contentions.

Non-constant returns?

(i) The Kurz–Salvadori 'Comments' fails to understand my contentions about non-constant returns to scale and, in my scoring, its final sentence's rejection of my 'untruth' is not cogently demonstrated. So let me help clear up the matter.

In response to a warning from Keynes, Sraffa, in 1960, makes clear: I do not necessarily assume *constant* returns *to scale*. Take him at his word: 'Therefore, we may apply your 1960 paradigm to the following clear departure from constant returns to scale and give it enough rope to hang itself.'

Wheat, q_1, is produced by labour and iron at strong *increasing* returns to scale; iron, q_2, is produced by labour and wheat at strong *decreasing* returns to scale. Concretely,

$q_1(t + 1)$ = wheat output

$\qquad$ = wheat consumption + wheat input

$\qquad$ = $c_1(t + 1) + q_{12}(t + 1)$

$\qquad$ = $8 \, (\text{Min} \, [L_1(t)/2, \; q_{21}(t)/1])^4$ $\hspace{2cm}$ (1a)

$q_2(t + 1)$ = iron consumption + iron input

$\qquad$ = $c_2(t + 1) + q_{21}(t + 1)$

$\qquad$ = $3 \, (\text{Min} \, [L_2(t)/1, \; q_{21}(t)/1]^{1/2}$ $\hspace{2cm}$ (1b)

Wheat's production function (of discrete Sraffian type!) is homogeneous of degree *four* for scale changes – increasing returns with a vengeance. Iron's is homogeneous of degree *one-half*, i.e. viciously decreasing returns to scale.

Specifying a Sraffian numerical example, like that of (1960, Chap. 2), I specify $q_{12} = q_{21} = 1$ and $q_{11} = q_{22} = 0$ in the stationary state. Relations in Equation (1) then presuppose total labour of 3, $L_1 + L_2 = 2 + 1$. The system is 'productive': positive net consumptions of wheat and iron will be $(c_1 \; c_2) = (1 \; 2)$, and gross outputs will be $(q_1 \; q_2) = (8 \; 3) = (7 + 1 \; 2 + 1)$.

Now dare to do something absurd. Calculate for Sraffians the technical coefficients that emerge from naive (input ÷ output) ratios:

$$a_{11} = q_{11}/q_1 = 0, \qquad a_{22} = q_{22}/q_2 = 0$$

$$a_{12} = q_{12}/q_1 = 1/8, \qquad a_{21} = q_{21}/q_2 = 1/3$$

$$b_1 = L_1/q_1 = 2/8 = 1/4, \qquad b_2 = L_2/q_2 = 1/3 \hspace{1cm} (2)$$

What is the Sraffa-defined price ratio, P_2/P_1? If the profit rate is specified to be zero, $\bar{r} = 0$, the computer grinds out

$$P_2/P_1 = \frac{10/23}{7/23} = 10/7 \hspace{3cm} (3)$$

Later, Equation (4a) will verify this. As computer hacks say, GIGO: garbage in, garbage out.

Can we squander a moment to compute Sraffa's standard commodity? Why not? It is a market basket of $\sqrt{8/3}$ units of wheat to each one unit of iron (with whatever meaning that can have in this specified returns scenario).

Playing pretend games, suppose consumers can [sic] buy goods at Sraffian real prices: $(P_1/W \; P_2/W) = (7/23 \; 10/23)$. Let workers first always spend their incomes in proportions 49/69 on wheat and 20/69 on iron – which is compatible with the pretend game that Sraffa's alleged prices in Equation (3) could be actual prices in (A) a perfect planned state, or in (B) a laissez-faire push–shove equilibrium, *cum* or *sans* 'external (algebraic) economies'. Be wary of calling (B) auction-market competition among replicable free entrants and give up Marshall–Pigou or Walras–Arrow–Debreu (*dd ss*) diagrams for it.

Let the reader now contemplate a change of tastes by workers to a regimen where 50% of income always goes to wheat and 50% to iron. The price ratio would have stayed at 10/7 if returns to scale had been constant! Only the $(L_1/L_2 \; q_{12}/q_{21})$ would then adjust. We know this from the 1949 non-substitution theorem of the Leontief literature, which Sraffians inherited in 1960. If this were called to Piero's attention, I do not think he would be surprised, but of course *none* of this applies under present specified returns when the c_2/c_1 ratio changes from the old feasible level to a new level. In neither regimen can more c_2 be got by the sacrifice of c_1 in the ratio indicated by 1960 Sraffian price ratios! We stagger from one irrelevancy to another!

Worse is to come. Specify a change from $(q_{12} \; q_{21}) = (1 \; 1)$ to $(4 \; 1)$, which is perfectly admissible. We get a new set of feasible Sraffian pseudo-numbers. New $(q_1 \; q_2 \; c_1 \; c_2 \; L_1 \; L_2 \; a_{12} \; a_{21} \; b_1 \; b_2 \; P_2/P_1,$ standard commodity weights)! They will be contradictory to the old set in all possible ratios $(L_1/q_{21} \ldots P_2/P_1)$.

Here are the old and new numbers. Readers who cannot deduce them all from Equation (1) and the boldfaced numbers below have not understood the present exposition of Sraffian arithmetic and its pretended extensions. Be reminded that there has been no innovative change in *any* technological opportunity.

	$\mathbf{q}_{12}$	$\mathbf{q}_{21}$	L_1	L_2	q_1	q_2	c_1	c_2	a_{12}	a_{21}	b_1	b_2	P_2/P_1
Old	**1**	**1**	2	2	8	3	7	2	1/3	1/8	2/8	1/3	10/7
New	**4**	**1**	2	4	8	6	4	5	4/6	1/8	2/8	4/16	9/4

As a helpful hint, here it is calculated out:

$$\text{Old } [P_j/W] = [2/8 \quad 1/3] \begin{bmatrix} 1-0 & -\frac{1}{3} \\ -\frac{1}{8} & 1-0 \end{bmatrix}^{-1}$$

$$= [2/8 \quad 1/3] \begin{bmatrix} \frac{24}{23} & \frac{8}{23} \\ \frac{3}{23} & \frac{24}{23} \end{bmatrix}$$

$$= \begin{bmatrix} \frac{6}{23} + \frac{1}{23} & \frac{2}{23} + \frac{8}{23} \end{bmatrix}$$

$$= [7/23 \quad 10/23], \qquad P_2/P_1 = 10/7 \tag{4a}$$

$$\text{New } [P_j/W] = [2/8 \quad 4/6] \begin{bmatrix} 1-0 & -\frac{4}{6} \\ -\frac{1}{8} & 1-0 \end{bmatrix}^{-1}$$

$$= [2/8 \quad 4/6] \begin{bmatrix} \frac{24}{23} & \frac{8}{23} \\ \frac{3}{23} & \frac{24}{23} \end{bmatrix}$$

$$= \begin{bmatrix} \frac{6}{23} + \frac{2}{23} & \frac{2}{23} + \frac{16}{23} \end{bmatrix}$$

$$= [8/23 \quad 18/23], \qquad P_2/P_1 = 18/8 = 9/4 \tag{4b}$$

Suppose in addition we also specify a range of positive profit rates $\bar{r}$. Now the limits on that range alter wildly with the initial specifications of $(q_{12} \ q_{21})$, and now none of the infinity of market-basket weights of the infinity of definable pseudo-standards can support *exact exponential growth*. For these returns there exists an infinity of growth paths that exceed *any* positive exponential rate.

Indeed, shrinking the original $(q_{12} \ q_{21})$ from (1 1) to, say, (1/100 1/100) will give a non-surplus and non-subsistence economy, and why not, when 'scale now matters'. The present system, it should be noted, could never start small and accumulate into a viable system that is 'productive'. Like a small pile of uranium$_{235}$ yearning to go bing–bang, it can never by itself attain the critical mass to go active. (If labour alone can produce any specified q vector – albeit inefficiently – this paragraph loses its force.)

I could go on and on. And on.

The mortal error is to think that any of Sraffa's Part III criteria for going through a switchpoint from one best technique to another (appropriate, say, to a higher $\bar{r}$ rate) still possess applicability and relevance.

We are now in the province of parametric non-convex programming. Answers are hard and complex, but they are definite. Sraffa, at various

intervals over 35 years, tried to navigate in the serene waters of linear technologies. With the help of Besicovitch and with admirable self-persistence, he almost reinvented some well-known wheels. However, in the rough waters of integer programming and non-convex parametric programming there is no evidence that he knew how to modify his 1960 procedures. The deficiencies are not merely mathematical; the basic economics is at fault when the valid preconceptions of one Santa Claus world are thoughtlessly hijacked into another more complex world.

Also, why should one want (gratuitously) to make extrapolations that are erroneous? I suspect ideology played a role. I do not mean Left versus Right ideology. I mean that Sraffa always seemed alienated from the twentieth-century trends of mainstream economics. That could explain his apparent vast ignorance of the *detailed* content of so many 1920–1960 authors. Leaving mathematical esoterica aside, it would seem to be a wilful ignorance. (There is evidence that out of reticence he never fully revealed to his Cambridge mathematician friends exactly what his needs and goals were – a self-imposed inefficiency.) Fair enough. Each to his own tastes and idiosyncrasies, but if you want to enter into the courtroom of a fundamental critique of marginalism and much else, you are ill-advised to tie one hand behind yourself. Trite counsel, but repeatedly in life I have had to remind myself of it.

1926 déjà vu all over again

(i) (continued) My point about constant returns is quite independent of the Kurz–Salvadori resurrection of ancient controversies about Sraffa's 1926 classic article. Part of that article gave a worthwhile reminder that falling marginal costs to a firm must destroy the firm's *competitive* equilibrium. Bring on Chamberlin (1933, 1962) and Robinson (1956) and reread 1838 Cournot! Another part was for at least 15 years widely interpreted to allege that: along with falling *ss* supply curves being ruled out in the absence of externalities, Marshall's *rising ss* curves were suspect in the *partial equilibrium* model (that was so over-touted in 1890–1925), and at best rising *ss* would have to be a rare curiosum. By exhaustion, Marshallian competition must therefore boil down primarily only to horizontal *ss* curves, and so the 1817–1848 classical model of (allegedly) constant natural prices was alleged to be not so bad (even in 1926!) after all.

To believe in such an interpretation is to believe in balderdash. Generically, that is singular coincidences aside, simple competitive models of 1750, 1817, 1920 or 1997 can expect a shift of tastes from wheat to rye to *raise P_r/P_w*. (If the process induces a change in the interest rate, the

reader can make the qualification indicated.) Also, wherever the gratui-
tous specifications apply that Marshall needed to make *his partial* equili-
brium geometry *rigorously* applicable, Sraffa ought to have deduced that:
rising *ss* curves are the generic rule, gently rising or steeply rising. If
partial equilibrium had cogently denied *this*, that would have been a
mortal flaw for partial equilibrium modelling. That it does not deny
this can be rigorously proved by one of an infinity of counter examples.
Example: wheat and rye use transferable homogeneous labour indiffer-
ently between them; in addition wheat needs available Land A, good for
wheat production only, while rye needs available Land B, good for rye
only. To validate the representative-agent scenario, let all have *equal*
ownership of Land A and Land B, and all render equal amounts of the
transferable labour. Finally, let each have the same utility–disutility func-
tion with *independent* marginal utilities of wheat and rye (declining of
course), and let all have marginal disutility of labour that is the same
strict constant. (See Samuelson, 1971, Section 5, for proof of these con-
tentions about *exact partial* equilibrium models.)

The example's result is rising *ss* curves for all goods, intersecting in a
Marshallian cross with the goods' declining *dd* curves. QED. Any shift in
tastes from one good to another raises the relative price of that one good.
QED. What was half the fuss about in 1926? Often when I beat down
resistance to this line of argument, at the end of the day I would be told:
'Well yes. And somewhere in the Italian 1925 version or the England 1926
version there are Sraffian words that do say this.' If so, Amen.

If the above were all wrong it would not matter for my present argu-
ment about non-constant returns to scale. If, for whatever reason of
externalities or internalities, constant scale returns do obtain, then 1960
Sraffa–Leontief arithmetic makes some sense; if not, not.

I should add that when a 1960 book can discuss price equilibrium
without having to mention firms in the industry, that is a mathematical
tip-off that systematic non-constant returns to scale cannot be operative –
as was known to Edgeworth, Pareto, Wicksell, Hicks and other giants.

Readers can test the robustness of my present analysis by making *all*
goods have increasing returns to scale like wheat here, or making all have
decreasing returns like iron here. Most interesting is to have firms enjoy
increasing scale returns when they are at low scales, which then turn into
decreasing scale returns at intermediate critical absolute scales. Then,
when demand for the industry is 'sufficiently large' to permit *replication*
of *many* medium-size firms, this will entail a '*quantum*'-economics indus-
try behaviour that closely approximates the constant returns to scale for
the industry that the 1960 Sraffa arithmetic needs for meaningful validity.
For 'quantum' matters, see M. F. W. Joseph (1933), P. A. Samuelson

(1967, 1973a,b). All this is 'marginalism' at its best, a critical 'critique' of the subject that should test constructively and without affect.

The Standard system's uselessness

(ii) Kurz–Salvadori reiterate the oft-read Sraffian view that, although the Standard commodity is not needed to describe and analyse effects on relative prices of changing interest rates, it has a use in that project. Having taught input/output economics to hundreds of students, I find that contrary to my experience. It is the false claims for the Standard commodity – agreed to be false by both me and Kurz–Salvadori – that first entice the students. Then, when they see how realistic induced switchings of techniques empirically do occur, they are disillusioned with it; as they are when they learn that the concept need not exist in the real number system under feasible joint product cases – and with no adverse consequences for a comprehensive understanding of income distribution, and as they are when non-labour primary factors arise in the real world, along with heterogeneities in labour itself, and as they are when non-indecomposable systems occur, and as they are..., and....

Suppose by happy coincidence the observed actual system involved productions in the exact proportions of its von Neumann balanced growth vector. Then it is the Standard commodity that is actually being produced. Then, when the interest rate is half-way between zero and its maximum possible, labour (paid *post factum*!) does get one-half of that income exactly and capital gets one-half. What a Santa Claus theory of serendipitous distribution! However, whenever this coincidence does *not* occur, actual distribution is a more complicated high-degree-polynomial expression. I deserved no criticism for speaking of shifts in the 'scale' of an industry in going through the 'thought experiment' of looking away from *actual* distribution reality to the 'auxiliary' case of the Standard commodity, and as the previous sections of this reply demonstrated, such scale changes would be fatal to the 1960 arithmetic if constant returns to scale did *not* obtain. Of course, thought experiments can be unconstrained; they can be twice irrelevant if you want to make them so.

Classical economics as merely a supply-and-demand paradigm

(iii) Kurz–Salvadori correctly point out some important differences in the posited behaviour equations of 1750–1850 classical writers as against modern 1870– mainstream economists who are labelled neoclassicists. They should know from my writings that I affirm this rather than deny

it. Thus, classicals posit a subsistence-wage determination of endogenous population, while economic demographers today entertain different hypotheses. Also, post-1870 economists offer a theory to explain the consumption demand by income receivers, whereas the classicals often ignored that issue.

None of the above touches my contention, which I know that a Luigi Pasinetti would strongly disagree with, that both classicals and moderns share the same basic paradigm that processes of supply and demand determine the competitive equilibria – long run and short run – which obtain in both their respective systems. *That* common paradigm is shared by neoclassicists like Cassel and Clark, one of whom rejects marginal utilities and marginal productivities and one of whom does not. If by your definition a 'neoclassicist' is one who (a) believes in smooth marginal productivities for produced and primary factors, and/or (b) believes in a *scalar* aggregate of capital, and/or (c) at least one of (a) and (b), then yes there is a difference in Kuhnian paradigms between the neoclassicist Frank Ramsey and the classicist David Ricardo, but what I contend is that both Ramsey and Ricardo rely on the same supply-and-demand mechanisms. (Of course I am aware of Ricardo's words claiming that he goes deeper than supply and demand.)

I tried to say, and I here reaffirm and explicate this, that a discrete-technology scenario of Sraffa or Leontief, when it has many alternative feasible techniques, can come as close as you like to a Clarkian smooth marginal-products scenario with vectoral produced inputs – as close in *all* its *qualitative* essentials of comparative statics and dynamics. Conversely, there exist smooth Clarkian technologies with *no* 'spurious margins' that can come as close as you like to any Sraffa scenario (including a 1-technique scenario) or to *any* 1817 scenario. Therefore, in a deeper sense the neo- and the classical paradigms are species of the same genus. A return by a modern Sraffian to a 'classical paradigm', if it should yield wonderful new insights, will not do so essentially because it rejects the *tools* of the modern mainstream tradition.

Every time I say something deservedly complimenting to Sraffa, that is construed to be a recantation of the methodology of mainstream economics. Properly speaking, it is rather a statement about one new thing that I have learned about the world from the genius of Piero Sraffa – about the neoclassical *and* the non-neoclassical world.

When Kurz and Salvadori say that 'Sraffa effectively demolished neoclassical economics', that is bombast. What he precisely demolished, and cogently demolished, is the erroneous notion that a lower interest rate must, if anything, raise society's producible standard of living. That erroneous notion is *not* erroneous in a Sraffa model with a single pro-

duced input; and it is not erroneous in a Clark marginal-product model with a single produced input. However, in both models, when there is more than one capital good, it can well happen – and *equally* happen! – that society's consumption plateau is higher at a higher interest rate than at a lower one. Let us render unto Caesar exactly what is Caesar's.

Classicals and post-classicals both struggled with short- and long-run distribution problems

(iii) (continued) I understood Kurz and Salvadori to hold something like the following view:

Neoclassicism (or for that matter Arrow–Debreu modern mainstream economists) tends to specify as endowments vectors of produced inputs, vectors of natural resources and homogeneous labour-supply scalars (or vectors of heterogeneous labourers), and then from these data and data on technological knowledge, they try, by supply and demand analysis, to deduce the resulting *distribution* of factors' incomes and the real relative prices of all goods and services.

What separates classical economics from these post-1870 scholars is, in their view, that before 1870 writers (a) did specify fixed vectors of 'primary land', but (b) concentrated on steady states (or stationary ones) in which exogenously specifiable real wage levels and interest-rate levels entailed the resulting endogenous permissible equilibrium quantities of outputs, real factor prices and factor requirements by industries and final distributive shares among input owners (labourers, landowners, capitalistic owners of produced inputs). When technology changed, before-and-after pictures of equilibrium each had to be constructed and reconstructed with no truly classical theory of transitional paths.

My 'scrutiny' of the literature denies this dichotomy. Yes, pre-1850 writers did concentrate much on a quasi-exogenous subsistence real wage level, asymptotically equilibrated by induced rises and falls in population numbers. Yes, Ricardo and Mill and others did have a (feebler) parallel subsistence-interest notion – never adequately spelled out and rationalized – of an effective exogenously knowable level of interest rate, above which 'accumulation' would be induced and below which 'decumulation' would be induced. However, all of them – Turgot, Smith, Malthus, Ricardo, Longfield, Mill and Mill, Senior and Marx – do deal repeatedly and at length with non-stationary processes that methodologically fall under the post-1870 rubric I have allocated to Kurz and Salvadori.

When a Pasinetti advises, back to the classical paradigm, on to a future non-neoclassical paradigm, I do not understand him to be saying: deduce steady states of population from physiological–conventional wage

levels that turn human fecundity on and off; deduce long-run interest rates from Senior–Schumpeter palaver about time preference.

Where does 1960 Sraffa fit in? His pages tell us nought about whether the interest rate will be zero or be at its maximal technological level. They say nothing about how a model with fixed homogeneous labour and land supply will have its wages/rent ratio determined, and nought about how technical change is likely to alter (wages/interest rent/interest) shares.

Why 'basics' are not basic

(iv) Generality and completeness are virtues. A non-negative matrix can be *indecomposable* or not. It can have one (principal) submatrix that is indecomposable (or *more* than one, or *none*). A simple boiler plate provides all the needed qualifications for *all* cases. Each morning in class it would be tedious to run through all the qualifications. Therefore, on lazy days at MIT I might stipulate: today we will assume that *all* goods form an indecomposable input/output $[a_{ij}]$ that is positive; all are basics. Or, today we will assume that **a** has one indecomposable subset only, and all goods outside it are positively linked with it. (That is Sraffa's convention, which Kurz–Salvadori cheer-lead for.)

To insist on *it* is bad economics. It requires that the greatest editor of classical texts must ostracize the personal services so beloved by Malthus and Ricardo. (I am not allowed to use a masseur; a barber who cuts my hair with produced scissors is *de rigueur*. What scholasticism.) The bread that is produced by labour out of the wild wheat picked off land by labour – the well-known 'Austrian' example that 1817 Ricardo and 1867 Marx could handle well, and which should have shown Marx how *sterile* and gratuitous was his *Mehrwert* innovation – is ruled out of bounds. Why this theology? No important economic theorem depends on it. No Sraffa system could ever have got started if historically there had not existed a technology that violated its dogma. Nor is it true that bread now splits off independently from all the other goods. (Nor would that be a repugnant result were it true.) Bread is still affected by iron through their common dependence upon land and labour and through their competition for the consumers' dollars.

I have no interest in fighting one theology with another. My deeper criticism is to repeat that Sraffa, with only 100 1960 pages, wasted so many of his precious words on unimportant basic vs. non-basic puzzlings. Consider again as a case in point the bare five pages allocated to *land*. Instead of treating primary land in the way that Chapters II–IX had previously treated primary labour, the author chooses to classify land as a non-basic joint product, and spends our time and impatience

on puzzling of how land fits into standard commodity palaver. I am a hungry Oliver Twist who complains about being given too little. (Would that Chapter XI had shown how to handle scarce homogeneous land and homogeneous labour in a 1-technique world, where no theory of distributive sharings and real prices, W/rent and (P_j/W), yields to Sraffian equalities, or had handled the easier case of a specific corn land and a specific rye land, where a classical subsistence-wage model of population size makes everything determinate at each interest rate after landowners' spending tastes are specified – and where their tastes determine whether rye-land rent rate is or is not zero. That might have caused the 1960 author to reword his 1926 downplaying of the rising supply price of P_{rye}/P_{wheat} when tastes shift toward rye, and it might have driven home how similar to post-1870 economics were the 1917 scenarios, especially when many techniques realistically displace a 1-technique specification.)

Let me borrow a fraction of the five 1960 pages on land to handle a scenario where wheat and iron each need as inputs *homogeneous* (transferable!) land and labour, along with some of the other goods as input: 1 wheat needs 2 land, 1 labour and $\frac{1}{2}$ iron; 1 iron needs 3 land, 1 labour and $\frac{1}{2}$ wheat. Set interest at zero: $1 + r = 1$. Then, as in the methodology of Chapter 2,

$$P_1 = W + 2R + \tfrac{1}{2}P_2, \qquad P_2 = W + 3R + \tfrac{1}{2}P_1,$$

$$R = \text{rent rate} = \tfrac{6}{3}W + \tfrac{14}{3}R, \qquad P_2 = \tfrac{6}{3}W + \tfrac{16}{3}R,$$

$$1 \le P_2/P_1 \ge 16/14$$

If there is one technique only, as in *all* of Part I, Sraffa knows he cannot find determinate real prices: $(P_2/P_1)^*$, $(P_1/W)^*$, $(P_2/W)^*$, $(R/W)^*$. As his friend Wittgenstein said: Whereof we cannot speak we must be silent.

Wrongful neglect of inequalities

(v) A besotted lover sees a black wart as a beauty mark on the face of the beloved. Kurz–Salvadori, instead of borrowing from Samuel Johnson's compliment to walking dogs and preaching women – 'It is not done well but it is remarkable that it is done at all' – pass up the opportunity to agree with the following paragraph.

Piero Sraffa was a self-taught mathematical economist who apparently never heard of the duality theorems of equalities–inequalities common to game theory, linear programming and (on careful reading of Fisher, Wicksell, Zeuthen, Neisser, Hicks, Schlesinger, Wald, von Neumann:

1890–1950) to the excellent economic literature on how slack redundancy makes a variable's price go and stay at zero, thereby avoiding negative prices under free disposability assumptions. (If disposability is *not* a free option, prices *should* go negative, which is a theorem in that mathematical literature.) Therefore, with charity and admiration we should commend Sraffa for exploring an imperfect solution, and with candour point out that it is inferior and point out where it is inferior. Sraffa's defended solution is to assume that, when a shift in exogenous data would make a 'square' technique entail negative prices, posit that there will be a convenient alternative viable square technique that still produces positive prices. Dr Pangloss would like this Sraffa wish list.

Instead of joining me in this amiable summing up, Kurz–Salvadori argue that all ad hoc assumptions are equally arbitrary. On the one hand, von Neumann antes up the *ad hoc* assumption of free disposability; on the other, Sraffa antes up the *ad hoc* assumption of there always existing a technical option that avoids negative prices. The dishonours are even. I know of no experts who would agree to that verdict after being given the problem with all proper names removed, and I tried a few.

Besides, if one accepts the Sraffians' view that only a *finite* number of technical options ever exist at each date, then the Kurz–Salvadori defence of Sraffa (which he himself never pressed) fails, and fails generically. Here is an example in the widely known Stigler–Cornfield 'least cost adequate diet' instance of jointness. 'You must daily get *at least* 10 calorie units *and* 10 vitamin units. Three goods are known to provide respectively the following number-per-unit of (calories vitamins) equal for the goods $(X_1\ X_2\ X_3)$ respectively to (3 2), (2 3), (4 1). If the goods all have equal unit prices, say \$1 each, what is the cheapest diet to buy?' (Stigler, primarily a literary economist, published on this in 1945 before George Dantzig (1951) had published in the *economic* literature the definitive mathematical theory of linear programming.)

Readers can verify, by trial and error or the *simplex* algorithm, that $(x_1^*\ x_2^*\ x_3^*) = (2\ 2\ 0)$ defines the cheapest diet. Yes, the relevant matrix is 'square' in the sense that no more than two goods need be bought to provide two nutrients. Modify the problem by raising the calorie requirement a little (but not too much) and indeed a new 'square' solution with sign $(x_1^*\ x_2^*\ x_3^*) = (+\ 0\ +)$ will emerge. However, proceed to modify the problem a lot, so that say at least 100 calorie units are now needed, while the minimal vitamins needed stay at 10. Sraffa's proposed dodge – which did work at first to avoid negative numbers by staying with a new 'square' matrix when the calorie requirement was raised by *not too much* – now does definitely fail. Only one of the possible goods can be optimally bought when the calorie/vitamin specification is made large enough,

and Sraffian equalities are then definitely made inapplicable. Only X_1 can now be bought, making vitamins redundant with a zero imputation value. Even if out of pity we let Kurz–Salvadori have a fourth good or a 999th good, there will always be a calorie requirement that will mandate non-squareness of the relevant matrix. QED. For my money, von Neumann dominates over the surrogate for Sraffa proposed by his zealous followers. (The correct Dantzig theorem is that the number of goods positively bought need never *exceed* the least of [number of nutrients, number of goods]: it can, though, have to fall short of both of the pair in brackets.)

'Prices of production' vs. 'cost of production'

(vi) Kurz and Salvadori agree with me: Yes, Sraffa's 'prices of production' are precisely 'competitive costs of production', and Sraffa explicitly recognizes that. Yes, Sraffa recognizes that the minimized costs of production must involve simultaneous equations, and the logical fact that they can always be mathematically solved self-demonstrates that the circle involved is a virtuous not a vicious one. Yes, Kurz–Salvadori and Sraffa recognize that many scholars – I offer Joan Robinson as one and Marx as another; Kurz–Salvadori offer Böhm–Bawerk for a third – wrongly regard simultaneous equations as a swindle. I do not know on what evidence Kurz–Salvadori attribute to Sraffa the view that some of these opponents believe simultaneous equations can be 'circumnavigated'; my experience is that such (mistaken) sceptics generally believe that those paradigms involving circular interdependences are unsolvable and are a blemish on their opponents' economics of various subcenturies.

With all this agreement, why was my mild remark about the non-optimality of Sraffa's choice of the nomenclature 'prices of production' not applauded? Sraffa's own defence (1960, p. 8) is to liken this decision to his decision not to use in the book the 'term "capital" in its quantitative [i.e., *scalar*] connotations'. I applaud *this* latter choice by Sraffa. Models with but *one* scalar produced good have different and special properties as compared to those with vectors of heterogeneous produced goods, and Sraffa's work I deem a classic, not because it reinvents some wheels of Frobenius–Leontief matrixes, but because it demonstrates even better than Joan Robinson's 1956 explorations how basic are these scalar-versus-vector differences. I must also reaffirm what both Joan and Piero denied in separate conversations with me: those vital differences between scalar and vectoral produced-input technologies are *not* differences between smooth Clark–Samuelson marginal productivity models

and von Neumann–Leontief–Sraffa discrete-techniques models. The differences are *common* to *both* techniques!

Why link 'cost of production' nomenclature to '[scalar or aggregate] CAPITAL' nomenclature? Where arises the nice abstention from prejudicing some cost issue by avoiding a still-suspect term? Surprisingly, Sraffa asserts (1960, p. 8) that cost of production has come 'to be inseparably linked with the supposition that [it stands] for quantities that can be measured independently of, and prior to, the determination of the prices of the products. (Witness the real costs of Marshall...).' I find this odd. Marshall was above all an eclectic simultaneous equation methodologist, in contrast to unidirectional writers like Böhm and Mill.

I would turn the nomenclature choice upside down. It was Marx who was the erroneous critic of Smith's resolution of steady-state long-run price into the sum of value-added wage–interest–rent components, and it was Marx who therefore used those words 'prices of production'. (As Dmitriev was the first to elucidate in 1898, Marx's gripe that Smith sends us from pillar to post in an infinite regress [actually *convergent* infinite sums!] is in fact answered by Marx's own tableau of simple reproduction when *its* simultaneous equations are properly formulated. No fool ever accused Sraffa of not knowing Marx's writing.)

When Sraffa rightly says competition select a cheapest technique, in terms of what is 'cheapness' measured? For his model it is cheaper *cost of production* (!) and nothing else that rules – cheaper total cost of all needed inputs, each input price being evaluated at the real price vector that is minimal. (For the correct theories of pure *exchange* by Jevons (1871) and Edgeworth (1881) another cheapness is guiding.) Sraffa's explication of a switch point on page 83, which Kurz–Salvadori cite, makes needless heavy weather over basic–non-basic babble. One rule only applies in competition and the scale returns it presupposes.

Synthesis

Finally, I can react to points that pertain to all of the Kurz–Salvadori (i)–(vi) comments and to their first two footnotes.

Their Footnote 2 presumably agrees that *increasing* scale returns do put us into Chamberlin–Robinson imperfect competition, but I believe they want to dissent where *decreasing* scale returns obtain for firms. No disagreement from me. Here is the needed special re-analysis for *decreasing* returns to scale. I deny that firm production functions could ever in the real world systematically and uniformly be everywhere homogeneous-of-positive-*fraction* degree. How can Dr. Samuelson tell the real world how to behave? I do not have to tell the world that: 'Industry output in

infinite amount cannot be producible by a *finite* vector of inputs'; the same world that knows perpetual motion machines are impossible itself knows that unit costs at the industry level cannot be brought down indefinitely close to zero by spontaneous replication of infinitely many firms each producing infinitesimal amounts of output. That is not Xeno's paradox, it is Xeno's nonsense, as Wicksell insisted. Therefore, what decreasing scale returns could there be to have to worry about? (It is irrelevant to confuse decreasing *scale* returns and good old *diminishing returns* to a subset of factors, say labour, while another subset, say land, is being held constant.) The experienced reader will recall that such a confusion has occurred in connection with discussions of 1926 Sraffa. Even Kurz-Salvadori's *Comment*, purporting to controvert any claim about an 'empty set of results', adduces as non-empty *positive* result a case where 'certain qualities of land are in short supply' and in consequence a rise in taste for burgundy will raise its price. I say 'Bravo' to late recruits to the regular pre-1926 army, but diminishing returns to variable labour applied to limited land is completely in accord with standard constant returns to scale! Sraffa himself, in his rare texts and in 1948–1958 conversations with me, repeatedly used nomenclature that similarly confused the reader by virtue of not explicitly distinguishing scale returns from proportion-of-factors returns and, as has been shown again and again, the *partial* equilibrium tools of Marshall are *not* self-contradictory in such cases. If Kurz–Salvadori will re-read Allyn Young (1913), Robertson (1924) and Knight (1924), they will understand that induced external-to-the-firm diseconomies – such as rising land rents and falling wage rate/price$_q$ as industry output expands – are precisely what Walras mainstream economics is all about. Marshall and Pigou nodded when they confused 'smoke nuisance', a *technological* externality which might need to be corrected by penalty taxes, with induced rises in relative factor prices as industries expand (a *pecuniary* externality). My point was never that competitive analysis is empty of results when multiple primary factors occur; it is that the 1960 Chapter II and Chapter XII matrix arithmetic was not augmented so as to handle it. When we augment Sraffian arithmetic to handle properly the more general scenarios, all is well – *except* when, in the augmented space of [male labour, female labour, high-quality pasture land, low-quality pasture land, vineyard land], constant returns to scale is systematically denied; then that augmented matrix arithmetic will not apply. In short, Sraffa should have replied to Keynes: 'I suspect that, after I think it through, I will want to restrict my competitive analysis to constant returns to scale models where total revenues must be exhausted by total costs [inclusive of equalized profit rates]'.

In certain absolute scale ranges, I gladly admit, decreasing returns to *scale* could obtain, but below certain critical scale levels, *increasing* scale returns must assuredly obtain in the lumpy world of Democritus–Planck–Einstein–Bohr. As in the Joseph–Samuelson quantum economics world already referred to, the industry oscillates in a damped way toward constant returns to *its* scale as demand permits replication of $1, 2, \ldots, 10, \ldots$ 99 viable firms (each with U-shaped long-run unit cost schedules). 1960 Sraffa arithmetic then handles tolerably well the N-large case where constant returns to scale tolerably holds for the industry. If not, not. Paging Chamberlin–Robinson. (A *curiosum*: Let returns to scale decrease like $q = f(L)$ – with $f'(L) > 0 > f''(L), f(0) = 0 < f'(0) < \infty$, with L being the only input. Then by replicating enough 'infinitesimal firms', the industry approaches as closely as we wish to a first-degree homogeneous $F(L) = f(1)L$ and, as I insist while Sraffa reserved disagreement, the 1960 arithmetic then and only then possesses relevance. Again, if not, not. The ball again is in the Sraffians' court.)

The theory of 'contestable markets', dreamt up originally by consultants to firms indicted for anti-trust violations, lacks credibility when systematic deviations from free-entry replicability of existing firm(s) by potential new entrants do obtain. But suppose this were not so: let a large monopolist, fearing potential new entrants, price his q down to (falling) average cost and tolerate $P <$ marginal revenue. Now test the truth of Sraffa's 1960 theorem that this product's relative price is unaffected by a change in consumers' tastes. Although Sraffian matrixes proclaim precisely that, what reader believes in *that* truth now?

The upshot of economic theory and of economic history, I believe, is common to classical and 1870–1970 writers, as well as post-1970 mainstream scholars. It holds that post-Newton technological change, plus accumulation of copious vector elements of produced inputs, is what creates the rising affluence of populations The differential sharings of that affluence, among people who differentially own amounts of the heterogeneous productive inputs, is influenced by changes in the relative total supplies of factors of production: The Netherlands, as Smith claimed, had greater prosperity and lower interest rates than (say) Portugal because the Dutch had sacrificed some past current consumptions in order to accelerate accumulation (of *vectoral* capitals). Yes, I know about the possibilities of reswitching and about the difficulties of the Hahn problem – the puzzle of how, in an uncertain world with incomplete Arrow markets, somehow an approximation to intertemporal Pareto-optimality seems to characterize the micro-allocations of macro-

accumulations. That super-sophistication must not divert me from what is all-important.

A generation of post-Sraffians have had their attention turned away from these important classical and post-1870 matters by a preoccupation with long-run steady-state models based upon nihilistic specifications tolerating any and all profit rates and exogenous (!) subsistence wage rates. Such steady states are the coward's way out, unless they are cogently deduced as asymptotic limits of non-stationary real-price and real-output proportionalities, asymptotic limits that are constantly changing in a Schumpeterian world. I mourn a lost generation whose counsel and empirical research are sorely needed.

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