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The Theory of Value and Distribution in Economics

Discussions between Pierangelo Garegnani and Paul Samuelson

Edited by
Heinz D. Kurz

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Contents

The Theory of Value and Distribution in Economics

This new volume explores two alternative economic theories – the classical theory and the marginalist or neoclassical theory – through a discussion between two eminent economists, Pierangelo Garegnani and Paul Samuelson.

The key themes of the volume are the difference in approaches to the explanation of the distribution of income and relative prices, and therefore different approaches to all other economic problems, in particular capital accumulation and economic growth. The book discusses whether there is a 'classical' approach to the theory of value and distribution at the core of economic theory that is fundamentally different from the later marginalist or neoclassical theory. In the volume, the late Pierangelo Garegnani argues for the validity of Piero Sraffa's position on this issue, whilst the late Nobel laureate Paul Samuelson vehemently contests it.

At a time of economic crisis, the future of the discipline is far from certain, and so it is extremely important to bring these debates back into the light, by reproducing them together for the first time. A comprehensive introduction by Heinz Kurz sets the debate in this context, and provides crucial background to the arguments.

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Contributors

Pierangelo Garegnani (1930–2011), literary executor of Piero Sraffa's papers and correspondence, was Professor at the University of Rome III and contributed in important ways to the revival of the Classical approach in economics. In his view the history of economic thought is not a relentless march towards an ever better understanding of economic phenomena, as the premature abandonment of the analysis of the classical economists in favour of that of the marginalists demonstrates. The marginalist approach is considered to be flawed, because of a defective concept of capital.

Paul A. Samuelson (1915–2009), Nobel laureate in economics in 1970, held a chair at the Massachussetts Institute of Technology and was arguably one of the most influential economists of the twentieth century. He advocated a 'Whig perspective' on the history of economic thought, according to which modern theory preserves all that was good in past contributions and eliminates all that was untenable. This led him to interpret the work of Piero Sraffa as a contribution to this march and not as a critique of the marginalist doctrine and an elaboration of an alternative 'classical' approach.

1 Is there a 'classical' alternative to marginalist theory?

Some introductory remarks

Heinz D. Kurz

When this volume was planned, Pierangelo Garegnani was still with us. He would have loved to see it in print, but was not given the opportunity. He passed away on 15 October 2011 at the age of 81.[1]

Since spring 2011 I had known that he was suffering from some illness relating to his lungs, but he did not disclose to me precisely what it was. Since at the time I was also suffering badly from a lung infection he perhaps did not want to alarm me unnecessarily. However, I remember him complaining on one occasion, talking on the phone, about the 'nuisance' of having to die. He still had so many things to do, ideas to work out, papers to complete, etc. I did not then interpret what he said as an indication of what he expected to happen before long. I had always experienced Pierangelo as a man of the long, ultra-long run, who forbade himself to take into account the finiteness of his own life. I did not even hear the alarm bells ring when, two days before he died, he called me, asking: 'Heinz, will you do the big thing?' I immediately thought that he was referring to the edition of Piero Sraffa's unpublished works and correspondence of which he, in his capacity as Sraffa's literary executor, had invited me to be the general editor.[2] I answered that, being on the mend after my illness, which had put me out of action for several months, I was about to get back to the Sraffa project. But he replied with a sigh: 'No, no. I am referring to the edition of my debates with Samuelson.' I assured him that I would do it and that as soon as I had drafted the introduction around the turn of the year I would get back to him. He was relieved, thanked me and we said goodbye to each other. It did not occur to me that his call was meant to settle things that were important to him before it was too late. Two days later, in the morning, Neri Salvadori called me from the University of Rome III, where a conference was taking place and where Garegnani had taught until his retirement and still acted as Chair of the Fondazione Centro Piero Sraffa di Studi e Documenti. He informed me that Pierangelo was no longer with us.

1 Is there a 'classical' approach to economic theory?

In the general introduction in Volume I of his edition of *The Works and Correspondence of David Ricardo* (Ricardo, 1951), Piero Sraffa had indicated that

Ricardo's approach to the problem of value and distribution was founded on the notion of social surplus. This approach was not an early and rude version of later marginalist theory, which tried to explain prices and income distribution in terms of the opposing forces of demand and supply, as Alfred Marshall and others had contended. It was a fundamentally different theory, which, alas, had been 'submerged and forgotten since the advent of the "marginal" method', as Sraffa was to stress in his *Production of Commodities by Means of Commodities* (Sraffa, 1960, p. v). That book was designed to accomplish two closely intertwined tasks: first, to reformulate and develop the 'standpoint … of the old classical economists from Adam Smith to Ricardo' in a coherent form; and second, to lay the foundation 'for a critique of [the marginal theory of value and distribution]' (Sraffa, 1960, pp. v, vi). Sraffa added: 'If the foundation holds, the critique may be attempted later, either by the writer or by someone younger and better equipped for the task' (1960: vi).[3]

Garegnani was one of those younger scholars. Others included, to mention but a few, Luigi Pasinetti, also a student of Sraffa's, the late Krishna Bharadwaj, John Eatwell, Geoff Harcourt, Bertram Schefold and Ian Steedman. Garegnani was at the forefront of those who carried out the critique in the so-called 'Cambridge controversies in the theory of capital' (for summary accounts, see Harcourt, 1972; Garegnani, 1990a; Kurz and Salvadori, 1995, ch. 13). It is in this context that he first got involved in debates with Paul Samuelson, the main representative of the Cambridge, Massachusetts, side in the controversy.

In 1953 the Cambridge, United Kingdom, economist Joan Robinson had published a frontal assault on the concept of the aggregate production function and the marginalist concept of capital (Robinson, 1953). She based her argument to a considerable extent on ideas she had picked up in conversations with Sraffa. Paul Samuelson from the Massachusetts Institute of Technology (MIT), in an attempt to counter the attack, contended that even in cases with heterogeneous capital goods some justification can be provided for the employment of simple neoclassical 'parables' which assume that there is a single homogeneous factor called 'capital', whose marginal product equals the rate of return on capital. His paper on 'Parable and realism in capital theory: the surrogate production function' was published in 1962 (Samuelson, 1962) and quickly became a focal point in the controversy in capital theory.

Garegnani spent 1961–1962 at the MIT on a fellowship of the Rockefeller Foundation. He met Paul Samuelson and was given the opportunity to read the latter's paper prior to its publication. He immediately spotted a major shortcoming in the argument and told Samuelson accordingly: in constructing the 'surrogate production function' Samuelson had boldly assumed uniform capital–labour ratios as between the machine-producing and consumer-good producing processes.[4] This was an extremely special case, in which the labour theory of value explained relative prices in a straightforward way, whereas the argument does not apply in the only interesting, while realistic, case of different capital–labour ratios. Therefore Samuelson's construction could not be considered a defence of the 'Clark–Ramsey Parable'.

Before I continue, it is perhaps worth noting that in a note written as early as 16 January 1946 Sraffa had anticipated *ante litteram* the flaw in Samuelson's argument. Sraffa had written:

> The Irony of it is, that if the 'Labour Theory of Value' applied exactly throughout, then, and only then, would the 'marginal product of capital' theory work!
>
> It would require that all products had the same org.[anic] comp.[osition]; and that at each value of *r* [rate of interest or profits] <u>each</u> comm.[odity] had an 'alternative method', and that the relations within each pair should be the same (i.e. that marg.[inal] prod[uct]s. should be the same; + also the elasticities should be the same); so that, even when the System is switched, and another Org. Comp. came into being, it should be the same for all products.
>
> Obviously this would be equivalent to having only one means-product (wheat).
>
> Then, commodities would <u>always</u> be exchanged at their Values; and their relative Values would not change, even when productivity of labor [sic] increased.
>
> (Sraffa Papers D3/12/16: 34; Sraffa's underlining)[5]

Garegnani worked out his criticism of Samuelson's argument and showed not only that the aggregate production function is generally untenable, but also that all long-period microeconomic versions of marginalism starting from a concept of capital whose quantity can be ascertained independently of income distribution and prior to the determination of prices are invalid. In particular, there is no reason to presume that the capital–labour ratio moves inversely to the interest–wage ratio.[6] By assuming equal proportions of labour and capital Samuelson turned the 'real' economy with heterogeneous goods into an 'imaginary' economy with a homogeneous output. In other words, the surrogate production function was nothing more than the aggregate production function. The publication of Garegnani's essay was delayed. Submitted to the *Review of Economic Studies* in April 1963, it was accepted for publication subject to revision shortly afterwards. Garegnani submitted a substantially enlarged version in October 1968. The essay eventually appeared in 1970 entitled 'Heterogeneous capital, the production function and the theory of distribution' (Garegnani, 1970). It soon became one of the most often cited works in the capital controversies.

Prior to this, Garegnani was involved in a debate that was also indirectly triggered by Samuelson, who had asked a student of his, David Levhari, to investigate whether the return of the same technique, its 'reswitching', at different levels of the rate of interest was logically possible. In his paper Levhari (1965) contended that it was not and that entire systems of production can be ordered monotonically according to 'degrees of mechanization'. This claim was disputed by Luigi Pasinetti (1966), Garegnani (1966) and others and gave rise to a symposium published in the *Quarterly Journal of Economics*. Samuelson and Levhari in their joint paper to the symposium and Samuelson in his 'Summing

up' paper (Samuelson, 1966) frankly admitted that the no-reswitching 'theorem' was wrong. Samuelson also provided some numerical examples which illustrated in simple terms why reswitching and capital reversing are possible.

But how important were these phenomena and did they effectively undermine marginalist theory? To Samuelson the most disturbing result was the finding in steady-state capital theory that consumption per head could be positively related to the rate of interest. But since, he contended, reswitching and capital reversing do not upset Pareto optimality their importance was limited, a view that was strongly contested by Garegnani and others.

In addition, in a number of papers, the most important of which is perhaps his essay 'The canonical Classical model of political economy' (Samuelson, 1978), he maintained that there are no fundamental differences between the theories of the classical authors and those of the later marginalists. To him, Smith, Ricardo, etc. were essentially marginalist theorists waiting to be born. He maintained that there is a fundamental *unité de doctrine* across all 'schools' of economic thought, once each particular doctrine has been purged of concepts that lack clarity, errors of reasoning and special assumptions. While Samuelson had influential followers in this regard, including John Hicks, he drew criticisms from numerous scholars coming from different fields and orientations in economics, and especially from historians of economic thought, who felt that he was patching over important differences between and within different schools of thought. However, the main criticisms of his point of view came from scholars working in the tradition of Piero Sraffa, especially Garegnani.[7]

Before we turn to this, two further events ought to be mentioned. First, in his keynote address at the History of Economics Society meeting in Boston in 1987, reflecting his *unité de doctrine* point of view, Samuelson proposed a programme for what he dubbed the 'Whig history of economic science' (Samuelson, 1987a). By this he meant a re-orientation of the history of economic thought 'toward studying the past from the standpoint of the present state of economic science' (p. 52). He motivated his proposal by asserting that there were no 'Kuhnian breakthroughs in current economic science' and that 'ours is not an age of heady accomplishments and new exciting syntheses' (p. 52). As an exemplification of the alleged cumulative character of the normal science of economics he mentioned Piero Sraffa's edition of *The Works and Correspondence of David Ricardo* (Ricardo, 1951–1973) and his reformulation of the classical approach to the theory of value and distribution (Sraffa, 1960). This was, of course, a deliberate provocation of those convinced by Sraffa's message that there is a classical alternative to marginalism.

Second, prior to this Samuelson had been invited to contribute a paper to a conference in Florence in 1985 commemorating the twenty-fifth anniversary of the publication of Sraffa's book. While he could not himself participate in the meeting, he sent a paper entitled 'Revisionist findings of Sraffa' (Samuelson, 1990), which was published in a volume edited by Krishna Bharadwaj and Bertram Schefold (1990), followed by comments by Eatwell, Garegnani and Schefold. Samuelson's paper consists essentially of a follow-up to his entry

'Sraffian economics' in *The New Palgrave* (Samuelson, 1987b), reiterating the view that some of Sraffa's propositions, especially those concerning the problems of constant returns to scale, joint production and the Standard Commodity, cannot be sustained and that Sraffa's analysis does not constitute an alternative to marginalism (see, in particular, Samuelson 1990, section 14). An implicit reply to Samuelson was contained in Garegnani's entry 'Surplus approach to value and distribution' in *The New Palgrave* (Garegnani, 1987),[8] and an explicit one in his comment in the Bharadwaj–Schefold volume (Garegnani, 1990b). In the latter, Garegnani tried to clarify the relationship between the analyses of Ricardo, Marx and Sraffa on the one hand, and their differences from the marginalist theories on the other. He concluded that

> 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.
>
> (Garegnani, 1990b, p. 297)

Apparently, there was a wide consensus among the debatants, especially Samuelson and Garegnani, that many of the problems raised had not yet been satisfactorily solved and that a continuation of the debate would be welcome. Or, as Samuelson had put it in one of his earlier papers: 'In this age of Leontief and Sraffa there is no excuse for mystery or partisan polemics in dealing with the purely logical aspects of the problem' (Samuelson, 1971, p. 154).

Another round of exchanges was organised, this time by the author of these lines. It led in 2000 to the publication of a volume entitled *Critical Essays on Piero Sraffa's Legacy in Economics* (Kurz, 2000). I had invited a number of scholars to contribute to the volume, including Paul Samuelson. He kindly accepted the invitation on the ground that what was at issue was 'an important topic' and sent me his paper entitled 'Sraffa's hits and misses' in good time (Samuelson, 2000, p. 111). It was agreed among the contributors to the volume that whoever wished to comment on another scholar's paper could do so within a given time span. The addressee of the comment would then be given the opportunity to reply to the comment, again within a given amount of time. Garegnani was, of course, keen to comment on Samuelson's paper, but he was also keen to contribute his own paper (Garegnani, 2000) and to comment on a paper by Samuel Hollander on Malthus and Ricardo. He first worked on the latter tasks and wanted to tackle Samuelson's piece only subsequently. However, given his meticulosity and quest for precision in thought and expression it came as no surprise to me (and others) that he could not accomplish all the tasks he had shouldered within the time available and so he asked me to grant him some extra time for his comment on Samuelson. This I did, upon consultation with Paul. After the additional months had elapsed Piero asked me for still more time, which I

reluctantly accepted on his promise to deliver for real in a short time: 'It is only a question of two or three weeks', he assured me. Alas, it was not. Eventually I told him that I could no longer make the other contributors wait and if I did not have his comment on hand within two weeks, I would deliver the material to the publisher without it. This is what happened. Regrettably, the *Critical Essays* appeared without a reply to Samuelson by Garegnani.

In the following years Garegnani worked hard on his reply to Samuelson. In 2005 he finally managed to finish the paper. This brings me to the origin of the present volume.

2 The origin of the present volume

Pierangelo Garegnani was keen to get some of his debates with Paul Samuelson published in a single volume. He knew that he was not very good at organising such things and he feared that if he had to introduce the volume himself he might get stuck in writing and re-writing the introduction time and again. Such things had happened to him several times before and caused him difficulties with editors and publishers. When Piero was supposed to put his hands on one of his manuscripts in order to get it ready for publication, he would rethink and rephrase the argument, change the line of reasoning, drop a passage here and add one there, reformulate bits and pieces or entire sections, introduce new vistas on a problem, search for better expressions, etc. Writing is a way of thinking, and thinking is a process without a prefixed end. He was a perfectionist, always on the lookout for a better solution to a given problem. Therefore he was never completely happy with what he had achieved and was reluctant to submit his papers for publication. He was in this regard similar to Sraffa. There was always a superior option ahead, which had to be grasped. Deadlines were for ordinary scholars, not for someone desperately struggling to get to the truth of the phenomena he studied. Piero's quest for truth and precision could not easily be stopped by such mundane things as delivery dates, space constraints, format requests, etc.

This was, on the one hand, one of his outstanding and truly admirable qualities – a concern with undiluted scholarship and an almost total lack of respect for some of the usual academic conventions. On the other hand, it could easily drive a wedge into his relationship with others, especially his friends. I experienced this on several occasions myself, on which Piero was unwilling to accept previously agreed terms, because he felt that they stood in the way of the imminent progress of his work.

As already indicated, Garegnani's reply to Samuelson's paper on 'Sraffa's hits and misses' took several more years to materialise. In late summer of 2005 Piero gave me his long essay on 'Professor Samuelson on Sraffa and the Classical economists' and asked me for my assessment and advice of what to do with it. I was in Rome at the time, and after having read it I discussed it with him. I suggested he submit it to the *European Journal of the History of Economic Thought* (*EJHET*). As one of the editors of the journal I would do my best to get it published despite its abnormal length for a journal article. I also expressed my

intention to involve Paul Samuelson, who was a member of the editorial board of the *EJHET*, and ask him to write a reply, with Garegnani being given the opportunity of a rejoinder. This would finally bring to fruition what had not worked out on the occasion of the *Critical Essays* project .

When Garegnani eventually submitted his essay to the *EJHET*, we had some discussion among the managing editors on whether or not to publish so long a piece, but there was soon agreement that it was a major paper. In this we were supported by the reports of referees. After some minor revisions (I helped Garegnani to shorten it a bit) the essay was published in 2007 (Garegnani, 2007a). Samuelson agreed to reply to Garegnani in a paper entitled 'Classical and Neoclassical harmonies and dissonances' (Samuelson, 2007), which appeared in the same issue of the *EJHET*. Garegnani's 'Samuelson's misses: a rejoinder' (Garegnani, 2007b) came out in the following issue of the journal.

Since Samuelson (2000) and the papers published in the *EJHET* form a unity, Piero in late 2009 approached me with the idea of bringing out the entire exchange in a single volume. This needed, of course, the consent of Paul Samuelson. Alas, he passed away on 13 December 2009, before he could be contacted.[9] The organisation looking after his literary heritage kindly gave permission to reprint his papers under consideration here.

The composition of the present volume is the following: Chapter 2 reproduces Samuelson's paper on 'Sraffa's hits and misses'; Chapter 3 is Garegnani's paper on 'Professor Samuelson on Sraffa and the Classical economists'; Chapter 4 is Samuelson's reply 'Classical and Neoclassical harmonies and dissonances'; and Chapter 5 is Garegnani's 'Samuelson's misses: a rejoinder'.

Let me end on a personal note. I had the good fortune to be on very good terms with both scholars most of the time and benefited a great deal from reading their works and having had the privilege of discussing and corresponding with them. Paul Samuelson knew that my orientation in economics was and remained different from his. This did not prevent him from always treating me well and supporting me in several ways. I have great respect for him as a man and a scholar.

My relationship with Pierangelo Garegnani was more difficult. We knew each other for a much longer time and were much more closely related. He could be very charming, but he could also be difficult and was easily suspicious of other people's intentions. I believe that some of his disturbing characteristics had to do with his conviction that the intellectual legacy of which he was the custodian and which he sought to preserve and increase was very precious and should not be 'submerged and forgotten' once again. Therefore he considered with extreme reservation if not outright opposition developments he took to deviate from what, to him, was the right orientation in economics. Progress in the field of economics, he was convinced, was slow, very slow, and frequently what purported to be progress actually implied regress. The premature abandonment of the classical approach was the most important case in point. This does not mean that Garegnani would not listen to objections to the views he held, but since he had thought them through very carefully, he would not easily give in to the critic. He

was an outstanding scholar and deep thinker, whose work had an important and lasting impact on me.

I am convinced that the works of Garegnani and Samuelson are destined to be essential reading for the profession for a long time to come.

Notes

1 For obituaries of Garegnani, see Parrinello (2012) and Kurz (2012).
2 Sraffa passed away in 1983.
3 We know from Sraffa's unpublished papers that in the 1960s he was working on the critique, but for various reasons, particularly deteriorating health, he could not accomplish the task.
4 In his paper Samuelson acknowledges that Garegnani had pointed out to him the problematic character of the assumption.
5 The reference is to Sraffa's papers kept at Trinity College Library, Cambridge, UK, as they were catalogued by Jonathan Smith, archivist.
6 Interestingly, a positive rate of interest is not indispensable in order to see that non-conventional results may emerge even in conditions that appear to be favourable to the marginalist approach, provided one focuses attention on *full* industry equilibrium. As Ian Steedman and Arrigo Opocher have shown in a number of papers and now in a book in progress (Opocher and Steedman, 2012), even with a rate of interest equal to zero there is generally no reason to presume that quantities of factors employed (per unit of output) are inversely related to 'factor prices'.
7 It deserves to be mentioned that a revised version of Garegnani's 1958 Cambridge PhD thesis on the problem of capital in various theories of income distribution was published in Italian in the same year as Sraffa's book (Garegnani, 1960) and was then translated into several other languages. It demonstrates impressively Garegnani's familiarity with the works of major economists, especially Ricardo, Böhm-Bawerk, Wicksell and Walras.
8 A second entry by Garegnani commissioned for *The New Palgrave* on 'Quantity of capital' could not be published in the dictionary because Garegnani could not deliver his piece in time. It eventually appeared as an addition to a collection of entries from *The New Palgrave* devoted to the theory of capital; see Garegnani (1990a).
9 For an obituary of Samuelson, see Kurz (2010).

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2 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 *inter-temporal 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

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

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), or 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).

Pedantry. 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 as *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, 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 editions 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 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 a 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.

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:

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

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

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

$$Q^{t+1} = f^1[Q^t_{11}, Q^t_{12}] = f^1\,[280 \text{ qr. wheat}, 12 \text{ t. iron}] = 400 \text{ qr. wheat}$$

$$Q^{t+1} = f^2[Q^t_{21}, Q^t_{22}] = f^2\,[126 \text{ qr. wheat}, 8 \text{ t. iron}] = 20 \text{ t. iron} \tag{2}$$

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

$$Q^{t+1} = Q^t_{11} + Q^t_{12} = 280 \text{ qr.} + 120 \text{ qr.} = 400 \text{ qr. wheat}$$

$$Q^{t+1} = Q^t_{21} - Q^t_{22} = 12 \text{ t.} + 8 \text{ t.} = 20 \text{ t. iron} \tag{3}$$

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*:

$$Q^{t+1} = 400 \text{ Min } [Q^t_{11}/280,\ Q^t_{12}/12]$$
$$\text{Min } [Q^t_{11}/280,\ Q^t_{12}/300]$$
$$\text{Min } [Q^t_{11}/a_{11},\ Q^t_{12}/a_{12}]$$

$$Q^{t+1} = 20 \text{ Min } [Q^t_{21}/120,\ Q^t_{22}/8]$$
$$\text{Min } [Q^t_{21}/120,\ Q^t_{22}/8]$$
$$\text{Min } [Q^t_{21}/a_{21},\ Q^t_{22}/a_{22}]$$

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

By convention, if an input–output technical coefficient a_{ij} is zero, we agree to omit its Q^t_{ij} from the Min $[\ldots\ Q^t_{ij}/a_{ij}, \ldots]$ 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:

$$Q_1 = 400(Q_{[illegible]}/280)^{[illegible]}(Q_{[illegible]}/12)^{[illegible]},$$
$$Q_2 = 20(Q_{[illegible]}/120)^{[illegible]}(Q_{[illegible]}/8)^{[illegible]} \qquad (5)$$

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 snap-shot, 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 Vortkiewicz (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 basic 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

$$Q_j^{t+1} = \{\mathrm{Min}[Q_{1j}^t / a_{1j}, \ldots, Q_{nj}^t / a_{nj} / a_{nj}]\}^2 Q_j, \qquad j = 1, \ldots, n \qquad (6)$$

or try rewriting Equation (6) with the exponent 2 changed to 9/10, as will *diminishing* returns to scale, or let each jth 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 $i, j = 1, \ldots, n$,

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

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 terms '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:

$$\begin{aligned} &350 \text{ wheat} + 15 \text{ iron} \rightarrow 500 \text{ wheat} \\ &90 \text{ wheat} + 6 \text{ iron} \rightarrow 15 \text{ iron} \end{aligned} \tag{8}$$

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 wheat + 6 iron → 200 wheat

60 wheat + 4 iron → 10 iron

Island Z

200 wheat + 16 iron → 400 wheat

200 wheat + 4 iron → 20 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, $g^* = 2^1/_3$ 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

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

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

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

$$\begin{aligned} Q_j &= \text{Min}[L'_j / a'_{Lj}, T'_j / a'_{Tj}, Q'_{1j} / a'_{1j}, Q'_{2j} / a'_{2j}, Q'_{3j} / a'_{3j}] \\ &\quad + \text{Min}[L''_j / a''_{Lj}, T''_j / a''_{Tj}, Q''_{1j} / a''_{1j}, Q''_{2j} / a''_{2j}, Q''_{3j} / a''_{3j}] \\ &\sum_j Q'_{ij} + \sum_j Q''_{ij} = C_i, \quad 0 < C_i \quad \text{consumption of } i \end{aligned} \tag{10}$$

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

$$\text{Min}\Big[(Wa'_{0} + Ra'_{0})(1+r) + \sum_{i=1}^{3} P_i a'_{i}(1+r)(Wa'_{0} + Ra'_{0})(1+r) + \sum_{i=1}^{3} P_i a''_{i}(1+r)$$
$$- P_j \quad j = 1, 2, 3$$
$$- Wa'_{0}\{R/W, 1+r\} + Ra_{1}a'_{0}\{R/W, 1+r\}[I - a^{0}\{R/W, 1+r\}(1-r)]^{-1}$$
$$- Wa_{1}\{R/W, 1+r\} + R\,a'_{0}\{R/W, 1+r\} \tag{11}$$

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 $(a_{Lj} \ldots a_{3j})'$ and $(a_{Lj} \ldots a_{3j})''$ vectors are replaced by an infinite variety of alternative $(a_{Lj} \ldots a_{3j})$ coefficients connected by each good's relation(s):

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

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:

$$1 - r = \Phi^{j}(R \cdot P_j / W \cdot P_1), \quad j = 1, 2, 3 \tag{13}$$

where $-\Phi^j(\ )$ is a quasi-concave function that is monotone-increasing. Reswitching or the mentioned permitted reversals in the $(1 + r$, consumption 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.
3 When the real world offers *alternative* techniques,

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

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 2.1 illustrates neoclassical versions of neo-neo-Ricardianism, and various Sraffian approximations to them. In Figure 2.1(a), AA′ is the neoclassical production-possibility frontier in the short run when supplies of labour, land and capital are fixed. Figure 2.1(b) shows the three factor prices (W/P_{corn}, R/P_{corn},

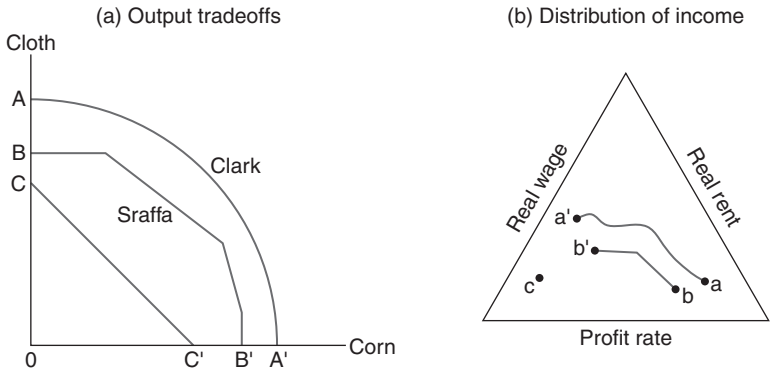

Figure 2.1

$1 + r$) 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′.

In Figure 2.1(a), BB′ is the Sraffian counterpart to AA′; in Figure 2.1(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 2.1(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 $P_{\text{cloth}}/P_{\text{corn}}$ ratio since, as shown in point c in Figure 2.1(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 an 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 *unreal* 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 (P_j/wage) vector itself – or, we might add, the (P_j/[½W + ½ 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_{gold}/W), or for that matter (P_j/W, P_j/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_{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 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 $a(1 + r_{\max})\overline{Q} = \overline{Q}$. *Ergo*, $W(1 + r)/\Sigma_1^n P_j \overline{Q}_j = \alpha[1 - (r/r_{\max})]$.)[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?

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 non 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 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.

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.

$W(1 + r)/P_{\text{sugar}}$ does fall *linearly* as the profit rate goes from zero to its maximum of $1+ r^* = 1/a_{\text{sugar,sugar}}$. 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 $[W(1 + r)/P_{\text{sugar}}, W(1 + r)/P_{\text{salt}}]$ 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 $\Sigma_1^s k_s$ can be a large or small fraction of unity. Moreover, Sraffa's linear $[W(1 + r)/P_{\text{standard}}]$ could well have little resemblance to the behaviour of $[W(1 + r)/\Sigma_1^n P_j k_j]$ or $[W(1 + r)/\Sigma_1^n P_j C_j]$ 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 $W(1 + r)/\Sigma_1^n P_j C_j$ 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 percent 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, 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 percent 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 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 $0 \leq r \leq r_{\text{switch}}$, 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 competitive 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 scepticism 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]

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 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 Sraffian's 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, $(\overline{Q}_i)$, 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

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

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 (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

$$Q_1 : 100 = \text{Min}[Q_{11} \cdot 200, Q_{21} \cdot 12], \; Q_2 : 20 = \text{Min}[Q_{12} \cdot 120, Q_{22}]. \tag{1.3}$$

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

$$Q_{i1} + \cdots - Q_{in} + 0 = 0 \quad i = 1, \cdots, n \tag{1.4}$$

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

$$\begin{bmatrix} a_{11} \cdots a_{1n} \\ \vdots \quad \vdots \\ a_{n1} \cdots a_{nn} \end{bmatrix} \begin{bmatrix} \overline{Q}_1 \\ \vdots \\ \overline{Q}_n \end{bmatrix} = (1) \begin{bmatrix} \overline{Q}_1 \\ \vdots \\ \overline{Q}_n \end{bmatrix}, \; aQ = Q \tag{1.5a}$$

$$[I - a]\overline{Q} = \begin{bmatrix} 1 - a_{11} & \cdots & -a_{1n} \\ \vdots & & \vdots \\ -a_{n1} & \cdots & 1 - a_{nn} \end{bmatrix} \begin{bmatrix} Q_1 \\ \vdots \\ Q_n \end{bmatrix} = 0 \tag{1.5b}$$

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

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

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 over-strong 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_{ji} or a_{ji} 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.

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

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

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

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

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 the 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

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

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

$$0 < \overline{Q} = a\overline{Q}, \quad 0 < \overline{P} = \overline{P}a \tag{1.7b}$$

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

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

$$\text{Every principal minor of } [I-a] \text{ to be non-negative} \tag{1.8b}$$

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.

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

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

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

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

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

When Sraffa's a is stipulated to be indecomposable, $\overline{P}/\overline{P}_1$ 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 $\overline{Q}$ vector of uniform maximal growth to concentrate on the new $\overline{P}$ vector of better-than-subsistence a.

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

$$aQ = (1+r)\ Q > 0, \quad Q : Q, \text{ unique} \tag{1.10b}$$

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, $m < n$; 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* (1.11c)

If $[I - a''']$ has a real characteristic $\overline{r}'''$ < than the $\overline{r}'$ of a', straightforward complications arise (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 ($a_{\text{land},1}$, $a_{\text{land},2}$, …).

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 independence 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.)

Notes

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.

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 $1 + r^*$:

$$c[1+r^*] = Q^* - \sum L^* = F[L_1^*, L_2^*, L_3^*] - \sum L^*$$

where

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

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.

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 arbitrate. See, for example, a modern treatment of the Ricardian economy in Samuelson (1959, Appendix, particularly pp. 28–35).

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 algorithms of tâtonnement is to bring it about (i.e. to convert transitory Equations (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 $[a_{ij}] = [Q_{ij}/\Sigma_i Q_{ij}] > 0$ 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 *divisability* 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.

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.

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 $\overline{Q}$. 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 $\overline{Q}$ at t will *produce* $t + 1$ (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:

$$[(1 + \varepsilon)\overline{Q} - \overline{Q}] \cdot \varepsilon = r^*\overline{Q} \text{ for } 0 < \varepsilon \text{ and as } \varepsilon \to 0 \tag{6.1}$$

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

where

$$\overline{Q}K^{t+1} : \overline{Q}K^t = 1 - r^* \tag{6.3}$$

For a Clark–Ramsey–Solow neoclassical story, let

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

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

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

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

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

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

If exogenously supplied labour is a needed primary factor along with the $(\overline{Q}_{ij})$ 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 $\overline{Q}$ 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, $\overline{P}a = \overline{P}/(1 + r^*) > 0$.

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

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

$$= \text{profit share} + 0 \text{ wage share, when } W^t \cdot P_L \equiv 0 \tag{6.6}$$

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.

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 percent labour theory of value. See Coleman (1990) for argumentation that 93 percent could well be 3 percent.) 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.

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'.)

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.

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*?'.

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).

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3 Professor Samuelson on Sraffa and the Classical economists

Pierangelo Garegnani

> But I fear that when the economic theorist turns to the general problem of wage determination and labour economics, his voice becomes muted and his speech halting. If he is honest with himself, he must confess to a tremendous amount of uncertainty and self doubt concerning even the most basic and elementary parts of the subject.
>
> (Samuelson 1956: 312)

> In principle [in mainstream economics today], we ask about allocation among individuals or among owners of different factors of production, but it must be recognised that distributional questions are not asked very loudly or answer very well.
>
> (Arrow 1991: 74)

Introduction

1. Professor Samuelson's essay on Sraffa in Kurz (2000) offers the possibility to bring to some maturity a discussion that has had several phases—one of which, conducted originally in Bharadwaj and Schefold (1990), is reprinted in the same volume. Samuelson will not be surprised if this author summarizes his disagreement with him into a single central point. The point is the existence, and the existence in Sraffa (1960), of a classical approach to economic theory, founded on the notion of social surplus, alternative to that which after a half century of transition crystallized and came to dominance during the last quarter of the nineteenth century—based on the idea of a substitutability between 'factors of production' and on the resulting demand and supply functions for both 'factors' and products. The controversies of the past decades have, I believe, brought to light a considerable amount of material about the features of that alternative approach, 'submerged and forgotten' since the advent of the marginal method (Sraffa 1960: v). If the discussion is to move forward, that material, whatever its contrast with present modes of thought, should be taken into consideration.

Indeed, an obstacle seems to exist for Samuelson in conceiving the very possibility of explaining distribution and the relative prices of products in terms other than demand and supply of factors of production. An interesting example of that, to which we shall have to return later, is provided in his (Samuelson

1990b): the consideration by Ricardo of a possible rise in the real wage because of the Napoleonic wars' need for standing armies, is described as Ricardo 'perceiving [how] a change in tastes for labour intensive goods […] raises the real wage relative to real land rents' (ibid.: 320). But envisaging a rise in real wages because of standing armies, and perceiving it in Samuelson's neoclassical terms above are not at all the same thing. The neoclassical terms would imply, for example, full employment of labour, which is just what is unambiguously contradicted by Ricardo in the very passage to which Samuelson appears to refer.[1]

This obstacle which Samuelson meets in conceiving of an alternative approach to distribution and relative prices may explain the contradictions in his attitude to Sraffa's work. On the one hand, the 1960 book is that which Samuelson often describes as 'a classic' (e.g. Samuelson 2000: 111) to which, he tells us, he has devoted 'a third of a century of exploration and reflection' (ibid.: 116). On the other, except for the phenomena of 'reswitching' and 'reverse capital deepening', he seems to find in that book mainly irrelevancies, such as the Standard commodity, or missing points such as the assumption of constant returns to scale. Is it not perhaps that Samuelson senses in the book something new, which, however, when he attempts to translate it into the terms he is familiar with, appears to be deficient, or even incomprehensible? And is that not the likely symptom of just what Samuelson wishes to deny: the presence in Sraffa (1960) of an alternative approach, or theoretical 'paradigm' (in the sense of Kuhn 1970)? Why otherwise reflect on that slender book for over 'a third of a century'?

However that may be, this paper will centre on the existence of such an alternative Classical paradigm. And, since my non-neoclassical language may have contributed to my being insufficiently clear in the discussion ensuing Samuelson's (1990a) article, this time I shall enlist Professor Arrow at the beginning of my enterprise. In a paper published in the meantime, Arrow (1991) appears, in fact, to have looked at the possibility of a theory of prices essentially different from the neoclassical one—a theory by which, as he puts it, 'Ricardo has made a bold attempt to determine values independently of demand considerations' (op. cit.: 75).

2. Thus, sections II and III of the paper will examine two key differences from modern theory to which Arrow points in Ricardo: (i) an 'exogenous wage'; and (ii) the co-existence of a positive wage and labour unemployment in what is taken to be a competitive labour market. That examination in sections I and II will entail an outline of the classical approach sufficient to proceed to the two main specific criticisms of Sraffa (1960), which Samuelson raises in (2000) and other works of his. They are: a) the supposed need of constant returns for Sraffa's price determination; and b) the alleged irrelevance of the Standard commodity. That will be discussed in sections III and IV respectively. Section V will then concentrate on Samuelson's denial of the existence of an alternative 'classical' approach, as argued by him in several of his publications and in particular in his 'Canonical' interpretation of the classical authors (Samuelson 1978). Argument and interpretation will be found to be in contrast with central features of

Smith's and Ricardo's works and in particular with their admission of permanent labour unemployment—a feature with which, significantly enough, Samuelson has been grappling over some decades in a series of articles dealing with the *locus classicus* for the question, the chapter 'On Machinery' in Ricardo's *Principles*. In section VI, finally, the attribution to Sraffa of a central concern for what Samuelson sees as steady or stationary states, but are in fact the traditional normal positions of the economy,[2] will lead us to the deficiencies of neoclassical theory—an issue inevitably underlying the debate on the alternative Classical paradigm. Those deficiencies are, we shall contend, what forced neoclassical theory to abandon those methods, characteristic of economics that far, based on such normal positions. We shall accordingly argue that, on the available and emerging evidence, the 'doom', which Samuelson foresees in (2000) for Sraffa's critique of the theory might rather fit the theory itself.

3. The debate to which the present essay wishes to contribute refers to the more abstract part of economics. It also focuses on the contrast between two paradigms of economic theory—the first of which has long been 'submerged and forgotten',[3] and remains unfamiliar to the profession at large, whilst the second is still for the majority of the profession a synonym for economic theory *tout court*. This makes the debate all but easy to follow. Painstaking independent reconstruction, rather than rapid conclusions suggested by received ideas and authorities, is therefore required from readers. An example of the difficulties of this debate is considered in appendix A to the present paper.

I Ricardo's 'exogenous wage' and the determination of prices and outputs

4. Let us then start by considering the first difference from contemporary theory, which Arrow sees as underlying the no-demand approach of Ricardo, namely, the 'exogenous wage', determined by the level of workers' subsistence (Arrow 1991: 75). The essential point here is that, unlike what happens in, e.g. Samuelson's (1978) *Canonical Classical Model* and similar interpretations of Ricardo, Arrow does not suppose the principle of population underlying the subsistence wage to act in Ricardo through sequences of demand-and-supply equilibria in the labour market.[4] As we shall see in section II below, Arrow envisages as a second feature of Ricardo's no-demand approach[5] the admission of permanent labour unemployment).

The classical 'exogenous' wage emerges then, we shall presently argue, as an *alternative* to the wage of modern theory, rather than as the demographic specification of it that we find in the mentioned interpretations by Samuelson and other authors. It becomes, that is, the basis for a different approach also to the determination of both relative prices and outputs.

Taking prices first, a given wage and given technical conditions of production suffice, essentially, for determining relative normal prices,[6] as the neoclassical mainstream had to rediscover not many decades ago by means of a 'non substitution theorem' (Samuelson 1961). Accordingly, as Arrow sees, the 'exogenous

wage' allows Ricardo to go on to obtain relative prices and the corresponding uniform competitive rate of profits (interest) independently of any demand functions for the commodities (and, ultimately, independently also of his own simplification, the labour theory of value.)[7]

The outline of a no-demand approach begins then to take shape. The above determination of prices, on the basis of a wage that is *not* the result of an equilibrium between demand and supply functions of labour along neoclassical lines,[8] entails a specific logical structure of the theory. The real wage plays there the role of what we may describe as an 'intermediate datum' of the theory.[9] It is a datum in a purely analytical 'core' of the theory where, given the wage, the relations that free competition enforces with regard to commodity prices and remaining distributive variables allow determining the latter in this way we saw. It is however only an *intermediate* datum because the explanation of the wage obviously constitutes a central object of the theory, although one to be dealt with at a stage of analysis logically *separate* from the determination of prices and the profit rate, or, more generally the non-wage distributive variables.

5. This role of 'intermediate datum' implicit in Arrow's interpretation of Ricardo's real wage is, I believe, the key for understanding classical analysis and its differences from later theory. The same role as *data* in a 'core' consisting of the competitive price relations—but at the same time as objects of investigation in the theory as a whole—is played there by two other sets of circumstances. The first are the technical conditions of production, which, unlike in most later theory, those authors took as an important object of analysis (think of Smith's considerations on the division of labour). The second set of circumstances is more surprising for modern economists: it consists of the level of outputs. The separate determination of the 'exogenous' wage, which allows for determining commodity prices without introducing demand functions, naturally leads, we shall presently argue, to a determination of outputs also independent of any such functions and, accordingly, separate from that of prices, where outputs can then appear as 'intermediate data', in so far as the technical conditions of production depend on them.

Before coming to that, it is, however, important to note how the treatment of wages and outputs as 'intermediate data' is in fact the same as what brings historians of economic thought to view the determination of the non-wage distributive shares by Ricardo and Smith in terms of a residual or 'surplus' of the product over the part of it that must be put back into the production process in order to allow for its repetition. It is a part including subsistence-based wages along the lines systematically traced earlier on by Quesnay and the Physiocrats. Now, in order to determine the non-wage shares as a residual or surplus, the product and the wages must be taken as in some sense given together with the technical conditions of production in the course of that determination (involving also, as we saw, that of relative prices).

But a question comes then spontaneously: why take as given some magnitudes that the theory has also to determine, and are therefore ultimately in the nature of unknowns? We shall see in section V below how this method of 'intermediate

data' has its basis in the distinction, implicit in the application of the notion of surplus to a market economy, between two fields of inquiry and the corresponding different methods of analysis. On the one hand, we have the necessary quantitative relations, which competition entails between commodity prices and distributive variables and, which, in their comparative simplicity, are of a nature allowing for a mainly deductive treatment. On the other, we have the circumstances determining what we have described as the 'intermediate data': the subsistence or, more generally, the wage, the outputs, the technical conditions of production. These circumstances were seen to be closely related to institutional and historical factors, which, because of their complexity and variability according to circumstances, prevented deducing the corresponding variables from a few basic principles as was possible for prices and profits in the 'core'. Those intermediate data rather required, for their study, methods of a more inductive kind. This distinction, concerning both contents and methods, which underlies the notion of surplus, appears to be what has entailed the separation between the two fields of analysis and the corresponding logical construct of the 'intermediate data'.

6. We can return now to the classical representation of the real wage as an 'intermediate datum' when determining prices, and to the connection between it and a similar treatment of the outputs. We may more easily understand that connection if, for a moment, we turn again to the non-substitution theorem, whose central point lies precisely in determining relative prices separately from outputs and therefore, essentially, on the basis of given outputs.

The theorem, it is true, is usually stated under the assumption of Marshallian 'constant returns': i.e. horizontal supply schedules.[10] However that assumption is irrelevant for the theorem *as such*, since no changes in outputs are involved in its demonstration, based as the latter exclusively is on the competitive equality between normal prices and expenses of production (the relations of our 'core'). The question of returns to scale only becomes relevant when the theorem is set in a wider theoretical context, where the separation of the determination of prices from that of outputs *may*, but also *may not*, involve the assumption of constant returns.

Let me explain. When the theorem is set in a neoclassical context, any change in prices is supposed to be accompanied by the *predefined* changes of outputs implicit in the demand functions appearing in the equations. The separate price determination of the theorem can then survive only when the accompanying output changes leave the supply prices unchanged, i.e. under Marshallian constant returns.

The situation changes, however, when the theorem is set in the classical context of Smith and Ricardo, characterized by their 'exogenous wage'. No general *predefinable* dependence of outputs on prices needs be present there and be included in the equations determining prices. Outputs are therefore naturally determined *separately* from prices, i.e. can be taken as given in determining the latter.

Let us in fact suppose for a moment the presence also in those classical authors of neoclassical-like demand functions for the products, and consider the

two elements that could cause Marshallian returns to be variable and accordingly make those functions be relevant there also. The first such element is changes in factor prices functionally linked to changes in relative outputs. The exogenous wage eliminates that element with regard to the division between wages and non-wage shares of the product and, to that extent, allows for a determination of prices separate from outputs and independent of the demand functions we have assumed.

The second element is non-constant physical returns to proportional changes of labour and capital: i.e. either *decreasing* physical returns to scale because of the scarcity of land (affecting in Ricardo the division of the product between rent on the one hand and profits plus wages on the other) or *increasing* physical returns because of an increase in the division of labour. However, Ricardo treated decreasing returns from land, just as Smith had treated the increasing returns from division of labour: as relevant, that is, only for the comparatively large output changes involved in capital accumulation and growth.[11] Unlike what happens in neoclassical theory, Smith and Ricardo could therefore leave physical returns to scale quite naturally aside when dealing with relative prices in a given position of the economy, with the kind of comparatively small output changes generally involved in that specific analysis.[12] Physical returns to scale raise then no more obstacle than changes in the wage to a classical determination of prices separate from that of outputs and independent of the demand function assumed here.

It is not surprising then, that the notion of demand functions, i.e. of predefined relations between prices and outputs, should have remained foreign to Smith and Ricardo, where they would not have had any sufficiently general and simple determining role on prices—and where, we may now note, the absence of the neoclassical equality between demand and supply of productive factors would in any case have deprived those functions of their clear basis in the simultaneously determined incomes of the individuals.

The alternative to such demand functions was accordingly what we may rigorously represent today as taking outputs as given,[13] i.e. as 'intermediate data' in determining prices. It is because of this analytical structure, we may conclude, that the determination of prices separate from outputs, which we find in the non-substitution theorem, needs no constant returns assumption in order to be confirmed in a classical context. The classical exogenous wage voids the level of outputs and therefore our hypothetical demand functions of their neoclassical predefined relevance for the determination of prices. It does so *directly* with regard to any predefinable effects on the real wage. It does so *indirectly* in that the above wage-based autonomy of prices from outputs, makes it natural to locate the analysis of physical returns in the analysis of capital accumulation and growth and, more generally, in a determination of outputs separate from that of prices.

7. The above has implicitly answered the question that comes naturally to a modern economist faced with outputs treated as data when determining prices: what, then, of the *interactions* between normal prices and normal outputs? The

answer lies essentially in the all-important distinction between determining prices and outputs *separately* from each other and determining them *independently* of each other. The objection of the modern economist concerns the latter, but what we find in Smith and Ricardo is only the former.

As we saw, the classical separate determination of prices and outputs does not exclude at all the dependence of normal prices on normal outputs because of variable physical returns: so much so that rigour asks us to speak of outputs being data when determining prices. Even less does it exclude the reverse dependence of normal outputs on prices, so much so that, as we shall presently see, Smith and Ricardo did refer their normal outputs to an 'effectual demand' of the commodity reckoned for a *given* 'natural' (normal) price. What happens is only that those dependencies and corresponding interdependencies are left to be considered, when needed, within the determination of each of the two sets of variables. What leads to a separation between determination of prices and determination of outputs is in fact not the absence of mutual dependencies: it is the *nature* of those dependencies; it is, that is, the complexity and variability of the circumstances affecting the outputs of the commodities and, therefore, their reactions to the changes in prices.

Unlike what happens with the neoclassical belief in the possibility of summarizing those circumstances under the concept of given consumer tastes and the resulting decreasing marginal utilities, Smith and Ricardo did not see the factors affecting outputs as susceptible of being re-conducted to general principles simple enough to lend themselves to a formal treatment like that possible for prices, once the real wage and the outputs are given. Little scope was then left for any however primitive formal simultaneity between the determination of outputs and that of prices. (This of course relates to the theory in its full generality and would not prevent specific circumstances—e.g. individual industries where resources are highly specialized, or specific problems, e.g. of taxation—making ad hoc formal treatments of price-output interdependences feasible and useful also in a classical context.)

We are finding here the same theoretical situation we saw earlier in its general terms, as imposing a more inductive and even historical method of analysis in studying the circumstances affecting the other intermediate data, that is, wages and technical conditions of production, and therefore also their interactions with prices and among themselves.

8. The inexistence of demand functions for commodities has in fact been noticed by the more attentive interpreters of Ricardo and the other old classical economists[14]—starting with Marshall and his attribution of constant returns to Ricardo—but it appears to have been viewed as a sign of primitivism, rather than as the expression of an alternative theoretical approach like the one intuited by Arrow in Ricardo.

This qualification of primitivism might at first seem to draw some support from Ricardo and Smith's frequent use of the phrase 'supply and demand', or similar expressions, thus apparently justifying the reference forward to a more developed modern conception of them as functions of prices. A closer examination of the

classical phrase reveals, however, a meaning that is quite different from the modern one and fits well instead with the alternative theoretical approach we are outlining.

The key notion there was Smith's 'effectual demand'—an element in the analysis of what we would today call the stability of the normal price and not its determination. The 'effectual demand' is in fact described as: 'the demand of those who are willing to pay the natural price' (Smith 1776, I: 49)'; and Smith proceeds then to argue that, should 'the quantity brought to market' exceed it, then the 'market price' would be below the 'natural price', causing the output ultimately to fall, and vice versa should supply fall short of the effectual demand.

Two basic differences from the modern notion of demand stand out. The first is that, in Smith, the effective 'demand' is a single quantity and not a function, the same being true for the supply, i.e. the 'quantity brought to market' (ibid.). This explains, for example, the word 'proportion', which Adam Smith (e.g. 1776, I: 49) and Ricardo (e.g. 1951–73, I: 382) frequently applied to the relationship between their 'supply and demand' and its effect on the market price of the commodity—a word clearly making no sense, had demand and supply been understood however vaguely as schedules.[15]

The second difference is that the natural price—corresponding to an equilibrium price in modern terms—far from being an unknown to be determined by those 'supply and demand' as in neoclassical theory—is there a *given* for the very definition of the demand, the single quantity. This fits with the possibility we saw above of determining the normal price on the basis of the given real wage and the technical conditions, independently of any demand functions. It is also perfectly in keeping with the limited role of the 'effectual demand' of providing the basis for an analysis of the convergence of the 'market price' to the 'natural' or normal price, and not for a determination of the latter.[16]

Nothing primitive, therefore, in this notion of demand. What emerges is something rather difficult to envisage on the part of a modern economist, namely that the neoclassical demand and supply *functions* for commodities are in fact only a particular way of dealing with prices, outputs and their interdependencies—the way that is related to an equally particular attempt to explain distribution in terms of a substitutability between 'productive factors'. Different ways of dealing with such variables and their interdependencies are possible and natural, when distribution is differently dealt with—and this, we contend, is just what we find in Smith and Ricardo.

We may now incidentally realize better how Marshall's above-mentioned influential resort to an implicit assumption of constant returns in Ricardo, in order to explain the absence of anything resembling demand functions in that author,[17] has to be seen essentially as a reflection of Marshall's own theoretical presuppositions. The arbitrariness of that attribution is made clear, among other things, by the fact that where constant returns could not possibly be ascribed to Ricardo, as in the case of agricultural products, Marshall has to resort to a second explanation quite different from the first, if not contradictory with it namely, an

absolutely rigid 'demand' of corn; indeed, the way in which a neoclassical theorist can attempt to represent the classical condition of given outputs.

II Arrow on Ricardo: the 'clearing' of the labour market

9. In section I, we have seen the implications of the 'exogenous wage' for the structure of classical analysis. There, we saw the qualifying importance of a second element, which Arrow singles out for characterizing the approach of Smith and Ricardo: the inexistence, that is, of a tendency to equalize labour employment to labour supply, once the level reached by capital accumulation is given. We must now consider more closely the specific implications of this second element for the nature of classical wage theory.

The results of this element are indeed striking for modern economists: a positive 'normal' wage[18] is found to coexist with permanent labour unemployment under the conditions of free competition, which Smith and Ricardo clearly assume for the labour market. The following question raised by Arrow with reference to the labour market is then inevitable: 'If prices do not have the property that all markets clear, then there must be an hypothesis that the price on a non-clearing market may, for some reason, remain unaffected' (Arrow 1991: 73).

As I have argued elsewhere (Garegnani 2002: 248), there is clear evidence for the kind of answer Ricardo and Smith were implicitly giving to the question. The factor from which we must start in order to understand that answer is the inexistence in the work of those authors of the idea of a wage-elastic demand function for labour; the idea, that is, of forces ensuring that a fall of wages will result in some regular increase of the labour employment, which can be provided with the capital endowment existing in the economy in the situation. Surprising as it may perhaps appear today, that idea only emerges in the years after the death of Ricardo, in connection with wage-fund theories which, though soon discarded, opened the way to the later neoclassical labour demand and supply functions.

Now, without an elastic labour demand function the modern conception of free competition in the labour market, entailing an indefinite flexibility of the real wage in the presence of labour unemployment does not make sense. It would lead to the absurd conclusion that, in positions of the economy one would then have to admit as possible, or even normal, the wage could tend to zero or, in any case, to levels intolerable for the workers, in contrast with experience and indeed with the possibility of survival of society itself.

In that theoretical situation it was therefore only natural that Smith and Ricardo should have taken for granted the effectiveness of the customs and institutions that are observable in the labour markets and are clearly meant to prevent and repress self-interested individual behaviour on the side of both workers and employers, which might lead to an indefinite lowering of the wage.[19] The general point here is the one made by Pigou when reporting a striking passage by Edwin Cannan to the effect that: 'the working of self-interest is generally beneficent, not because of some natural coincidence between the self-interest of each and

the good of all, but because human institutions are arranged so as to *compel* self-interest to work in directions in which it will be beneficent' (Cannan 1929: 333, in Pigou 1932: 128, our italics) and human institutions could hardly allow individual self-interest to destroy the material basis on which a community's existence rests, by endangering the subsistence of workers.

10. Thus, in the first place, we would expect Smith and Ricardo to view the labour market as one where self-interest could act for the uniformity of the wage of labour of any given quality (see below), but where, with regard to the general level of the wages, institutional elements would constrain the wage bargains struck in any given position of the economy within limits set by the previous history of the wage and, in particular, by the notion of subsistence accepted in the community.[20] We shall consider more closely in section V the factors setting that normal wage in Smith and Ricardo: what is relevant here is only to note that labour unemployment would then be seen to play a role as one of the factors, perhaps the main factor, affecting the current normal wage relative to its level in the immediate past, but not to cause an indefinite lowering of the wage down to any supposed market clearing or to zero.

11. Thus, in the second place, the same inexistence of the idea of a wage-elastic labour demand function must have made it natural for Smith and Ricardo to see no conflict between free competition in the labour market as conceived by them and the institutions and customs affecting the wage bargains actually struck. They would see no more conflict between the two than we generally see today between free competition and the several customs and regulations that variously ensure the orderly working of each market by, e.g. preventing and repressing a lowering of expenses of production and product prices by means that would endanger the safety of consumers or of workers. Just as today, we generally take such regulation as a natural part of the institutional framework within which alone can competition be conceived to be free, so the classical economists would take as an essential part of that framework the customs and laws preventing any indefinite downward flexibility of the real wage. The fact that the latter institutions impinge *directly* on the determination of a price (the wage) while the former generally don't, ceases to be relevant when the inexistence of an elastic labour demand makes it clear that both are equally imposed by an orderly survival of the community.

As we have said, the customs and laws constraining the wage bargain are no less directly observable than those whose consistency with free competition we generally take for granted today. The difficulty, in that respect, fell rather on the shoulders of the post-classical theorists, who will be led by the idea of the elastic labour demand function to defining free competition in terms of an indefinite flexibility of the wage. The contrast between this and the results of observation had then to be explained away, whether by viewing the influence of institutions and customs as a mere reflection of the long-run demand-and-supply forces of the theory,[21] or as frictions slowing down the effects of such forces, or, also, as an expression of monopolistic elements.

We shall see below (section Vc) how the view of wage bargains outlined here may in fact help to solve what the more attentive modern interpreters have seen

as the many insoluble puzzles raised by Smith's and Ricardo's texts on wages. We shall return to the role of free competition in the labour market as it emerges from those texts.

III Constant returns

12. We now have a first outline of the approach of the classical economists and Sraffa, sufficient for commenting in this and the next section on the two issues around which, we said, Samuelson focuses his criticism of Sraffa (1960): the need for constant returns for the validity of the (1960) price equations, and the claimed irrelevance of the Standard commodity.

With regard to constant returns, we have already argued in section I against the idea that Sraffa and the classical economists need to assume them: the consideration of outputs as 'intermediate data' when determining profits and prices makes any assumption about returns irrelevant at that stage of the theory. What we must now see is how that general contention of ours fares in the face of Samuelson's specific arguments. These seem to fall under three main headings.

The first is the claim that in developing the argument of his book (1960), Sraffa himself in fact assumes constant returns to scale. So far as I can see, Samuelson's argument here consists of interpreting as changes in *actual* outputs what in Sraffa (1960) are simply the application of *abstract multipliers* to the price equations, in order to analyse relations that continue to pertain to the initial economy with its given outputs. One instance will suffice here. In his demonstration that only in an economy without surplus can prices provide for just the replacement of the means of production (inclusive of workers 'subsistence'), Sraffa takes the equations in the size they would have if the economy were in a 'self-replacing state'. He then notices that: 'every system of the type under consideration is capable of being brought to such a state merely by changing the proportions in which the individual *equations* enter it' (1960: 5, our italics).

At this Samuelson objects: 'Only in constant returns to scale technologies do proportions matter and alone matter!' (2000: 121). That of course is true, but it applies to proportions between *actual outputs* and not to proportions between *equations*, as Sraffa is careful to specify in the one word we italicized in his passage.

I am less clear about Samuelson's second line of argument for Sraffa's need to assume constant returns. It relates to a consistency with the uniform profit rate of his price equations and, thus, it seems, with the assumption of free competition underlying it (e.g. ibid.: 117, 123). However, even in the received treatment of competition are not *in*creasing returns to scale to the industry generally admitted to be consistent with free competition, provided they are due to economies *external* to the firm? And is that not all the more so with *de*creasing returns for the industry whether due to 'external diseconomies', or to the need to economize on resources such as land, whose relative scarcity and, therefore, relative remuneration, changes in response to changes in relative outputs?[22]

13. We may then proceed to the third and more interesting of Samuelson's three lines of argument: the one concerning the application of Sraffa's equations

to the actual economy. Samuelson writes: 'Does [the non specification of constant returns] matter? Of course it does, as soon as the author hazards assumptions about how the prices of Standard or of other market baskets of goods will vary with the profit rate' (ibid.: 119).[23]

Samuelson is clearly right: it does matter, because outputs will generally change with the rate of profits, and therefore with any change of the 'intermediate data' on which it depends. But, as we have argued in section I, the treatment of the wage as an 'intermediate datum', and the separation this allows between the determination of prices and that of distribution and outputs, makes any assumption about returns to scale irrelevant for the relationships we find in what we called the 'core' of the theory. The question of returns only arises in the separate determination of outputs and in accordance with the circumstances of the case.

Samuelson's passage above may, however, provide the occasion to pin down a misleading interpretation of Sraffa (1960) as, so to speak, a 'mutilated' general equilibrium system. This interpretation is what appears to often underlie the claim as to the necessity of constant returns for his price determination.

Given in fact a change in any of its data, the equations analogous to those we find in Sraffa (1960) and that are part of a neoclassical general equilibrium system, based on normal positions of the economy (think e.g. of Wicksell 1934), will in principle give the new prices *for the economy* and not, simply, for a stage of the reasoning towards that final result. All effects of the change in question, including those of outputs, are supposed to have been taken into account in writing the general-equilibrium equations. Suppose now we somehow expected from Sraffa's price equations the same kind of general equilibrium results, *directly* applicable to the economy, at least in principle. True, the data for factor endowments and consumer tastes are missing in Sraffa, but if we somehow granted (i) a *given real wage* (*or rate of profits*) and (ii) *constant physical returns to scale and free land* (i.e. Marshallian constant returns), then those equations would allow us to determine distribution in the economy without introducing endowments, and, above all, without introducing consumer demand. Sraffa's price equations would then appear as a neoclassical long period general equilibrium system, 'mutilated' of its parts regarding endowments and tastes and therefore limited and imperfect, but—and here is the point—still producing results *directly* applicable to an *economy* for which the given wage and above all the constant returns could somehow be postulated.

This interpretation entailing constant returns undoubtedly makes Sraffa's *Production of Commodities* more easily comprehensible to neoclassical theorists, by representing it as a kind of 'special case' of their theory, but I believe it would be a radical misconception of what the book is—and, also, of what the classical economists were doing. That supposed *direct* applicability is a peculiarity of the solutions of a neoclassical general equilibrium system where, as we said, we implicitly suppose to have 'boxed' into the equations, once and for all, every relevant relation among the variables of the given economy. It most certainly is *not* what Sraffa expected and we should expect from his equations,

which do not describe a 'mutilated' general equilibrium system any more than they describe an unmutilated one.

The fact that the wage, the outputs and the technical conditions of production are *intermediate data*, entails that the prices and the profit and rent rates obtained by solving the equations are *intermediate results*, not yet results directly applicable *to the economy*. To arrive at these, what caused the changes in the 'intermediate data', the interactions between the latter and the possible feedbacks on them from prices and the residual distributive variables will, at least in principle, also have to be considered by means of appropriate, more inductive methods of analysis. Thus, in order to conclude about the effects in the economy of, for example, a wage rise, a Sraffa, or an Adam Smith, or a Ricardo, would want to know not only its size but also its possible effects on the other parameters of the price equations. They would also want to know its causes, whether, for example, it is question of determining changes in normal prices, relating, that is, to a usual long-period just sufficient for productive equipment to adjust to 'effectual demands', or instead it is question of a longer run, lasting, say, more than one trade cycle, etc. More generally, those authors would hold that the interactions with the other intermediate data (outputs and technical conditions of production) of that wage rise, or the feed-backs of prices and profits on those data, are likely to be different according to the circumstances of the case in hand. Also, unlike in neoclassical theory, it is in the course of that further analysis, and not in the equations determining prices that the question of returns to scale can arise.

This, incidentally, makes clear why it is not the case that Sraffa's *Production of Commodities* 'evaporates' into a 'half page of vapid chit chat' (Samuelson 2000: 123) unless constant returns to scale are assumed for his equations. This passage is indeed as clear an example as we may wish of Samuelson's presupposition that no theoretical paradigm can exist in alternative to neoclassical demand and supply functions requiring constant returns for determining prices separately from outputs.

IV Standard commodity and all that

a Ricardo's 'basic theorem'

14. So far we have discussed classical analysis on the basis of only one of the two methods we find in Sraffa (1960) for determining the rate of profits (or alternatively the surplus wage, ibid.: par. 44) the price equations, that is, as distinct from the Standard product. This has made comparison with neoclassical theory easier, but it is not the most direct and natural way to arrive at classical distribution: it was not, essentially, the way in which Quesnay, Smith, Ricardo or Marx arrived at it and reached many of their most important propositions. The exact mathematical solution of a system of n simultaneous price equations was in any case not a way open to those authors at their time.

As stated earlier, in connection with the method of 'intermediate data' (par. 5), those authors viewed the non-wage distributive variables—or more exactly

what they saw as the basic variables (rent for the Physiocrats, rent and profits for Smith, profits for Ricardo)—as the residual of the product over the subsistence-based wages. We may express that procedure in terms of a 'surplus equation', where the residual non-wage distributive variable appears as the unknown of the equation.[24] It is to this alternative representation, characteristic of the classical paradigm that Sraffa's Standard system pertains. To this representation we must accordingly turn in order to consider Samuelson's second group of specific criticisms of Sraffa, centring on the alleged irrelevance of the Standard commodity.

A convenient way to approach the subject is to focus attention on what, after Sraffa (1951), historians of economic thought have referred to as Ricardo's 'fundamental' or 'basic' theorem on distribution, or also as his 'corn model'. It is essentially the proposition that the rate of profits is fully determined, once the real wage rate is known, together with the outputs and the technical conditions of production—and, further, that under sufficiently general conditions the two rates vary inversely with each other.[25] Now, as I recalled in my 1990 comment (p. 293), Ricardo viewed his analysis of the question as a correction of what he described as 'Smith's original error regarding value' (Ricardo 1951–73, VII: 100): that for which, in Ricardo's own words, Smith thought that: 'as after stock was accumulated a part [of all the produce of labour] went to profits, that accumulation necessarily […] *raised* the prices or exchange values of commodities' (ibid.: 377, our italics), where the word 'raised' is the key word, because Smith saw prices as arrived at by a process of *adding up* wages, profits and rents (Sraffa 1951: xxxv–vii).

That this is not, in Smith, the innocent question of words, or of numeraire, it may seem to us, comes to light when we find that in Smith's view the natural price would vary 'with the natural rate of each of its component parts, of wages, profit and rent' (Smith 1776, I: 56). An important expression of that view of the natural price lay in the idea of a rise in all manufacturing prices as wages rose, e.g. because of a tax on workers' necessaries. And that price rise was what allowed for Smith's belief that the previous level of the rate of profits could conceivably be maintained in the presence of a rise in the real wage. It thus brought him to his theory of a rate of profits determined by a 'competition of capitals' in apparent logical independence of the real wage, thus contradicting the constraint for which, as we all know now, under given technical conditions, (and under Ricardo's other assumptions) a given real wage rate entails a given rate of profits, and a rise of the former a fall of the latter.[26] It is this key constraint that Ricardo was able to unearth by his 'basic' or 'fundamental' theorem. As he put it: 'whatever increases wages, necessarily lowers profits' (Ricardo 1951–73, I: 118, also 292, 289–91 and *passim*).

15. But how could Ricardo establish that correct 'theorem', when he still accepted the Smithian notion of manufactures' prices rising as money wages rose,[27] inevitably obscuring the existence of any definite relation between wages and profits?

We find here the alternative representation of classical distribution in the shape of the so-called 'corn model' and its 'surplus equation'. Given the corn output (an 'intermediate datum') and some simplifications, Malthus's theory of

rent allows Ricardo to single out a no-rent land. Assuming then the wage to consist essentially of corn, the wages required to produce a given quantity of corn are determined once the wage rate and the method of production of corn on the no rent land are given: the amount of (corn) profits are the residual in the resulting 'surplus equation'. Since Ricardo followed Smith in essentially (and erroneously) identifying capital in the community as a whole with total wage advances,[28] the rate of profits was also determined and it could be established that it would have to fall as the corn wage rose, raising its denominator and lowering its numerator.

And if Ricardo's implicit use of this surplus equation in terms of corn seems to be still controversial among historians of thought,[29] there appears to be a good degree of unanimity about Ricardo's use of the surplus equation in terms of labour values for the 'fundamental theorem' as stated in the *Principles*.[30] As Sraffa put it: 'it was now labour instead of corn that appeared on both sides of the account – in modern terms, both as input and output' (Sraffa 1951: xxxii).

In fact, for a 'surplus equation' to be consistently used to determine the profit rate, it was necessary that the product and the wages be expressed in terms of quantities that: (i) would be homogenous with one another; (ii) be *given* when the corresponding physical magnitudes were given, i.e. be independent of relative prices, but at the same time (iii) be related in an appropriate way to the values of the corresponding magnitudes, the rate of profits being a ratio between two *value* quantities. Both the corn and labour measurements satisfied those conditions (in particular condition [iii]) under the respective assumptions of a wage consisting entirely of corn, and of the equal 'organic compositions' of capital. Either assumption could not, however, be held to be sufficiently verified in reality, so that the key condition (iii) was not strictly fulfilled and Ricardo's argument could only be an approximate argument on which he continued to work. It was, however, sufficient to bring to light the constraint that binds the wage and the profit rate the one to the other.

16. Both Smith's 'original error' and the 'surplus equation' route by which Ricardo did the correction, thus establishing his theory of profits, appear to be overlooked by Samuelson and this, we contend, is what makes it difficult for him to appreciate the central meaning of Ricardo's 'invariable measure of value' or that of Sraffa's Standard commodity.

Samuelson's oversight emerges when, for example, in the (1990) discussion he argues that Smith's idea of a profit rate determined by the 'competition of capitals' is 'right on the target' (1990b) since when: 'the accumulation of capital sufficiently exceeds the pace of population growth, […] there can be a *decline in the rate of profits and a rise in the real wage*' (Samuelson 1980: 577, see also e.g. Samuelson 1987: 459).

But, in the words we italicized, Samuelson is in fact attributing to Smith the very thing that Smith failed to see because of his 'original error' and that Ricardo had to contend against him; namely, that, in any given situation of the economy, a change in the rate of profits logically entails, under the conditions of the case, a definite change in the real wage.[31]

17. The source of this difficulty in seeing the 'error' of Smith emerges when Samuelson writes: 'I indict Ricardo (and Sraffa) for not explicitly following Smith in formulating a *tripartite* model of relative prices, real prices and distributive shares' (2000: 133) or, as he had more fully explained in the (1990) discussion: '[the labour theory of value] is simply a wrong one-parameter theory of value when every schoolboy [. . .] knows that only a three parameter theory of value that gives proper scope to rent, wages and interest can properly describe [relative prices]' (1990b: 322).

Samuelson fails here to distinguish between what we could describe as the *writing* of the price equations and the *solving* of them. 'What every schoolboy knows' is that in the writing of the equations all three resource prices must appear in their appropriate form. But that—on which there has never been any dispute among economists, certainly not between Smith and Ricardo, or Smith and Marx—gives so very little help in *solving* the equations (i.e. effectively determining profit rate and relative prices) that, for some decades before Ricardo's *Essays on Profits*, or his *Principles*, and for several decades even after it, authors could follow Smith and in effect believe, as we just saw, that the rate of profits would depend on the 'competition of capitals', while wages could remain at their previous level.

It follows, in particular, that the labour theory of value of Ricardo (and Marx) was not a wrong 'one-parameter' *writing* of the price equations: it was an attempt at *solving* them for the rate of profits via a 'surplus equation' and it indeed provided, as we saw, a basis for bringing out the constraint binding profits to wages.

The nature of the labour theory of value of providing an (approximate) *solution* of the price equations, and not an *alternative* to them, can incidentally clarify my answer (Garegnani 1990: 292–5) to the challenge Samuelson had advanced in a well-known 1974 symposium: 'to show that [Marx's] 'novel analytical innovations concerning positive equalised rates of 'surplus value'' being 'other than a detour to one who would understand 19th century or earlier century distribution of income' (Samuelson 1974: 69).

My (1990) answer was that Marx's—and Ricardo's—equalized rates of surplus value (a necessary logical *implication* of both authors' labour-theory of value[32]) were an integral part of Ricardo's theoretical breakthrough. It was the breakthrough that allowed uncovering the link binding wage and profit rates, and to do that in 1817, nearly a century before the mathematician Perron[33] could provide the tools by which the problem was to be dealt with in mainstream theory after some further decades. Hardly a 'detour', then, to a historian of economic thought; particularly to one who would understand what Samuelson himself once perceptively singled out as to the 'technological predictability'[34] of labour values (i.e. their independence from changes in distribution).

b The standard commodity

18. The labour theory of value as a solution procedure via a surplus equation, rather than as an alternative system of one-parameter prices, is in fact what may

be seen to underlie Ricardo's late treatment of an invariable measure of value, to which Samuelson devotes most of his section on Sraffa's Standard commodity (Samuelson 2000: 127–36; also Samuelson 1998: 231).

The way in which Ricardo used what we describe as the 'surplus equation' for his reasoning in the *Principles* was to ascertain the change in the rate of profits following upon changes in the real wage (because of changes in the quantities of its constituents or in their methods of production). The labour measurement ensured of course the conditions (i) and (ii) listed above (par. 15) and, therefore, with [ii], the constancy of the product's size as the wage varied, but the imperfection of the measurement with regard to requirement (iii), of proportionality with prices, put Ricardo in a position of latent inconsistency. He assumed the constancy of the values of commodities 'in the production of which no additional quantity of labour is required' (Ricardo 1951–73, I: 110–1) in order to determine the change in the profit rate and then used that change in order to ascertain how those very values did *not* remain constant (cf. e.g. the headings of sections 5 and 6 in chapter I: op. cit., I: 30, 38).

The lines along which Ricardo was trying to overcome that contradiction began to emerge in the third edition of the *Principles*. He chose there as his 'invariable measure of value' a commodity that would be produced, (a) by a constant quantity of labour and (b) in conditions, as to the proportion between labour and means of production, which would be a 'medium' with regard to the economy as a whole (Ricardo 1951, I: 73). In terms of such a measure, as Sraffa notes: 'the average prices of commodities and their aggregate value would remain unaffected by a rise or fall of wages' (Sraffa 1951: xliv–xlv). Ricardo could then hope to dissociate the two stages of his previous reasoning and overcome their latent inconsistency. In the first, he would determine the change in the rate of profits by means of a surplus equation, based on an 'invariable' measure of the social product—invariant, that is, with regard to changes in the relative prices of its commodity constituents so as to fulfil, jointly with (i) and (ii), also condition (iii) above, which the labour values of individual commodities could *not* satisfy. Then, in a second stage, the profit change could be legitimately used to ascertain the changes in the relative prices of the *individual* commodities—a procedure close to the one that Marx actually followed with his theory of the 'prices of production' (and entailing the same difficulties).

The question of the classical 'invariable measure of value' is of course, historically, a complex one, into which several elements have entered, which we would today see as heterogeneous with one another, but the above two-stage procedure attempted by Ricardo should suffice to provide an answer to Samuelson's question as to: 'why that mean [Ricardo's measure of value after the third edition of the *Principles*] is golden or useful as a comparison rock for measuring Absolute or Invariable value' (Samuelson 2000: 129): a 'why' that was clearly not 'simply and gratuitously taken for granted' by Ricardo as Samuelson contends (2000: 129)—though it may not be easy to grasp that 'why' before the alternative distributive paradigm of Quesnay, Smith, or Ricardo is perceived.

19. The analytical task that Ricardo set to his 'invariable measure of value' is essentially the same that Sraffa sets to his Standard system, namely, to determine the profit rate prior to prices.

As I recalled in Garegnani (1990: 290), from a strictly mathematical point of view, the Standard system is an instance of the general procedure of a change of 'coordinates system', familiar to mathematicians and scientists, who use it to render complex systems more transparent. In the case in hand, that general procedure reduces the representation of the distribution between wages and profits from the $(k + 1)$ price equation down to a single equation. Moreover, that single equation corresponds to the mental picture of a social product, homogeneous with the capital required for its production, entailing therefore a linear relationship between the wage and the profit rates as the distribution of that product between wages and profits changes.[35] It seems hardly possible to deny that this particular change of coordinates system is a scientific achievement of some magnitude, in that it makes immediately visible a distributive process acting through thousands of intermediate prices.

Of course, like other instances of the Surplus equation, the Standard system has its intuitive origin in the analytical structure of classical theory and has particular importance there because of the classical treatment of the social product and the wage (or the profit rate) as intermediate data, in the sense we saw in section I. But the Standard system clarifies thereby relations implicit in any system of competitive normal prices and cannot therefore but be of importance also for neoclassical theory, though its role will there be limited to clarifying some steps in those longer chain, of deductive reasoning.

Thus, it is the grasp that the Standard system gives of the relationship between the distribution of the social product and the system of relative prices that, as I contended in (Garegnani 1990: 91), renders immediately evident a proposition such as the 'non-substitution theorem'. In his 1961 article on the matter Samuelson wrote: 'it is nonsensical to say that a change in wage rate, with the other factor return, interest, held constant, will lead to some kind of 'lengthening' of the period of production' (Samuelson 1961: 533). Familiarity with the Standard system makes it immediately clear that, in that two factors context, the 'constancy of the interest rate' prevents any change in the real wage and in relative prices and, therefore, the question of a change in the most profitable methods of production ('lengthening of the period of production') cannot even arise.

The above example might suffice to answer Samuelson's question 'towards what is the Standard an auxiliary?' (2000: 135). Samuelson himself however provides an additional example when he writes: 'The Standard vector to me is more importantly the von Neumann vector than the Sraffa vector' (2000: 134). Indeed, the von Neumann properties of the Standard vector are a result of the very same singularity that makes the Standard commodity important for Sraffa and for the distribution of the social product between wages and profits—namely, the homogeneity between the commodity and its means of production.[36]

c Basic products

20. Not unconnected with his strictures on the Standard commodity, but newer and more surprising, is Samuelson's criticism of the distinction between basic and non-basic products. The criticism is surprising, first of all because, by the beautifully simple criterion of whether the product enters directly or indirectly into the production of all commodities, we are able to distinguish between two kinds of commodities that have very different properties in the system. Given the real wage and the technical conditions of production, elementary phenomena such as a tax, or a change in the method of production, pertaining to a non basic will only affect its normal price and that of any connected non basics, whereas the same changes will affect the rate of profits and *all* prices in the case of a basic product.

Of course the immediate relevance of these results owes much, again, to the classical treatment of real wages, technical conditions and outputs as intermediate data, which puts the relation between method of production in use and prices at the centre of the stage, without the obfuscation caused by a simultaneous treatment of the alternative of techniques and consequent 'production functions'. But, as in the case of the Standard system, those results cannot but also be of relevance within neoclassical theory, whichever the further effects, supposed there to follow from the predefined dependencies of prices also on factor endowments and consumer preferences. Indeed the fact that these results have failed to come clearly to light in the century and a half after Ricardo had begun to bring them out seems significant of some strictly technical drawbacks of the neoclassical attempt at treating prices simultaneously with distribution and outputs.

21. Samuelson argues however that Sraffa's assumption of the existence of at least one basic product in the system is unlikely to be fulfilled. As I recalled in my 1990 Comment, (1990: 291–2), workers' necessaries are essentially means of production for Sraffa, so that basics are sure to exist in the system so long as labour enters directly or indirectly all commodities.[37]

Also, more generally, it seems inevitable to note that if we were to ignore the 'basic' role of workers necessaries, and we were prepared to go along with Samuelson's present scepticism about other sources of basics, yet an inexistence of the latter would importantly affect the properties of the system (e.g. on the existence of a maximum rate of profits) and the reference to basic products—whether present or absent in any particular economy—could hardly be avoided in a satisfactory analysis of it.

22. But Samuelson's criticism of the concept of basic product is all the more surprising when we realize that, in 1958, he would have supported and even reenforced my remarks above about the existence and importance of basic products. In the tenth chapter of the well-known *Linear Programming and Economic Analysis*, which he co-authored, we in fact find the following definition of the economy assumed there:

> 'Every industry might directly use some positive input of every other industry. Failing this […] every industry might indirectly use some positive input

> of every other industry, if not buying directly or indirectly from it, at least buying from intermediary industries which buy directly or indirectly from it—the chain of intermediary industries consisting of 1, 2,…, up to n+1 industries'.
>
> (Dorfman *et al.* 1958: 254–5)

where, therefore, in Sraffa's expressions, each product enters directly or indirectly in the production of all other products or, even more concisely, *all* products are 'basic'. And it is for an economy in which basics thus weigh 100 percent of gross national product that the authors state most of their theorems there. Indeed, they explicitly stress, even overstress (it is difficult to deny the existence of non-basics), the realism of that assumption: 'If sales could be calculated to the last dollar, it is probable that any actual economy would have the above so-called 'indecomposable' property, in which all pairs of industries are interlocked directly or indirectly in a two way fashion'.

However, the more general case considered by Sraffa of decomposable, but not separable, input matrices is also present, where it is defined as follows:

> We can concentrate on [a matrix of input coefficients] and suppose that it has no industries which cannot be split further into completely separable subsystems. It follows that every industry is, directly or indirectly, in some kind of connection with every other industry. Thus it might be the case that Industry 1 both buys from, and sells to, Industry 2; and Industry 1 might sell to Industry 3 but not buy from it; similarly, Industry 4 might buy from Industry 1 but not sell to it; Industry 5, on the other hand might neither buy nor sell from Industry 1, but might be indirectly linked to Industry 1 by virtue of the fact that it does have transactions with either Industry 2 or 3 or 4.
>
> (ibid.: 258)

The painstaking concern with sorting out the economic meaning of the mathematical condition undoubtedly sets a deserving example for today's mathematical economists: but just because of that, we can appreciate the greater clearness and precision that Sraffa's rigorous distinction between basics and non-basics can impart to the matter. Thus, authors whom Professor Samuelson views as 'amateur' mathematical economists (cf. e.g. 2000: 113) emerge with more transparent and rigorous definitions than 'professionals' do—which is, of course, what Samuelson himself honestly implies when he calls Sraffa (1960) a 'classic'.

V Two alternative paradigms of economic theory

a Samuelson on the 'mathematical heart of Sraffa'

23. As we said in introducing this Comment, a theme runs like a red thread through Samuelson's paper, as well as through his other works:[38] it is the denial

of the existence in Adam Smith and Ricardo of a theoretical paradigm alternative to neoclassical demand and supply. As Samuelson himself puts it in his paper: 'I strongly believe in the evidence that Smith, Ricardo and John Stuart Mill used essentially the same logical paradigm as did Walras and Arrow-Debreu' (Samuelson 2000: 140; cf. also ibid.: 113, 117, 126; and 1978: 1430).

We may leave aside Mill, whom we can better see as a transitional figure:[39] but, if competing scientific paradigms entail alternative 'ways of seeing the world and of practising science in it' (Kuhn 1970: 4), then what we saw this far on Smith and Ricardo's theories of wages and outputs fits the definition remarkably well. On the question, and on its multiple aspects we must now focus our attention.

Indeed the two specific criticisms of Sraffa's (1960) discussed in sections III and IV above were already a clear expression of Samuelson's rejection of the possibility of a theoretical approach alternative to neoclassical demand and supply. The alleged need for assuming constant returns is the result of implicitly taking the neoclassical demand functions as an inevitable reflection of reality. Similarly, Samuelson's claim as to the irrelevance of the Standard commodity and of the notion of basic products ultimately comes from the difficulty of conceiving the division of the product as based on a wage broadly determined by institutional forces, with the consequent treatment of it and of the product as independent variables when determining profits and prices. The same appears to be the source of Samuelson's objections to Ricardo's invariable measure of value and indeed to his labour-value measurements.

However in the section on the 'Mathematical Heart of Sraffa' in his (2000) paper, Samuelson exposes his interpretation of Sraffa's theoretical approach, and he had previously done the same for the classical authors in his *Canonical Classical Model* (Samuelson 1978) and elsewhere. To these more explicit statements of his interpretation we must therefore turn first for our discussion.

24. In the mentioned section of (Samuelson 2000), Sraffa's theoretical position is seen by Samuelson to consist of two main elements. The first is what Samuelson describes as a 'short run' analysis, which at times appears to refer to the traditional normal positions of the economy and, at other times, to steady states of the same.[40] It is exemplified in terms of a three-goods, three-factor model, indistinguishable, as far as I can see, from the standard neoclassical treatment, except for the discrete number of alternative methods available for producing the commodities. Given those premises it is not surprising that Samuelson should conclude that the only difference from dominant theory is that: 'what in smooth neoclassical technologies are smooth demand and supply curves become in Sraffa land-step function loci' (2000: 125).

As for the second element, relating to the 'long run', Samuelson apparently also attributes to Sraffa (1960) the kind of 'stationary state' built on neoclassical lines which we shall presently discuss in his 'Canonical classical model' (Samuelson 2000: 126–7).

The inevitable comment on both these accounts is that there is no evidence of such demand-and-supply analyses in Sraffa (1960), whether in the 'short' or

'long run' forms described by Samuelson. On the contrary, it would be hard to reconcile them with, for example, Sraffa's remark about the rate of profits being determined by the money rate of interest (ibid.: 33). Samuelson seems to simply state here his belief that Sraffa cannot but share some form of the neoclassical approach based on demand and supply for productive factors[41] taken, thus, as the only conceivable approach to explaining distribution and relative prices.

b The 'Canonical Classical Model' versus the classical wage

25. Samuelson's 'Canonical Classical Model' (1978) is perhaps the best known among a group of similar interpretations of the classical economists, which were published almost simultaneously between 1976 and 1978.[42] Its main lines are familiar: J.S. Mill's famous stationary state (Mill 1871, Book IV, in particular chapter VI), where decreasing returns from the land would have forced the wage and the profit rate down to the level for which population and capital would both be stationary, is also attributed to Ricardo and even to Adam Smith.

That stationary state is then described as the 'long-run equilibrium' of the classical authors (Samuelson 1978: 1416) and is taken to reveal, in their final state of rest, the forces relating population growth and capital accumulation to wages and to the profit rate. During the 'transition' to such long-run equilibrium, those forces would tend to realize what is indicated by Samuelson as the classical 'transient state' or 'short run transient development' (ibid.: 1416–17). To simplify, he assumes labour and 'capital' to be employed in the same fixed proportion everywhere in the economy, so that we can refer to a balanced 'doses' of 'labour-cum-capital' (op. cit.: 1415–6). The 'transient state' is then the one where the marginal product of a 'dose' on the cultivated land shows an excess over the minimum of the final stationary state and where this excess is shared between the two factors so as to make them grow in the required proportion relative to each other.

It should be noted that these 'short run' or 'transient' states are meant by Samuelson to represent the *trend over time* of Smith's and Ricardo's 'natural' or 'normal' positions of the economy and not the positions themselves. The latter, as Samuelson interprets them, are instead the positions (to which no name is given) for which there hold 'any initial conditions of positive quantities [of capital and labour] balanced or unbalanced' (ibid.: 1421, 1423, 1428n.) and are accordingly identified, essentially, with neoclassical equilibria. It is here, with regard to these unnamed positions, that there emerges one basic difficulty affecting Samuelson's and similar reconstructions.[43] To it we must turn our attention in the remainder of this subsection.

26. The stress of Samuelson's overall interpretation falls, we saw, on two relationships involving the real wage and, which, for brevity, we may call here the 'Classical wage relationships': i.e. an *inverse* relation between the wage and the *growth* in the demand for labour (higher wages would lower profits and thus the source and incentive of capital accumulation), and a *direct* relation between it and the *growth* of population and labour supply. However, despite the *prima*

facie resemblance, and the impression one may get from the presentation of the Canonical and similar models, these two 'Classical relationships' do not in fact entail or support any interpretation of Smith and Ricardo's wage determination along neoclassical labour demand-and-supply lines. As we shall argue presently, such an interpretation turns exclusively on a supposed wage elastic demand for labour based on the given amount of capital available in any given situation: i.e. it turns on a proper demand function on neoclassical lines, and has in fact nothing to do with the growths of the demand and supply of labour contemplated in the 'Classical wage relationships'. The neoclassical features attributed to the classical economists in the Canonical and similar models appear therefore to rest, we shall contend, on what those models should demonstrate; namely the existence of a labour demand function on neoclassical lines in Smith and Ricardo.[44]

The question can be seen in all clarity in Samuelson's (1978) article. He notes the necessity for the Canonical model to be 'determinate and globally stable' (p. 1423), i.e. such that the level of the wage should lead back towards the balance in the growth of the two factors, characterizing the above-mentioned 'transient state', whenever the economy deviates from it. And in the face of the admittedly rigid labour supply provided by population in each given position of the economy, that task would require *a negatively elastic proper labour demand function*, founded, that is, on the given amount of capital of the position. The sought-for demand-and-supply mechanism would otherwise force us to the unacceptable conclusions that the wage falls to zero whenever population runs ahead of capital accumulation, or, symmetrically, gross returns on capital goods do the same, when the opposite is true; or, finally, the division between wages and profits is indeterminate in the fluke case when population and capital happened to be in exactly the 'balanced' proportion.

Samuelson admits, at this point, that no such needed elastic labour demand function can be traced in Ricardo or Smith, who always took as given the amount of labour employment possible in each given position of the economy, just as they did for the amount of population out of which that employment had to come. Samuelson's reaction to these admitted rigidities is then double.

In a first such reaction, the classical given labour employment of each position of the economy, forces Samuelson who *presumes* the existence of *some* labour demand function, in those authors, to attribute to them a vertical such demand function. He then justifies the latter by an alleged classical assumption of fixed proportions of capital to labour (and implicitly, of uniformity of that proportion between sectors[45]) due, Samuelson writes to the fact that it would be 'a-historical' to ascribe to Smith and Ricardo a variability of factor proportions. With that vertical labour demand, there comes, however, the mentioned threefold alternative between zero wages, zero gross rentals, and indeterminate distribution between wages and rentals. And Samuelson would have to argue that this behaviour of factor prices would push them towards the levels of the 'transient' balanced state (ibid.: 1423)—a difficult task for a sequence of equilibria, in which, apparently, the ratio of wages to gross rentals can only be zero, or infinity, or indeterminate.

But the most obvious difficulty is of course that those zero wages or gross rentals, or indeterminacy, were never even vaguely contemplated as a possibility by Ricardo or Smith.

Those conclusions are clearly unacceptable and here we come to Samuelson's second reaction. Instead of leading him to discuss his attribution to Smith and Ricardo of a mechanism based on demand and supply functions on neoclassical lines, the difficulty induces him to relent on the alleged classical condition of fixed factor proportions and the consequent vertical labour demand function: He writes 'Ricardo and Marx'[s] *knowledgeable commentaries on current events* presuppose recognition that, at certain price and profit rates, substitutions will be made that would not be competitively viable at other price and profit rates' (Samuelson 1978: 1523, our italics).

27. At a closer inquiry, therefore, the 'Canonical model' imposes on us the uneasy task of holding together the following three claims advanced by Samuelson: (i) Smith and Ricardo did have labour demand schedules for their 'natural' or normal positions of the economy; (ii) these schedules were however vertical because the proportions of labour to capital were assumed fixed in each sector (and uniform between sectors) as implied, Samuelson thinks, by the classical labour employment independent of wages (which, incidentally but importantly, Samuelson, here admits as characteristic of those authors); (iii) in 'knowledgeable commentaries on current events', however, Ricardo and Marx would have admitted some substitutability between labour and capital.[46]

There are contradictions in this argument. Either the 'commentaries' of point (iii) affect the labour demand function postulated in (i), which is just what Samuelson himself denies in (ii)—or we are back at having to explain why (i) and (ii) did not force those authors to the conclusions of zero wages or zero quasi-rents or indeterminate division between the two.

The most natural way out of the quandary into which the Canonical model ultimately lands us is, I contend, to recognize that Smith and Ricardo had no labour demand functions however primitive—whether with or without, the variable proportions of capital to labour, which are in fact irrelevant to the issue. But then, clearly, no ground is left for claiming as Samuelson does (1978, 1415) that: 'in every classical economist there is to be discerned a modern economist trying to be born'. Here again, therefore, Samuelson appears to view factor demand functions as an immediate reflection of facts, so that Smith and Ricardo's rigid labour employment—in fact *a result of their outputs treated as 'intermediate data'*—could only be explained in terms of a vertical labour demand function.

We should note in this regard, in analogy with what we stated earlier in par. 8 for the neoclassical demand functions of products that the inexistence of demand functions for labour in classical theory is no denial of dependencies of labour employment on wages. Such dependencies are quite conceivable in either direction—but the neoclassical labour demand function and the resulting demand-and-supply mechanism for labour are in effect a very specific conception of those dependencies. It is what a particular theory of distribution based on a 'substitutability between factors' construes them to be: quite different was the way of

dealing with them in Smith and Ricardo, based on their 'exogenous' wage and their other 'intermediate data'.

Samuelson's Canonical model seems thus in conclusion to fail to explain the wage of Smith's and Ricardo's normal positions by anything resembling neoclassical labour demand and supply functions. Despite the somewhat similar ring, the 'Classical wage relations' between wages and the *growths* of population and capital are totally irrelevant for that purpose. Only labour demand and supply functions existing in each given situation would be relevant and Samuelson's arbitrary attribution of them to the old classical economists lands him into the problems of the zero or indeterminate distributive variables, which we have just seen. His attempt in 1978 to solve those problems by a confessedly 'Neoclassical Elaboration of the Classical model' (1978: 1423) is on the other hand clearly irrelevant for an interpretation of Smith and Ricardo as they were.[47] It however bears witness of what we claimed above, of how that is, the 'canonical' interpretation ultimately rests on assuming what it set out to show: the existence in the classical economists of a mechanism on neoclassical lines.

All this raises, however, the question of which forces, then, other than demand and supply functions on neoclassical lines, did Smith and Ricardo suppose to trigger the wage changes adjusting, in their view, population to capital accumulation. This brings us back to the questions we approached in section II from the side of Arrow's query about a 'non-clearing' labour market in Ricardo.

c The classical 'proportion' between the demand and the supply of labour

28. The problem with the labour demand function, attributed to Smith and Ricardo in the Canonical and similar models, is not so much the difficulty of finding evidence for it in those authors: it is the ease of finding evidence to the contrary. And, paradoxically, the evidence begins to come to light as soon as we give closer consideration to passages that might *prima facie* seem to support the existence of such functions.

This is the case, partly seen already, with Smith's and Ricardo's statements about adjustments between population and capital accumulation. It is most importantly the case when Smith and Ricardo argue that wages depend on the 'proportion' between the demand and the supply of labour:[48] in fact, as we shall presently contend, the forces to which the classical authors refer that phase are fundamentally different from those underlying the neoclassical demand and supply functions.

A first key to that difference may be found in one of the many well-known puzzles that the treatment of wages by Smith and Ricardo has raised for modern interpreters.[49] McCulloch thought that, should a tax be imposed on the 'necessaries' of labourers, the compensating rise in money wages would not come 'until the pressure of famine or the slackened operation of the principle of population' have made themselves felt (letter to Ricardo of 15 May 1820, Ricardo 1951–73,

Vol. VIII: 190). Ricardo had instead written in the *Principles* that such a rise would occur with 'no interval which could bear oppressively on the labourers' (ibid., I: 165–6). And to McCulloch's question about the grounds for his position, Ricardo answered that 'it is in the interest of all parties that wages should so rise' (ibid., VIII: 196).

The contrast is clear between Ricardo's statement and what, for example, Samuelson's Canonical model attributes to him. Ricardo excludes here exactly the relative fall in population that that model requires for a return of real wages to their former level after a fall. Moreover, the contrast would be even sharper if we tried to envisage the adjustment as occurring within a 'natural' or normal position of the economy, interpreted in terms of some, however primitive, neoclassical labour demand and supply functions, Ricardo's conclusions would then follow only with a horizontal labour supply function raised by the amount of the tax, and in turn raising the equilibrium wage by the same amount:[50] but a horizontal labour supply from a given population clearly makes no neoclassical sense.

As we go further and attempt to understand Ricardo's surprising observation about the compensating rise in real wages being 'in the interest of all parties' (how can the rise in money wages relative to constant money prices for the products, be in the interest of the *individual* capitalist?) we must therefore turn, I submit, in a direction quite different from the neoclassical demand and supply functions of the Canonical model—a direction for which we shall see below further, strong textual evidence. It is the direction from which mainstream theory has departed, at least since Marshall rejected the 'relative strength of the competing parties'[51] as a determinant of wages. Indeed, the tax has presumably left that 'relative strength' unchanged and it would only be reasonable to say, as Ricardo does, that the compensatory wage rise is 'in the interest of all parties': it would be only rational for capitalists to yield straightaway what they would otherwise have to yield after useless conflict.

29. More general support for this line of interpretation as to what (proximately) controls the level of the wage in Smith and Ricardo can in fact be obtained when we turn to what those authors mean when using expressions such as 'the proportion of the supply to the demand for labour' to indicate what wages depend on in any given normal position. Indeed, the main argument that Ricardo uses in the *Principles* in support of his conclusions on the tax on 'necessaries' is: 'a tax on corn does not necessarily diminish the demand compared with the supply of labour: why should it diminish the portion paid to the labourer?' (op. cit., I: 166).

We do not need to stress how extraordinary that 'demand compared with the supply' would again have to be if we were to try to understand it along the lines of modern demand and supply functions. By any reasonable meaning we might give to such functions, supply and demand would surely change relative to each other because of the tax raising the labour supply curve.[52] However, when the demand and supply of labour are conceived as the classical single magnitudes, in analogy with what we have seen in par. 8 above for Smith's analysis of the

market prices of products, it is only natural that we should see them as remaining unaffected by the tax.

Then, however, the difference between those two magnitudes will measure the labour under-employment existing in the economy in any of its various forms. The 'proportion' between the two could thus be taken (as we in fact just did above) as what Marshall's 'relative strength' would pre-eminently depend on—almost a synonym for that 'strength' in Ricardo's times when labour hardly had any political and social power. On the basis of a plausible, if partial analogy with the market price of a product,[53] such a proportion could then be seen as the regulator of the wage in the given situation. It would in particular trigger, in a way completely independent of neoclassical labour demand and supply functions, the wage changes which those authors thought would provide for a long run mutual adjustment of population and capital accumulation.

30. It is from Adam Smith that the above interpretative line gets its most direct support: and it is a safe rule to assume that Ricardo implicitly defers to Smith whenever he does not explicitly disagree with him. As in the case of the Ricardo–McCulloch disagreement, the support comes mainly from passages on wages that have long puzzled modern interpreters. I am referring first of all to the 'advantage' that Smith tells us the 'masters' have in disputes over wages (1776, I: 59). Indeed this 'advantage' makes the role of what we have called the 'relative strength of the competing parties' quite explicit. It also specifies the elements making up that 'relative strength', when Smith explains that 'advantage' in terms of the masters' 'tacit but constant and uniform combination not to raise wages'; or of the greater ease for masters, than for workers, to form 'combinations'; or of the fact that masters can hold out longer in 'disputes over wages' because of their lesser dependence on workers, than of workers on them.

Now, these and other constituents of the masters' 'advantage' may be viewed as aspects of that same institutional framework, which we attributed in section II the classical role of setting lower to and upper limits to the individual wage bargains possible in any given situation of the economy—and, also, of making those limits compatible with free competition as understood by Smith and Ricardo. The 'masters' advantage' itself raises, however, a question of compatibility with free competition, which is partly different from that we saw in section II because the institutional framework is here supposed to affect the *level* itself of the normal wage rate, rather than simply set *limits* to the individual wage bargains around a level that could be conceivably determined only by other forces.

The question is in fact one that the turn economic theory took after Ricardo makes us ill-equipped to deal with. But what I believe can already be said is that such an 'advantage' does not imply that, individually or collectively, 'masters' have the power to set the wages of their workers so as to maximize in some sense their returns as a monopsonist in modern theory is supposed to do—a power which some modern interpreters have instead read into Smith's 'advantage', only to find themselves embroiled into a net of contradictions.[54] It rather is an 'advantage' that exists, we could say, *at an institutional level*, where its influence on the wage level is limited by the similarly 'institutionalized' strength of

the 'competing parties' and the equally 'institutionalized' interest of the community in its ordered functioning (as we saw to be the case for the minimum 'subsistence level' of the wage).

Viewed in this light, the 'advantage of masters' may then not contradict free competition as envisaged by the classical economists. Unlike, for example, the privileges of medieval corporations (Smith 1776, e.g. I, ch. X: 107), that 'advantage' would not raise obstacles to the freedom of the individual to buy labour from, or sell labour to, whomever he saw fit. This ensured the tendency to a uniform rate of remuneration for each kind of productive resource—and to uniform prices for products, thereby providing for any paying or 'effective' demand for commodity—and these elements appear to constitute the main content of free competition as conceived by Smith and Ricardo.

31. That conception of free competition may then, in particular, help us to understand what to a modern economist is perhaps the most surprising element among those listed under the 'advantages' described by Smith. It is that Smith appears to have viewed as compatible with a competitive labour market, not only the quoted *tacit* combination among the 'masters' not to raise wages (which could perhaps be interpreted as merely part of the just-mentioned general institutional frame within which the wage bargain occurs) but, as we shall presently recall, also the *explicit* combinations of 'masters' or 'labourers', the comparative ease in forming which he describes as directly influencing the outcome of wages bargains. The complex and, at the same time, limited nature of the 'advantage' of the masters is in fact illustrated by passages such as the following, where, after stating that 'there are certain circumstances […] which sometimes give the labourer an advantage', Smith continues:

> when in any country the demand for those who live by wages […] is continually increasing […] the workmen have no occasion to combine in order to raise their wages. The scarcity of hands occasions a competition among masters, who bid against one another, in order to get workmen, and thus *voluntarily break through the natural combination of masters* not to raise wages.
>
> (Smith 1776, I: 61, our italics)

where we may see: (i) the fluidity of the 'combinations' affecting wages and, so to speak, their continuity with situations with no combination, both kinds of situation being included by Smith among the 'circumstances', which may alternatively give 'advantage' to either of the two parties—a fluidity and continuity that we could hardly envisage in current theory defining competition and combinations in terms of independent maximizing individuals; (ii) a confirmation of how classical demand and supply are strictly related to elements concerning the 'relative strength of the competing parties' and therefore are not in contrast with combinations, whether of masters or workers; (iii) how these combinations in their continuity with situations of no combinations can be held not to contradict the individual freedom to buy and sell labour; and therefore free competition as

viewed by Smith—and this despite the recognition of their influence upon the level of wages.

32. The above also makes it possible to clarify what I argued about demand and prices in the classical economists in a comment of mine (Garegnani 1990: 288). In his reply to it, Samuelson takes my position to imply that Ricardo would have denied that: 'changes in demand and outputs altered factor prices and relative goods prices' (Samuelson 1990b: 320–1), to which Samuelson reacts by pointing out that Ricardo did, for example, admit a rise in 'the real wage relative to real land rents' because of the Napoleonic Wars' need for standing armies (1990b: 320).

Because of its general relevance, I have touched on the point already (in par. 1 above), but it is now perhaps easier to see how the question does not lie in any claim of mine that Ricardo would have *denied* such a wage rise, but rather in Samuelson's difficulty in recognizing the specific way in which Ricardo would *not* have denied it. It is indeed easy to envisage the Napoleonic standing armies raising 'the proportion of demand to supply of labour' in Smith's and Ricardo's sense, and hence raising the wage: but as the texts to which we referred indicate, that would rather have to do with existing levels of labour employment and unemployment, and the resulting 'relative strength of the competing parties', and not with the neoclassical re-distribution of a fully employed labour force from land-intensive sectors to labour intensive services.[55] The possible rise in wages is not then a predefined result of the given change in 'tastes': if the size of the output changes is considerable, a wage rise is indeed likely to follow, but if the changes are small, or compensated by decreases of other outputs, or cushioned by a very large reserve of unemployed labour, or contrasted by broader, social or political events, wages might well not increase at all. What we find here is simply an instance of the action of one set of 'intermediate data' (the outputs) on a second one (the wage) to be studied separately from the determination of relative prices, in accordance with the case in hand and with no pre-ordained results deducible from a few postulates.

It is indeed in this connection that Samuelson objects to my '*binding on Ricardo*' (Samuelson 1990b: 320) such separate treatments and reasonings 'by stages'. But the latter are indeed one and the same thing as the inexistence of demand functions for commodities in Ricardo, widely recognized by historians and, as we saw, in a way admitted by Samuelson himself. Surely, it would be more difficult to join Samuelson and instead bind on Ricardo, the full employment of labour that the classical authors would share with Walras and Arrow–Debreu (Samuelson 2000: 140).

This point of classical labour unemployment is one to which we have referred several times in the present paper so far, and to which we must now devote some more specific attention.

d Samuelson on Ricardo on 'machinery'

33. Samuelson appears in fact to have appreciated the decisive role that the question of Adam Smith's and Ricardo's position on permanent labour

unemployment may have for his interpretation of those authors as essentially sharing the neoclassical paradigm. Over the last three decades he has repeatedly attempted to interpret the *locus classicus* of the question, Ricardo's chapter XXXI 'On Machinery', in a way that would leave open the possibility of attributing to that author the neoclassical tendency to full labour employment (cf. Samuelson 1978, 1987c 1988b, 1989, 1994, 1998).

As I had happened to recall, back in (Garegnani 1970: 427), Wicksell brought sharply to the fore in (1934) the contrast between neoclassical theory and Ricardo's argument in chapter XXXI.[56] Ricardo's thesis of a fall of the 'gross produce' as a result of the introduction of machinery was, Wicksell contended, 'theoretically untenable' because: 'as soon as a number of labourers have been made superfluous by these changes, and wages have accordingly fallen, then, *as Ricardo failed to see*, [other] methods of production will become more profitable […] and absorb the surplus of idle labourers' (Wicksell 1934: 137; our italics), with the product rising correspondingly back to, and beyond, the previous level.

In his articles on the subject, Samuelson criticizes Wicksell for assuming that Ricardo ascribed the fall of produce to labour unemployment. Samuelson's argument, when reduced to its essential terms, is as simple as it is surprising: Ricardo's fall in gross product would result from a decrease of population, not from labour unemployment.[57] In Samuelson's interpretation, that fall would only come *after* an 'intermediate' full employment equilibrium, such as that which Wicksell accuses Ricardo of overlooking.[58] But to achieve that equilibrium, wages would have to fall below the subsistence level at which Ricardo would have assumed them to be, before the introduction of machinery. And with wages below subsistence, Ricardo would have supposed population to fall in a longer run, the social product falling then accordingly. Wicksell's criticism would thus have been unjustified: Ricardo's different conclusions about a fall in the produce would have been due merely to his having carried the analysis some steps further, on the basis of the classical peculiarity of the subsistence wage.

Once Samuelson's argument is understood in its basic simplicity, the comment can be equally simple. As an application of the 'Canonical model' to technical innovations, the argument is imaginative, but when referred to Ricardo's chapter XXXI, it runs against all the textual evidence I can see in the chapter.[59]

A few quotations should suffice here. Ricardo writes: '[the capitalist's] means of employing labour would be reduced in the proportion of 13.000 to 5.500 l. And, consequently, all the labour which was before employed by 7.500 l. would become redundant' (Ricardo 1951–73, Vol. I: 389), where the fall in the *social* gross product that Ricardo argues would follow from that 'substitution of machinery for human labour' is taken to be just that experienced by the producer of the example, that is the difference of 7.500 pounds between the former product of value 15.000 (13.000 of wages plus 2.000 profit) and the new product of value 7.500 (5.500 of wages plus 2.000 profit). No indication anywhere in the chapter of simultaneous compensating rises in both labour employment and production elsewhere in the economy, on the basis of the existing resources as would be required by Samuelson's intermediate equilibrium.

In fact Ricardo writes more generally:

> the discovery and use of machinery may be attended with a diminution of gross produce; and whenever that is the case, it will be injurious to the labouring class, as some of their number will be thrown out of employment, and population will become redundant, compared with the funds that are to employ it.
>
> (Ricardo, op. cit., I: 390)

where the diminution of production (now explicitly related to the *social* 'gross produce') is unambiguously ascribed to labourers having been 'thrown out of employment'.

Further light is then shed, if necessary, by passages such as the following, relating to the longer run:

> But with every *increase* of capital [the capitalist] would employ more labourers; and, therefore, a portion of the people *thrown out of work in the first instance would be subsequently employed*.
>
> (Ricardo, op. cit., I: 390; our italics)

This passage contradicts Samuelson's interpretation in at least four respects: (i) in the longer run, instead of the gradual *fall* in employment due to the fall in population supposedly caused by the below-subsistence wages of Samuelson's intermediate equilibrium, we find a *rise* in employment; (ii) that rise in employment regards 'a portion of the people thrown out of work in the first instance' who, in Samuelson's intermediate equilibrium, would instead have already found re-employment together with those still unemployed, elsewhere in the economy, and what is more with the *old capital* and no necessity of the 'increase'; (iii) according to Samuelson's interpretation that early increase in capital should have gone instead to increasing the ratio of capital to labour,[60] thereby raising the wage gradually back towards subsistence, and slowing down the fall in population; (iv) the increase in capital could only be accompanied by *the rise* in employment mentioned by Samuelson, *after* the wage has risen above subsistence, when it would concern only new workers from the increasing population (and not the old displaced workers, who would have had early re-employment only to prematurely die for failing subsistence.

VI A doomed critique?

a The traditional 'normal position' versus 'stationary' or 'steady' states

34. If Quesnay, Smith and Ricardo were in effect elaborating a theoretical approach basically different from the subsequent one resting on the conception of substitution among 'productive factors', the question that naturally arises

before we can conclude is: why revive that classical approach today, with Sraffa, so long after its effective abandonment?

Whichever the circumstances of that abandonment—and Sraffa reminds us that the approach was 'submerged and forgotten' (1960: V) and not, therefore, rejected after an informed criticism, as we would expect in a normal scientific development[61]—reviving it today has clearly to do with the deficiencies of the approach that was developed and came to dominance subsequently. This leads us to Samuelson's views about what he sees as Sraffa's 'One Basic Novelty' (2000: III) and to what we shall contend that novelty really implies for neoclassical theory.

It will however fittingly introduce this second basic issue to start by commenting on Samuelson's reproach to 'Sraffian literature' for the excessive attention, which, in his opinion, it devotes to 'steady states' of the economy (Samuelson 2000: 136). Four points will be recalled in succession in this and the following two paragraphs.

The first point is that the position of the economy to which Sraffa (1960) refers is *not* a stationary or 'steady' state (stationary state for short from now on), as contended by Samuelson. It is the traditional 'normal position' to which the classical economists had referred, being then followed in this respect by all subsequent neoclassical authors until comparatively recent decades.[62] Stationary and normal positions are in fact basically different. Thus, as we shall presently argue considering the economy in a stationary state flatly contradicts the central purpose of considering it in a normal position: namely allowing for the possibility of a correspondence between theoretical and observable variables.[63]

The two positions have in common two properties. The first is the uniform rate of return on the supply prices of those capital goods that pertain to the dominant techniques—for brevity, from now on 'uniform returns on capital supply prices'. This means of course that the price of no such capital good has to fall below its supply price because of arbitrage and that all of them can be produced and replaced.

No less importantly, both positions have in common their definition in terms of prices constant over the relevant period of time, thus entailing a uniformity of the commodity own rates of interest (not to be confused with the just-mentioned 'uniform return on capital supply prices'[64]).

Here however, in the logical foundation of that assumed constancy of prices, comes the essential difference between the two positions. In a normal position the price constancy is a straightforward abstraction from all kinds of changes in the data, justified merely by a sufficient *persistence* of them[65] (ensured, among other conditions, by the above 'uniform returns on capital supply prices'[66]). In a stationary or steady state instead, the price constancy is the result of the hypothesis of the inexistence of tendencies to changes in the data and, in particular, of a tendency to changes in the one datum of the normal position for which the theory implies a longer term endogeneity, namely, the capital endowment (or, in a steady state, the capital per worker).[67]

Now, it is this logical foundation of the constancy of prices in the stationary state that differentiates it from the normal position because in conflict with the

possibility of a correspondence between theoretical and observable variables. No actual economy can in effect be supposed to be on average in a state where its data are not changing and, in particular, in a state where no incentive exists for changes in the capital endowment (capital per worker in steady states).

The weaker foundation of price constancy, represented by the 'persistency' of the normal position, allows instead for those inevitable changes in data. This is the result of focusing attention on 'persistent' forces and by accordingly taking their changes as generally too slow to be considered in the definition of the equilibrium and susceptible instead to be treated, over time, as *una tantum* changes among the other such changes by the method of comparative statics.[68] Given a stability of the theoretical variables, and therefore the tendency to self-correct deviations from the normal levels, the persistency of the theoretical variables would allow for the repetition of transactions on the basis of nearly unchanged data, ensuring a compensation of those deviations and making the theoretical level a guide to an average of the actual levels. In Marshall's words:

> Though the actual value at any time [. . .] is often more influenced by passing events, and by causes whose action is fitful and short lived [yet] in long periods these *fitful and irregular* causes in large measure efface one another's influence so that [. . .] *persistent* causes dominate value completely.
>
> (Marshall 1920, V, iii, 7: 291, our italics)

The fact that Sraffa refers to normal positions of the economy is, on the other hand, made quite clear, among other things, when he writes that 'such classical terms as necessary price, or natural price or price of production', would correctly describe the prices of his book and proceeds then to distinguish them from 'market prices' (evidently taken in Adam Smith's definition), to which, he adds: 'no reference is made in this book' (Sraffa 1960: 9).

35. The second of the four points I wish to recall is one I raised back in 1976. It concerns the causes of the quiet disappearance of the normal positions from the pure theory of the last decades. It therefore centres on Hicks (1939), who initiated in mainstream literature the movement away from them and towards an alternative consisting of the temporary and intertemporal equilibria—of which today's stationary or steady states are in effect the strict complement.[69]

As I argued (1976), a close examination of *Value and Capital* (Hicks 1939) suggests that the origin of that development lay essentially in Hicks's implicit recognition of the failure of the three-quarters of a century search for a consistent conception of capital as a single productive factor. On such a conception was in fact based, in neoclassical theory, the possibility of determining a normal position of the economy with its uniform return on capital supply prices: Walras' attempt at such a determination, on the basis of a capital endowment expressed as a given physical vector, had ended in the logical inconsistency of his general equilibrium system when completed with 'capital formation', as Walras himself came to admit in the fourth editions of his *Elements*.[70]

The crisis of 'capital', the single magnitude—on which Hicks himself had based his *Theory of Wages* (Hicks 1932) not many years before *Value and Capital*—entailed, therefore, the crisis of the normal position, in favour of the mentioned Hicksian equilibria, where the Walrasian conception of the capital endowment was and is adopted, but the condition of the 'uniform return on the capital supply prices' inconsistent with that conception was and is in effect quietly abandoned together with the normal position. It seems significant of the connection between the two crises that Hicks' (1939) 'temporary' and 'intertemporal' equilibria could in effect achieve dominance in mainstream pure theory, only after the initial stages of the capital controversies had, by the early 1970s, made the notion of a 'quantity of capital' absolutely untenable in pure theory. A result was the increasingly lamented extreme formalization and opacity of contemporary pure theory, interacting with its sharply reduced explanatory capacity.[71]

It seems therefore—and this is our third point—that the attention that Sraffa and other authors are again able to devote to the normal position can be viewed as an important expression of their success in recovering, through classical theory, the possibility of a correspondence between theoretical and observable variables.

36. Our fourth and last point relates more strictly to the argument by which Samuelson rejects as 'undue' the attention given to what are in effect the traditional normal positions. He writes: 'steady states are subsets of the dynamic paths that economic systems can and do follow […] rare in comparison with the totality of states' (2000: 136).

The argument here evidently rests on taking Sraffa's normal positions as a stationary or steady state, usable only when the economy satisfies the corresponding conditions. But for Sraffa, as well as for all neoclassical theorists up to the Pigous, Robertsons or Champernownes—up, that is, to what I have got used to calling the 'Hicksian divide' in neoclassical theory, when, three or four decades ago, the new notions of equilibrium became dominant—there was no question of reproducing the 'paths that economic systems […] do follow'. As, again, Marshall had pointed out long before, and the predecessors of Hicks (1939), including Hicks (1932) himself, had in effect unanimously accepted: 'dynamical solution in the physical sense of economic problems are unobtainable [so that] statical solutions afford starting points for such rude and imperfect approaches to dynamical solutions as we may be able to attain to' (Marshall 1898: 39). Normal positions and their comparison over time approved accordingly to be the essential constituents of such *attainable* 'imperfect approaches'.[72]

The positions of the economy to which Sraffa refers are not, then, particularly rare subsets of dynamic paths: normal 'positions' are meant to analyse *any* dynamic path, i.e. in Samuelson's words, the 'totality of states', but to do that by the only means that, Marshall tells us, are seriously possible, though the inexistence of capital as a consistent single magnitude has forced those means out of present-day neoclassical pure theory.

This brings us now to the mentioned 'One Basic Novelty' concerning capital for which Samuelson gives recognition to Sraffa (2000: section I).

b A doomed theory?

37. Samuelson sees Sraffa's 'one basic novelty' to lie in the conclusion that consumption per head in a stationary economy need not fall as the interest rate rises and the methods of production adopted change accordingly. That seems, however, a rather indirect statement of results questioning the basis of the neoclassical approach and, with it, the very possibility that the theory, though with complete 'future markets' (or perfect foresight), can at all sustain its doctrine that in a competitive market economy the community can ensure additional future consumption by increasing individual thrift.

Let me explain. Samuelson (1961, 525) had put the doctrine in the following way. When the community's propensity to save increases 'profit (interest) rates fall', and *then* the community 'uses its productive resources to create more of all varieties of physical capital goods [so that] more consumption […] will be producible at a later date'. The (1961) statement points to the key element in the doctrine, not recalled in the (2000) passage: namely, the fall in interest rate that should cause the demand for more capital goods. The increased future consumption depends in fact on today's production of additional capital goods and, therefore, on somebody demanding them. But that demand cannot come *directly* from the new savers: it must come from firms motivated by the fall in the interest rates[73] resulting from the attempt to save.

Now, that demand for 'more of all varieties of physical capital goods' is just what will not occur when, as Samuelson puts it: 'consumption available per scale of primary factors does not decline when the equilibrium interest rate rises' (2000: 111–12). The failure of consumption to fall as interest rates rise is in fact strictly associated with the more immediately significant and disturbing failure of the incentive to raise the proportion of capital to labour in the economy as interest falls.[74] When that occurs, the increased thrift and consequent fall of the interest rates will cause firms to demand *less and not more* of those 'all varieties of physical capital goods'. Less and not more of them will accordingly be created, just when the potential supply of savings has increased. And the fact that 'consumption per head in a stationary economy need not fall as interest rates rise' more simply questions, at the level of pure theory and, I repeat, under complete futures markets (perfect foresight), the power of the rates of interest to balance savings and investment decisions.[75]

What, then? The disparity between decisions to save and invest will entail according to the theory that the increased propensity to save will *not* materialize in *any* additional consumption for the community, so long as the technical conditions imply stationary consumption per head not to fall as the interest rate rises.[76] And if the question still had a meaning at that point, beyond merely following the logic of the theory, future consumption would *fall and not rise* as realized physical savings fall in step with investment, leading towards what the

theory could entail to be an implosion of the system.[77] These, it appears, can be the implications of what Samuelson describes in the comparatively innocuous terms of a failure of stationary consumption to fall as the interest rate rises.

So, as we now focus on the 'doomed critique' of Samuelson's section title (2000: 115), it seems natural to start by noting the radical difference between the above and what the theory was originally believed to imply on saving and, more generally, on distribution and prices. The earlier putative implications, unlike the later ones—appeared to be in no conflict with experience and they contributed accordingly to the initial credence in the theory. Once this radical difference between the early (believed) results and the present ones is duly considered, and of course if the latter are correct, the theory would surely seem a more likely candidate for 'doom' than its critique, which is progressively bringing the new results to light.[78]

Demand and supply functions, let us recall, are not the immediate reflection of facts that their long dominance make them appear today—a dominance strongly favoured, if not determined, by the Marshallian interpretation of Ricardo, and the associated lack of clarification of Smith's and Ricardo's alternative conception of demand and supply. Those functions are the result of a highly sophisticated attempt to explain those facts in terms of the idea of a substitutability between 'factors of production'. In this basic role, the idea was entirely absent from the early untrammelled theorizing of Petty, Quesnay, Smith or Ricardo, and it has in effect progressively exhibited its deficiencies over the last three-quarters of a century, since at least Keynes' *General Theory*.

38. Professor Samuelson must be congratulated for having agreed to continue in 2000 the discussion started in 1990. One way or the other this will help, I believe, towards a better economic science. Scholars should be put in the best conditions to assess what suits that aim, and this requires that the possibility of an approach alternative to the dominant one should be admitted and discussed in its own terms, rather than 'submerged' as historically it once was, as Sraffa reminds us in a book that not for nothing has prompted Samuelson to a 'third of a century of exploration and reflection'.

Appendix A: An alleged error and the real question

In (1926) Sraffa wrote: '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: 540–1: our italics), where consistency with the assumptions of *partial equilibrium* is seen to prevent consideration of decreasing or increasing Marshallian returns (increasing or decreasing supply curves).[79] Samuelson (1990a) sees, however, a 'fatal error' in this position and argues that in *general equilibrium* decreasing Marshallian returns are the normal case, since the relative prices of the factors required in high proportion by the expanding output rise, also forcing changes in favour of techniques economizing on such factors, and thus raising the supply price of that

output in terms of most other commodities. The reader can however note that no logical contradiction exists between Sraffa's and Samuelson's propositions and no error need therefore be present in either of their statements. The contrasting conclusions are due simply to the different kinds of equilibrium referred to.

Samuelson's argument to the contrary has, however, had the unfortunate effect of diverting the 1990 discussion from the true and important disagreement between the Sraffa of 1926 and today's Samuelson; namely, Sraffa's opinion at the time, that neoclassical general equilibrium is: 'a […] concept whose complexity prevents it from bearing fruit' (Sraffa 1926: 54, quoted in Garegnani 1990: 287).

The stand taken by Sraffa was in fact the one generally taken at the time. In Garegnani (1990: 287), to exemplify that I gave the following passage by Umberto Ricci about general equilibrium:

> Among the theories of equilibrium enshrined in the formidable apparatus of the formulae of [Pareto's] *Manuel d'e´conomie politique*, […] there is to be found no bridge leading to nine-tenths of the problems which economists set themselves […] we can [therefore] by no means afford to put aside the theory of particular equilibrium as developed by Marshall and his many followers […].
>
> (Ricci 1933: 20–1)

which, coming from a close pupil of Pareto, is highly revealing of the mainstream's stand at the time.

The (1990) diversion from that true disagreement was unfortunate, we said. This was so because it would have been fascinating to take the chance to rediscuss with Samuelson that stand, originating of course from Marshall, whose stress on partial equilibrium and its seeming grasp on concrete problems was certainly important, if not decisive, for the initial acceptance of the neoclassical system. Professor Samuelson has in fact been, together with Hicks, one of the main artificers of the radical change that has intervened in the meantime in neoclassical economic theory.

Appendix B: Capital in the classical economists

Paradoxically, it is a revived 'classical' theory with its analysis of the outputs and the competitive wage—integrated by the Keynesian advances on what is today understood as the distinction between decisions to save and decisions to invest—that can reassure us against the implosion of competitive markets, seen in par. 37 of the text, to emerge as a conceivable result of intertemporal theory once the inconsistency of the notion of capital as a single magnitude is properly taken into consideration. Samuelson implies the contrary when he writes:

> 'It would be easy here [in the Canonical classical model] to deal with many capital goods of differing durability. But it is ludicrous to think that

> problems that haunt a post-neoclassical writer today […] were themselves absent from the century of 1750–1850 or were better handled by some lost paradigm of the capitalist [*sic*: for 'classical'?] writers. Under a powdered wig you find the usual head, like yours and mine, sometimes inflated and sometimes sage, but quite innocent of magic charms and skeleton keys to banish complexity.
>
> (Samuelson 1978: 1429)

He does not appear to be correct here. The difficulties with capital, which we glimpsed in the text,[80] concern the attempt to determine the interest rate (profit rate) by demand and supply functions of savings (capital), according to the theory of distribution and relative prices that goes with them. As intuition may perhaps suggest, once the classical theoretical paradigm is perceived, those difficulties are not likely to arise if the profit (interest) rate is to be determined on the basis of the difference between what is produced net, and a wage treated as an 'intermediate datum' in the way discussed earlier in this essay. Investment could then be whatever it can be, under the assumed conditions and affect the social product accordingly,[81] without any risk of implosion of the system. It would therefore seem natural to conclude that the problems in question were not present (and not even lurking in the background) in the economic analyses of 1750–1821—and are also not present today, when we study the economy along the approach of those economists.[82]

Abstract

In this article, part of an ongoing discussion, Samuelson (2000) is taken as the occasion for a critical examination of Samuelson's work on the classical economists and Sraffa, a subject of continuing interest for that author, especially after Sraffa (1960). The article argues for the existence in Smith and Ricardo of an alternative approach to distribution and prices, and it aims at a critique of Samuelson's contention that 'Smith, Ricardo and J.S. Mill used essentially the same logical paradigm as did Walras and Arrow and Debreu' (2000: 140).

In the first two sections, the attempt by Arrow (1991) to detect in Ricardo a theory of prices independent of demand—and founded instead on a real wage determined separately from, though not necessarily independently of, prices and the non-wage distributive variables—is considered with its implication of the wage entering the determination of the latter as an 'intermediate datum' of the theory. This then makes it possible to outline the characteristic analysis we find in Smith and Ricardo, where the wage as 'intermediate datum' entails a similar treatment of the output levels. The resulting theoretical structure is then used in order to answer, in sections III and IV, the two basic criticism, that Samuelson has advanced against Sraffa (1960). While the claimed dependence of the (1960) prices on an assumption of constant returns is voided by the mentioned treatment of outputs as intermediate data, the relevance of the Standard commodity, as well as that of Ricardo's 'invariable measure of value' is explained by the needs

of determining non-wage incomes as a difference or 'residual', the essence of the theoretical structure under consideration.

Section V then deals more directly with Samuelson's denial of the existence of a classical paradigm of economic theory. His arguments and interpretations are found to be in contrast with central features of Smith and Ricardo's work and, in particular, with their theory of wages. Thus, the admission of labour unemployment in 'normal' competitive positions compels Samuelson to a highly questionable interpretation of the chapter 'On Machinery' in Ricardo's *Principles*. In section VI, finally, the attribution to 'Sraffian literature' of a central concern for what Samuelson sees as 'steady states', but are in fact the traditional 'normal positions' of the economy leads the article to the deficiencies of neoclassical theory—an issue inevitably underlying the debate on the Classical paradigm. The dependence of the traditional versions of the theory, based on normal positions, on the notion of capital as a single magnitude—which forced the generalized abandonment of those versions in pure theory after the early stages of the capital controversies—is argued to emerge as equally present in the contemporary reformulations of the theory, thus affecting them, it is argued, no less than it did the abandoned earlier versions.

Keywords

Samuelson, Sraffa, classical economists, neoclassical theory, capital, wages, demand and supply, distribution, surplus theories

Notes

1 Samuelson gives no Ricardo reference for his attribution and the closest I have found is the following passage, where an 'improvement' in the conditions of the 'labouring class' is seen to be possible in a country at war: 'which is under the necessity of maintaining large fleets and armies' and therefore 'employs a *great many more men than will be employed when the war terminates*' (Ricardo 1951–73, I: 393) and where the passage we have emphasized states the unemployment of those 'many more men' *after* the war, and presumably *before* it. Ricardo's admission of permanent unemployment (such, that is, that it can be eliminated only by further capital accumulation) in chapter XXXI, 'On Machinery', of the *Principles*, will on the other hand be considered in section Va below.
2 On the notion of 'normal positions' of the economy, see section VIa below.
3 Sraffa 1960: V.
4 See below par. 29 where Smith's and Ricardo's altogether different notions of demand and supply for labour are considered.
5 The no-demand features of Ricardo's work, to which Arrow is pointing, were shared, in their essentials, by Smith and the other classical economists up to Ricardo: as Arrow notes (1991: 70), John Stuart Mill was already on partly different lines (cf. also Garegnani 2002: 242).
6 We are at the moment ignoring for our outline the presence of scarce natural resources: what we shall say below concerning outputs treated as 'intermediate data' will take care of them.
7 Arrow attributes to Ricardo the assumptions leading to the labour theory of value, but he is certainly aware that these assumptions are unnecessary for reaching the essential

Ricardian result of interest to us now, namely a no demand determination of prices and the non-wage distributive variables.

8 When neoclassical demand and supply functions are mentioned in this paper, whether for factors or commodities, 'general-equilibrium' demand and supply functions and not partial equilibrium ones are generally meant (on these notions cf. e.g. Garegnani 2003: 395 n.4).

9 Garegnani 1998: 419.

10 This Marshallian notion, we may observe, is more restrictive than the constant physical returns to scale by which we qualify a production function today in that the former generally also entails constancy in the relative prices of productive services.

11 Cf. e.g. in Ricardo, the increase in the output of 'corn' with the progress of accumulation.

12 Thus, for example, Ricardo did not deem it necessary to consider any change in corn consumption per head as its price increased because of capital accumulation. The question is taken up by Stigler (1965) and Barkai (1967), who fail to stress that what was relevant for Ricardo was only the *sign* of the changes, and that a fall in corn consumption per head due to the rise in the corn price could not, in any case, reverse the very rise explaining it.

13 Of course in the discourse which Ricardo and Smith conduct in their texts, what we indicate as constant returns may be seen to be often implied, but to no exclusion of variable returns in other circumstances. The essential point here is not the *denial* of constant returns, but the denial of the *need* for them.

14 Cf. e.g. Blaug (1999: 223), Stigler (1965: 449), Barkai (1967: 75), etc. Arrow himself stresses the point (1991: 75).

15 Significantly enough, Smith's and Ricardo's word 'proportion' applied to demand and supply was criticized by J.S. Mill, just when the notion of a demand schedule was beginning to take shape and to attract attention. That critique hopelessly obscured the original Smithian notion of 'effectual demand' (see the following footnote and J.S. Mill 1871: 448, quoted in Bharadwaj 1989: 138).

16 The question of the stability of the position of the economy to which the theory refers its variables is of course no less important for classical theory than it is for the neoclassical one (On the question see also the special issue of *Political Economy*, 6, I–II, 1990; Garegnani 1997 has argued that Smith's conclusions about the stability of the normal price hold, essentially also when simultaneous deviations of 'market' from normal prices are allowed for in all markets). Starting with Marshall, Smith's and Ricardo's analyses of the market price have, however, often been used to argue continuity between the classical approach and the later demand-and-supply determination of prices. This line of argument, which seemed to have become less prominent after Sraffa's edition of Ricardo, was revived by Samuel Hollander, who described Ricardo's treatment of the market price as 'Ricardo's analysis of resource allocation' (cf. Hollander 1979, e.g. 271: on that argument cf. Garegnani 1983: 178 n.). Arrow (1991), on the other hand, while contending, as we are seeing, that Ricardo attempted to determine price independently of demand considerations, also argues that 'some of Ricardo's analyses can only be made sensible on the basis of [the concept of a demand schedule]' (ibid.: 75) and takes as an example that of 'market prices'. However, it appears that in the analysis of Smith and Ricardo the market price needs be generally postulated only as ordinally higher or lower than the natural price depending on a quantity brought to market, ordinally lower or higher than 'effectual demand'. Empirical observation seems then sufficient to validate those propositions, which may well find part of the basis in purely temporary phenomena such as changes in inventories (cf. e.g. Smith 1776, I: 50) and require no particular justification in the systematic phenomena postulated in neoclassical theory and necessary to justify the definiteness and persistence of the relation between price and quantity observed by the market.

17 Thus, Samuelson unwittingly acknowledges Ricardo's treatment of outputs as 'intermediate data', when often reproaching him for taking as given corn production and therefore under Ricardo's simplifications the position of the no-rent land, when determining prices.

18 The 'normal wage' here referred to, while coinciding with Smith's 'natural wage', corresponds rather to Ricardo's market wage, which appears to often have the character of persistency of a *normal* wage.

19 It may be interesting to note how the absence of an elastic labour demand function tends to reverse the causal relation between labour unemployment and wage rigidity: whereas in neoclassical theory real-wage rigidity appears as the cause of unemployment. In Smith and Ricardo, it rather emerges as an effect of the normalcy of the latter along lines not dissimilar from those argued much later by Keynes for money wages. On the consideration of labour unemployment as a normal phenomenon by Adam Smith and other eighteenth century writers cf. Hollander 1973a: 245; Blaug 1958: 75, 179, Stirati 1994: 183 and *passim*. On the evidence provided in that respect by Ricardo's famous chapter 'On Machinery', see section Vd below. On the specific question of the absence of the idea of a negatively elastic demand function for labour in the early writers cf. Stirati op. cit.: 183 and *passim*.

20 Smith wrote: 'There is a certain rate below which it seems impossible to reduce, for any considerable time, the ordinary wages' (1776, I: 60). Customs and institutions are also seem to set a symmetrical upper limit to the wage bargains in each given situation of the economy, Smith writes about masters being 'in a sort of tacit, but constant and uniform combination not to raise the wages of labour above the actual rate' (Smith 1776, I: 59; for a closer examination of these and similar passages see below section Vc).

21 Cf. e.g. Marshall 1920, Appendix J, 2: 679, where the 'relative strength of the competing parties', which decides day-to-day wage levels is seen to be ultimately dependent on the demand and supply forces considered in his theory of wages.

22 Samuelson might be envisaging the necessity of constant returns, not so much because of the mere free-competition aspect of Sraffa (1960)'s uniform rate of profits on the supply prices of the capital goods: but rather because of the quite distinct constant-relative-prices aspect of that uniformity, for which the *own commodity rates* of return would also have to be uniform. Constant returns would then be necessary for the steady state hypothesis allegedly underlying the constancy of relative prices over time. If that were the meaning of Samuelson's remark, his criticism would not hold because, as we shall argue in section VIe below, Sraffa refers to a normal position of the economy and not to any such 'steady state' (I owe the above possible interpretation to my colleague Sergio Parrinello).

23 I find some other passages relating to this particular line of argument more difficult to follow, since they seem to argue the necessity of assuming constant returns to scale for solving important problems: but, surely, Sraffa's point is that we *do not need* that assumption, not that we *cannot make it*, when appropriate. Thus, for example, referring again to Sraffa's chapter I, on the no-surplus economy, Samuelson assumes an invention to double all outputs obtainable from the techniques of that economy, so that 'the system can now grow exponentially doubling every period' and he concludes: 'who can believe *that* if constant returns to scale is in any way denied?' (2000 121–2; italics in the original). But is there any reason why Sraffa's (1960) analysis should not be used to carry out such constant-returns analyses, if we so wished? The question is whether Sraffa's analysis should be *confined* to such analyses, or be used also for studying the way in which an economy is actually likely to grow.

24 Cf., e.g. Garegnani (1990: 293). On the two alternative, but analytically equivalent, classical treatments of distribution see also Garegnani (1984, sections V–VII).

25 Thus, Stigler refers to 'Ricardo's basic theorem on distribution: a rise of wages would invariably lower profits' (1952: 190); in Samuel Hollander (1979: 7), we similarly

read: 'the entire Ricardian scheme is designed to relate the rate of return on capital to the value of per-capita wages […]. This relationship will be referred to as the fundamental theorem on distribution'; cf. also Blaug 1958, e.g. 24.

26 Of course Smith had the difficulty of the rent of land as a second element in the surplus—an element of which Ricardo could get rid of by means of Malthus's theory of rent. Indeed, some elements make it appear that Smith's notion of a rate of profits varying independently of wages might be reconstructed not so much as the result of an adding up price theory but, rather, as an erroneous quasi Physiocratic scheme, where the rent of land constitutes the ultimate surplus. As, unlike in Quesnay, profits on capital also entered the surplus, Smith seems to have treated them as independently variable within the limits of the aggregate surplus according to a rate determined by whatever Smith may have meant by 'competition of capitals'. This at least appears to be the logical entailment of Smith's argument when, for example, he views a tax on wages falling ultimately on rent (1776, book V, ch. II, art. II; cf. also, in the same chapter, art. IV on a tax the similar treatment of tax on 'necessaries'). The tax, Smith argues, will be borne first by capitalists. In the manufactures, they will, however, be able to maintain the previous profit rate by raising the price of their products (relative to corn) to compensate themselves for the higher wages inclusive of tax. The reasoning here rests on the constancy of the corn price, and it will not therefore apply to the production of corn itself. In that production the maintenance of the previous profit rate will instead impose, in Smith's view, a lower payment of rent, i.e. a lower share of corn output for the landlord who will thus *physically* pay the tax on *agricultural* wages. The landlords will also pay most of the tax on *manufacturing* wages, through the smaller purchasing power, in terms of manufactures, of a unit of the corn constituting the residual rents. The same change in the price of manufactures relative to 'corn' and the same basic distributive scheme seems then to be envisaged by Smith for the case of independent changes in the real wage due to changes in the 'demand of labour' (see below par. 29) and also, it appears, for that of an autonomous change in the profit rate. We referred to an inconsistency in this entire distributive scheme: it emerges when, with Ricardo, the *differential* nature of rent is brought to light. Then, as Ricardo concisely notes, the farmer of the marginal land 'could not deduct the tax [on wages] from his rent […] for he pays no rent' (Ricardo 1951–73: 156). The constraint binding the distributive variables through the 'surplus equation' applies then to wages and profits alone with profits as the surplus on which the tax on wages falls.

27 As is well known, Ricardo had reached his theory of profits by the first half of 1814, more than a year before he came to abandon Smith's idea that a rise in wages would raise all prices.

28 Cf. e.g. Sraffa (1951: xxxi).

29 On Hollander's (1973b) and (1979)'s criticism of Sraffa's interpretation of Ricardo's early 'corn' theory of profits, cf. Garegnani (1983). I am, on the other hand, unclear as to how my reference in Garegnani (1990: 293–4) to Sraffa's above corn measurements could be interpreted as belief in the labour theory of value and be an incentive to demonstrate that a 'corn-model' violates that theory 'as generally as the *n* goods case does' (Samuelson 1990b: 321–2). Indeed no demonstration was necessary once the 'corn model' was correctly understood (Sraffa 1951: xxxi–ii) as including any number *n* of commodities, produced in any technical conditions whatsoever, and exchanging, therefore, in any ratio whatsoever—provided only that the wage is assumed to consist entirely of corn.

30 As Stigler writes: 'Ricardo's basic theorem on distribution […] is thus strictly dependent on his measure of value. The product of a given quantity of capital and labour […] always has the same value' and this is so, Stigler specifies, in terms of a commodity produced with a constant quantity of labour and an average ratio of capital to labour (Stigler 1952: 190–1). See also '[Ricardo] proceeded with the analysis of profits by […] utilising a labour theory' (Hollander 1979: 6) and Blaug (1958: 222).

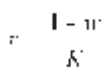
31 Samuelson's (1980) discussion with Hollander may help to explain further Samuelson's difficulties in accounting for Smith's error. He appears there to take Ricardo's criticism of Smith's theory of profits based on the 'competition of capitals', as a denial of the possibility of any long-run fall in the rate of profits for reasons other than decreasing returns from the use of land (1980: 577). However, Ricardo never had any difficulty in envisaging a fall in the rate of profits due to a rise in the real wage. Take the following passage relating to something close to the cause Samuelson accuses Ricardo of ignoring, i.e. a rise in the proportion of capital to labour: 'there is only one case […] in which the accumulation of capital with a low price of food may be attended with a fall of profits, and that is when the funds for the maintenance of labour increase much more rapidly than population' with the resulting rise in wages (Ricardo 1951–73, II: 292–3). Samuelson seems on the other hand also to refer to 'Say's law' as a possible basis of Ricardo's objection to Smith (ibid. 1980: 577); however, a rejection of Say's law would not have done away with Smith's error: deficiencies of aggregate demand lowering the general profit rate would only introduce an additional cause of rise in the real wage, as Malthus himself came to recognise under Ricardo's influence.

32 Whenever we measure the 'value' of a commodity by the labour embodied in it, as Ricardo also did, the uniformity of the real wage entails, by definition, that the part of value added, not going to wages, must be proportional to the uniform labour necessary for its production, i.e. will give equal rates of surplus value in Marx's terminology (differences in the working day would, for example, amount to differences in hourly wages and would not cause differences in rates of surplus value, because of the usual Ricardian procedure for reconducting labour to uniformity according to the scale of the relative normal wage: Ricardo 1951–73, I: 20–22).

33 Dr. Tucci of Rome 'Sapienza' University informs me that the theorem was first published by Perron in 1907. Professor Samuelson describes as 'Herculean' the task I am attributing to Ricardo (Samuelson 1990b: 322). On the evidence available I find that the adjective is not excessive for Ricardo's logical achievements through first his 'corn', and then his labour measurements.

34 The passage reads: 'It is […] *technological predictability* rather than vague philosophical implications, which constitutes what it is that would be interesting about a simple labour theory of value, a conclusion that seems to have been rather overlooked in the literature' (1961: 521; our italics). That 'predictability' had surely not been overlooked in the passage by Sraffa (1951) quoted in Garegnani's (1990) Comment (293–4), and to which Samuelson curiously objected in his reply (1990b).

35 With circulating capital only, Sraffa's equation is $r = R\ (1 = w)$, with r, w and R as, respectively, the rate of profits (interest), the wage in terms of the standard commodity, and the 'Standard ratio' between product and means of production in the Standard system. The equation yields the classical surplus equation once the Standard ratio R is expressed as P/K with P and K as the product and capital (circulating only) respectively, in the Standard system: since the Standard product is set by Sraffa as the unit of the standard commodity we have:



36 In his (2000) paper Samuelson returns to the question he raised in 1990 (271–3), for which changes in the method of production of basic commodities, and hence in the Standard commodity, would deprive the latter of any relevance. I had commented then (1990: 29) that the change of the Standard commodity as methods change would disqualify the latter no more than changes under the same conditions of e.g. the 'factor price frontier', would disqualify that frontier. To this Samuelson replies that 'the logic of the two cases is disparate: there is one and only one [wage-profit] trade off locus no

matter how variable the techniques' (1990: 321). However, my analogy was with the individual frontier corresponding to *one* 'technique' for producing the wage good: this, Samuelson would agree, is a highly interesting construct in itself (e.g. as the *necessary basis* of the single 'trade off locus' of Samuelson's passage) and is certainly not disqualified by being different for each different technique.

37 As recalled in my 1990 comment (Garegnani 1990: 291–2), the reason why necessaries do not appear as basics is only, Sraffa tells us, that he wishes to refrain from 'tampering with the traditional wage concept'. But, he continues: 'necessaries are essentially basic, and if they are prevented from exerting their influence on prices and profits under that label, they must do so in devious ways, e.g. by setting a limit below which the wage cannot fall, […])' (Sraffa 1960: 10). Thus, there seems to be little textual basis for Samuelson's claim, in his answer to a 1990 comment, that Sraffa intended to build his Standard commodity 'on the rock of technology' by therefore excluding necessaries from the means of production (Samuelson 1990b: 321 n.1). It seems, on the other hand, that even if necessaries were eliminated from the list of basics, the existence in general of some basic product would hardly be in doubt (different steels may be required for different commodities, but they all require iron ore and even services require some tools in order to be accomplished).

38 Cf. Samuelson (1977, 1978, 1980, 1987a,b,c, 1988, 1990a,b, 1998, 2000).

39 Cf. Sraffa's specification of his standpoint (1960) as being that of 'the *old* classical economists from Smith to Ricardo' (1960: V, our italics) thus clearly excluding J.S. Mill.

40 Thus, Samuelson implies a normal position in its neoclassical long-period-equilibrium form when referring to the supply of 'capitals' as a given (2000: 126–7) and not as the unknown of a stationary or steady state (Samuelson seems not to notice here that the plural of 'given capitals', as distinct from the singular of 'given amount of capital', is incompatible with the uniform rate of return on the capital goods' supply prices of the neoclassical normal position that he attributes to Sraffa). But, elsewhere, Samuelson takes that same 'short run' as a 'stationary' or 'steady' state (ibid.: 123–4). On the distinction between normal position and steady state, see par. 34 below).

41 This interpretation of Samuelson draws of course some support from Sraffa (1925) and (1926), who, although highly critical of Marshall's stress on demand and utility, still shared the overall demand and supply approach, at least with regard to the partial equilibrium form of that approach. But already the three pages of his (1960) Preface gave clear indication of a change in Sraffa's (1960) thought in that respect (on that change, cf. Garegnani 2005).

42 Cf. Levy 1976, Hicks and Hollander 1977, Casarosa 1978, on these models cf. Stirati 1994: 157–8.

43 To focus on that one difficulty, we shall here leave aside other deficiencies of these interpretations, such as the characteristic disturbing juxtaposition between the mechanical analogy implicit in demand-and-supply equilibria and the historical–cultural circumstances determining the classical subsistence wage. The latter circumstances underlie, however, statements by Ricardo, such as the following: 'population may be so little stimulated by ample wages as to increase at the slowest rate – or it may even go in a retrograde direction' which of course is sufficient by itself to threaten the whole 'Canonical interpretation' (Ricardo 1951–73, VIII: 169). In fact, if the subsistence minimum wage is cultural, the reactions of population to a divergence of the actual wage from it might be 'cultural' too, even in their sign, and above all likely to change with the social circumstances as in fact they did. And this seems to be what is contemplated by Smith in his complex position on population (cf. e.g. Spengler 1959: 7; on this uneasy coexistence between mechanical and cultural elements in the model).

44 On the matter cf. also Garegnani 2002.

45 The uniformity between sectors of the (fixed) proportion between capital and labour

would in fact be necessary in order to avoid an elasticity of demand based on consumer goods' substitution. The condition is not mentioned by Samuelson and is in effect contradicted by Ricardo in numerous passages, starting from sections IV and V of ch. 1 of the *Principles*, concerning the 'modifications' of the labour theory of value because of the 'unequal durability of capital and unequal rapidity with which it is returned to its employer' (Ricardo 1951–73, Vol. I: 38) in the various sectors. In fact, as we shall observe in the text, the basic question in Ricardo is not at all that of a lack of alternative methods of production or of difference in the proportion of capital to labour between the various sectors: it is the absence of any attempt to found a labour demand function on such phenomena. We may here note that other authors are less circumspect in attributing elastic labour demand functions to Ricardo and the classical economists than Samuelson is (cf. however n.67 below). Thus, Casarosa assumes we can find in Ricardo the wage-fund doctrine in the form it took in J.S. Mill before the famous recantation (1977: 316), while Hicks and Hollander (1977) apparently go the whole length of attributing to Ricardo a straightforward neoclassical investment demand.

46 We may note that in any case the admission of permanent labour unemployment by Ricardo and Marx for their natural or normal positions of the economy would make any considerations by them about variability in the proportions between labour and capital goods irrelevant for Samuelson's purpose, which is that of finding some support for a classical wage determined by the equilibrium between labour demand and supply functions.

47 In fact Samuelson comes close to admitting the inexistence of any labour demand function in the classical economists when, in introducing that elaboration, he airs the idea of a 'missing equation' in Smith's and Ricardo's theory of distribution. He writes: 'nonetheless if we wish to flesh out the torsos of their logically incomplete models we must supply the equations missing for the additional unknowns' (Samuelson 1978: 1423). 'Missing equations' in earlier writers, may however be a question of *us* missing 'the equations' that are in fact there. And this appears to be the case in point here, where what is being missed is how the classical 'exogenous wage' can determine distribution without passing through the equilibrium between the labour demand and supply function, which Samuelson presupposes must somehow exist.

48 The 'proportion between demand and supply of labour' is in fact what Ricardo sees as regulating his *market wage* (e.g. op. cit., I, 94), to which, however, as we remarked in n.18, he tends to attribute a persistence making of it what we call here a normal wage: persistent enough, that is, to give rise to a normal position of the economy. That same 'proportion' is, on the other hand, what Smith refers to as affecting the *natural* wage, when he often refers simply to the 'demand' for labour, the supply being implied in the existing population. In both Smith and Ricardo there remains the idea that in some longer run the wage is determined by subsistence, just as the price of a commodity is regulated by its expenses of production. But in both authors the analogy is more or less explicitly admitted to be imperfect because of the different time required for the supposed response of the supply to the price in the case of labour and because of the variable cultural aspect of both the response and the subsistence level itself. It may also be noted that the temporary increase in the quantity of a product absorbed by the market as its price falls (e.g. because of storage) does not appear to be envisaged in the case of labour.

49 Cf. e.g. Shoup 1960: 64–77, 126–9, 140–2; Hollander 1979: 393–4.

50 On the question, Hollander (1979), while writing 'the precise mechanism [Ricardo] envisaged in the market remains difficult to grasp', points to Ricardo's passage 'the value of things I believe to be influenced, not by immediate demand and supply only, but also by contingent demand and supply', and he comments: 'It would appear that Ricardo allowed for a forecast by employers of the consequences of permitting real wages to decline – namely a reduced growth rate of labour supply' (1979: 393–5). It

is not, however, clear how that forecast should cause a single employer to pay now the higher wage that he might be forced to pay in the future, the more so since the wage he individually pays today can do little about his future labour supply. Hollander seems rather to resort here to an idea that Knight (1935) had advanced, perhaps polemically, to point out what he saw as the glaring deficiencies of Ricardo's theory of wages, namely the idea that the employer would fix the wage by 'arbitrary fiat' at the level appropriate for the required increases in population. The interpretation of Ricardo's 'contingent' demand and supply as being taken care of at the very same institutional level preventing the wage to fall below subsistence wages seems certainly to make more sense than the idea of employers *individually* assessing the wage balancing population growth with accumulation and paying it spontaneously in disregard of their individual interest.

51 Marshall [1920], App. J: 679.

52 Only in the case of a vertical supply curve could the tax be said to leave unchanged the 'demand compared with the supply' in their neoclassical sense—the very case in which, contrary to what is argued by Ricardo, the wage far from rising to fully compensate the tax, would not rise at all.

53 Cf. n.48 above.

54 Thus, Edwin Cannan asks: 'If the combination of masters has the power of depressing wages with which it is credited [by Smith] why should it leave the labourers enough to support a family?' (1967: 185). And Frank Knight similarly notes: 'since workers are not actually slaves by inheritance, there is no reason why the individual employers should provide the workers with maintenance for a family' (1956: 81). Samuel Hollander, on the other hand, cuts the knot by simply postulating an 'arbitrary decision of monopsonistically organized employers' to act in accordance with what Smith describes as 'common humanity', a hint perhaps here, at institutions providing for the orderly survival of society (Hollander 1973a: 185 n).

55 Samuelson's difficulty in conceiving of an explanation of distribution alternative to neoclassicism emerges again, when he writes that 'understanding how changes in demand and outputs altered factor prices and relative goods prices' is a 'pre-marginalist banality' (1990b: 320). The pre-marginalist banality might be the above rise in wages because of the 'Napoleonic standing armies', but certainly not the highly sophisticated neoclassical tendency to the full employment of labour, which constitutes an essential part of that 'understanding' according to Samuelson.

56 It may be interesting to note, as how Lars Jonung (1981) reports, an article by Wicksell with his lucid argument on the question, submitted in 1925, was rejected by the *Economic Journal.*

57 See e.g. 'Ricardo's readers should not have been shocked by his third edition discovery that invention of machinery could depress the real wage *and lower the population and the total of product*' (Samuelson 1978: 1428, our italics).

58 Thus, Samuelson describes the post-innovation full employment equilibrium, which Wicksell envisages in opposition to Ricardo and concludes: 'But Ricardo never denied *that*. Wicksell failed to notice that Ricardo went on to consider the long-run equilibrium when the supply of labour shrinks in order to insist on receiving the subsistence wage' (1989: 52, italics in the original).

59 Samuelson writes: 'Although, strictly speaking, we cannot find in Ricardo's words what would pass today for an entirely satisfactory proof of his contentions, his basic intuition is on the mark' (1989: 47). The difficulty however is not the absence in Ricardo of a *proof* of his contentions: it is the *absence* in Ricardo of the contentions themselves. An admission of some weakness in his interpretation can perhaps be detected also when Samuelson writes 'Ricardo's *result* has not the slightest reason to invoke disequilibrium levels of unemployment.' (1989: 54, our italics). It might seem here that Samuelson is only claiming that a fall of social product can be argued *independently* of labour unemployment, whether or not Ricardo did so. But

certainly Wicksell, and Samuelson himself in passages like those of nn.57, 58 above, refer to Ricardo's chapter XXXI, and not to a theoretical possibility. And, in any case, if Samuelson's contention were not intended as a reconstruction of Ricardo's own argument, it would be of no relevance here since it would leave us with Ricardo admitting labour unemployment in contrast with the paradigm of 'Walras and Arrow–Debreu'.

60 Samuelson seems indeed to have abandoned here his misgivings about the existence of a negatively elastic labour demand function in Ricardo and to attribute him what Samuelson himself had described as merely a 'neoclassical elaboration' of the Classical model (cf. 1978: 1423). And this occurs already in the very (1978) article containing those misgivings and elaboration. For contexts other than chapter XXXII, in which Ricardo implies the existence of labour unemployment, see e.g. n.1 above.

61 Blaug's ch. V of *Ricardian Economics* (1958) makes interesting reading in this respect, as does e.g. Foxwell's (1899) statement, in his Introduction to Anton Menger's *Right to the whole produce of labour* (1899), about Ricardo, 'who did more than any intentional socialist authors to sap the foundations of that form of society which he was trying to explain'.

62 For a selection of quotations of neoclassical authors regarding the former unanimous reliance on normal positions for the analysis of the economy, cf. Garegnani 1990.

63 On the general 'rules of correspondence' between theoretical variables and observable magnitudes cf. e.g. Nagel 1961: 105.

64 The uniformity of commodity own rates of interest only entails, with regard to the 'uniform return on capital supply prices' that, when the latter uniformity holds, its nominal expression given by the own rate of interest of the numeraire becomes independent of the numeraire. The two uniformities have indeed been frequently mixed up in the course of the capital controversies and have created, at times, a serious obstacle to an understanding of the issues involved. Thus, the abandonment of the normal position with its uniformity of returns on capital supply prices' caused by the inconsistency of the previous notion of capital as a single magnitude, a defensive change undoubtedly limiting the explanatory capacity of the theory has been confused with the abandonment of the uniformity of the commodity own rates of interest, resulting merely from the consideration of future variations in relative prices, a change that could instead be represented by Hicks (1939), as an advance towards an 'economic dynamics' (on the confusion of the two uniformities, see Garegnani 2003, Appendix II, where references are given to works of Frank Hahn and Christopher Bliss).

65 For the notion of persistency of the normal position, see Garegnani 1976: 28.

66 Clearly, in an 'equilibrium' in which the returns on the supply prices of the capital goods differ, and we must suppose gross investment to be concentrated on a few such goods only—prices would, other things being equal, change faster than they would when, starting from an adjusted physical composition of the capital stock, gross investment would tend to be spread over all kinds of capital goods.

67 For an early, neat distinction between, on the one hand, the normal position in its neoclassical version as a long-period equilibrium and, on the other, the stationary state, cf. Robbins 1930.

68 Garegnani 1976: 28.

69 Once the position of the economy to which the theory refers has been changed toward the 'Hicksian' equilibria and their dated prices, the 'persistency' of the normal price comes naturally to be interpreted in terms of the strict constancy of a stationary state. Even apart from the inherently temporary character of those Hicksian equilibria, the dating of prices excludes, by definition, the conception of a price, like the normal one, meant as a centre of gravitation and accordingly validated through a sufficient *repetition* of the transactions. The prices of the theory appear instead to aim at nothing less than a faithful reproduction of the path of the

actual prices (on that attempt, see e.g. the quotation from Pareto in n.72 below), where, of course, constancy can only mean stationarity. The stationary or steady states become then a peculiar partner of the intertemporal or temporary Hicksian equilibria, seemingly tempering the perplexities about the fruitfulness of the latter and somehow filling the gap left by the quiet disappearance of the normal position. Conveniently enough, the stationary or steady states also entail representing as unknowns the capital endowment and its physical composition, thus doing away, also in the interpretation of past authors, with the inconsistency of treating that endowment as the given single magnitude of the normal position (cf. the following footnote) it thereby removes the most transparent, though not the most basic, aspect of the difficulties which capital raises for neoclassical theory.

70 On Walras's inconsistency see Garegnani (1960, Part II, ch. II and III; also e.g. 1976, 34–5). To ensure the 'uniform returns on capital supply prices' the physical structure of the existing capital endowment must in fact be determined endogenously, i.e. the neoclassical given capital endowment must be allowed to change in form though not in quantity in the process of achieving equilibrium, as Hicks himself had aptly put it in his *Theory of Wages* (1932: 20), when he was still basing his theory on normal positions (long-period equilibria).

71 There clearly is a relationship between the loss of the possibility of 'correspondence' in neoclassical pure theory and 'the risk […] that economics progressively loses touch with real problems, develops on its own into a scholastic' noted by Malinvaud (1991: 66). Reference has thus been increasingly made of late to a 'formalist revolution' in the neoclassical theory of the period after the war, though what are here contended to be the roots of it, does not seem to have as yet been sufficiently uncovered. Cf. e.g. Blaug (2003).

72 The following remark by Dennis Robertson is also significant in this respect: 'It seems to me that anybody who rejects these two ideas, that a system can move towards equilibrium and that it may never get into it – has made it extremely difficult for himself to interpret the course of events in the real world' (1963: 144–5). And, taking up the same question from a more general angle, Pareto had written: 'we do not know nor shall we ever know any concrete phenomenon in all its details: we can only know *ideal phenomena* […]. We must therefore assess a general theory on the basis of general or average facts, not on the basis of accidental facts' (Pareto 1896–7, paras. 35–6; my translation).

The 'normal position' may be taken as a typical instance of Pareto's 'ideal phenomena' in economics, centred as it is on Adam Smith's 'central price', to which 'the prices of all commodities are continually gravitating' (1776, I: 51) and therefore providing what Pareto calls here a 'general or average fact'.

73 On the tendency of the commodity own rates of interest to move in the same direction in a system of general intertemporal equilibrium, cf. Garegnani 2003, para. 16.

74 See e.g. Fig. (a) below, relating to the model that Samuelson used for his 'Surrogate Capital' article (1962), where a single consumer good is produced by alternative techniques differing by the kind of the single capital good employed which is the one also used to produce itself. The wage curves α and β for the corresponding 'techniques', have vertical intercepts $Og_\beta > Og_\alpha$ representing the maximum wage, i.e. the physical net consumption output per worker in the integrated production of the consumption good (or equivalently, the stationary consumption per worker), with the respective technique. We can then see that as the interest rate *falls* from just above, to just below, r_s, β becomes more profitable than α, and we have *a fall* of 'stationary consumption' per worker from Og_α to Og_β. For exactly the same reason, the consumption-good value of capital per worker in that integrated production, evaluated at switchpoint S, will also fall from k_α to k_β, measured by the trigonometric tangents of the respective angles (on the reading of these quantities see Garegnani 1970: 410).

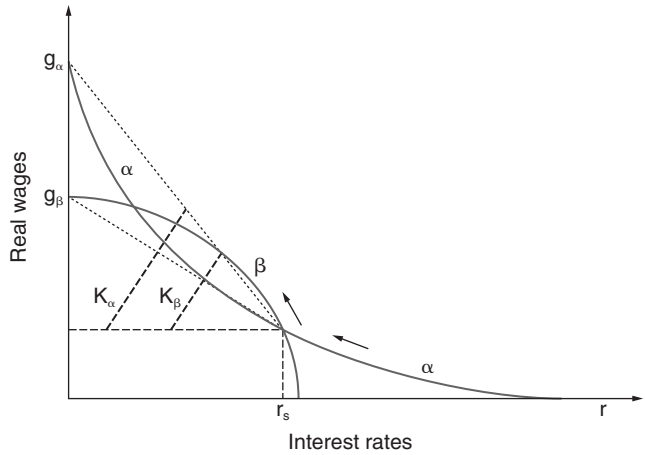


Figure a Wage curves α and β show the relation between the real wage ω and the interest rate *r* when the corresponding techniques α or β are in use. At point *S* as *r* decreases, permanent consumption per worker falls from g_α to g_β and capital per worker in the integrated production of the consumption good, valued in terms of the latter, correspondingly falls from K_α to K_β.

75 The argument in the text can be used to invalidate the intuitive argument advanced at times, according to which, with complete 'futures' markets, excess savings would not be possible, because to any decision to save there would correspond additional, specific, future consumptions and, therefore—it seems to be thought—a matching amount of investment (see e.g. the passage by Arrow discussed in Garegnani 2003: 435, n.59). Suppose, however, that in a two-period intertemporal equilibrium, with circulating capital only, and no scarce natural resources, the auctioneer had just achieved equilibrium but for some excess savings ΔS in $t = 0$ and the corresponding excess consumption $\Delta C = \Delta S$ (at discounted prices) in $t = 1$. The neoclassical fall of the interest rates resulting from the additional decision to save will, with reverse capital deepening, cause firms to produce that additional consumption (just like all other consumption for $t = 1$) with a lower ratio of capital (investment) to the given labour force, just when the ratio in which capital (saving) is being supplied relative to labour has increased. The equality that complete 'futures' markets ensure between planned individual demands and planned firms' outputs will then simply entail excess saving supply matched by excess labour demand. (Our example here, with its circulating capital may incidentally be used to easily show, if necessary, that the future consumption ΔC cannot by itself ever cause investment equal to the savings ΔS, since the value of the consumption produced ΔC must include wages besides the investment which has been necessary for its production).

76 The freedom with which capital (investment) per worker can change with the interest rate is exemplified by the three curves k_1, k_2, k_3 of Fig. (b) below, which are taken from the numerical examples given in Garegnani 1970: 428–36. Indeed, any other relation *k* between the two variables, keeping within the shadowed area of Fig. (b) is also possible. (The above results were obtained with reference to the traditional normal positions: for their application to an intertemporal system, see Garegnani 2000: 29–30). It may here be noted that the strict parallelism between changes in value of capital per worker and in physical stationary consumption per worker can be expected only when the comparison is effected at the switchpoint between the

techniques, as in n.74. Thus, the net physical consumption output per worker for $r \geq 0$ must always reach its maximum for $r = 0$ even when k decreases for part or even the whole of the positive range of r as is the case respectively of k_3 and k_1. in Fig. (b).

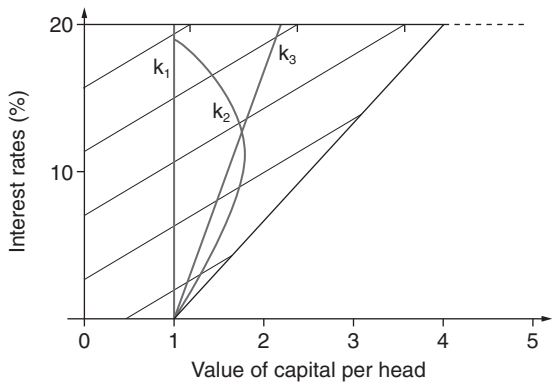


Figure b k_1, k_2 and k_3 are possible relations between the interest rate and the value of capital (as defined for Fig. (a)), and the same is true for any such relation keeping within the shadowed area of the diagram.

77 Cf. Garegnani (2003, par. 23).

78 See Appendix B on Samuelson's contention that the difficulties concerning capital could not but be present or latent in the works of the classical economists.

79 A summary of Sraffa's argument is provided in my comment (Garegnani 1990: 284–7).

80 Samuelson cites 'the 1966 Hahn problem' as an example of the questions 'that haunt a post-neoclassical writer today'. That problem, however, is the result of some very particular assumptions about price expectations, and does not appear to have any relation with the questions here discussed (which as we said hold also under conditions of complete future markets or perfect foresight) except for the incidental fact that both issues depend on the existence of more than one capital good.

81 As I have argued elsewhere (Garegnani 1978–9: 338–40), 'Say's law' was in Ricardo neither an implication nor a premise of his theory of distribution and relative prices. Ricardo's position on it was due essentially to two elements: (i) the absence at the time of a sufficiently clear distinction between decisions to save and decisions to invest; (ii) Malthus's failure to see the necessary potential equality between value of output and individuals' purchasing power, the equality which, it appears, Ricardo was attempting to establish.

82 In fact in the 1958 Cambridge Ph.D. dissertation, *A problem in the theory of distribution from Ricardo to Wicksell* (see also Garegnani 1960), I had argued that a problem of measuring capital independently of distribution arose in *both* theoretical approaches but that, whereas the classical problem is soluble by means of a vectorial measurement of capital, the same solution is ultimately in conflict with the requirements of neoclassical theory.

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4 Classical and Neoclassical harmonies and dissonances

Paul A. Samuelson

'For a man with a hammer, everything looks like a nail'.

Warren Buffett

'Where it is a duty to worship the sun, the laws of heat will be poorly understood'.

John Morley

'Where it is a duty to *abhor* the sun, the laws of heat will be poorly understood'.

Paul Samuelson

1 Prologue

Possessing an idiosyncratic antipathy to adversary procedures in scientific discourse, I intend here to present a low-key, candid sample of my takes on heterogeneous capital competitive models *for non-neoclassical limited-substitutability convex technologies*. Just as to understand one country one needs to know two (or more) countries, I will be repeatedly comparing and contrasting neoclassical paradigms with earlier century classical paradigms and with my understanding of post Leontief-Sraffa paradigms.

In advance I want to honour Joan Robinson (1956) and Piero Sraffa (1926, 1960) for their seminal questioning of mainstream economists' complacencies and normative dogma about intertemporal capital theory.[1] Later I will list some personal indebtedness to modern non-mainstream economists.

The following text is, by agreement, not peer reviewed. So let every reader be on notice that errors and infelicities may be present. To optimize the rationed space allotted to me, I skip formal proofs and make no attempt to integrate optimally discussions of the several different topics addressed.

My ordering of different topics is neither related to their relative importance nor to their chronological provenance.

2 Introduction

Here are a few points that the present analysis will try to explicate.

1 It is a myth that there ever did exist a plausible classical paradigm in which competitive price ratios—among deer, beaver, corn and rye—were invariant under changes in objective consumers' demand tastes.
2 Also it is textually dubious that post-1870 neoclassicism (Jevons, Menger, Walras, Wicksell, Wicksteed, Marshall, Edgeworth, Cassel, Ramsey, Hicks, Meade, Solow, Samuelson, Arrow, Debreu…) differed from classicism (such as in Cantillon, Hume, Turgot, Smith, Malthus, Ricardo, James and J.S. Mill…) importantly because the former linked purely competitive supply and demand with *constant returns to scale* whereas the earlier group definitely did not have to do so.

The limited invited space here will be used analytically, not textually. Readers can consult such excellent commentators as Schumpeter (1954), Blaug (1978), Hollander (1987) or Niehans (1990). My own teachers in and out of the classroom were Viner, Knight, Taussig, Cannan, Robbins and numerous others. My views on these matters, as explicated analytically here, do happen to mostly agree with *their* views. But our common views should carry no weight in present debates. Today's purpose is to *deduce* what are correct behaviour equations under well-specified scenarios. If anywhere my non-peer-reviewed syllogisms are found to be erroneous, the present exercise will have been valuable in helping establish where the truth does *probably* reside.

3 In my many, many dialogues with Professor Joan Robinson, we worried about the normative properties of supply–demand markets. What she deemed to be apologetics for too-fat capitalists, I took to be solvable problems about 'intertemporal Pareto optimalities or *non*-optimalities'.[2]
   a Does 'double switching' imply intertemporal Pareto *non*-optimality? (see Pasinetti *et al*., 1966). Does 'capital', reversibility?
   b Does the 1956 Ruth Cohen *curiosium* (see Robinson 1956: 109–10) and the Liviatan and Samuelson (1969) violation of the 'normal' Ricardo–Hollander *inverse trade-off between the real wage rate and the interest rate* imply a similar non-optimality? If so, the Samuelson and Etula (2006b) violation would be also non-Pareto optimal. Since my space is so limited, I will simply report here that the 1956, 1969 and 2006 (so-called) anomalies are provably intertemporally Pareto optimal.
   c What about a view that *only stationary* states are deductively tractable? My use here of twenty-first century *dynamic* Samuelson–Etula Master Functions will rebut that claim for heterogeneous-capital scenarios (as was done in Samuelson and Etula (2006a) and in the Samuelson and Etula (2006c) *divertimento*-sonnet for Graz's sixtieth birthday Fest for Heinz Kurz). Demonstrated here will be some generic problematics about stationary states.
   d Buffett's above quoted quip can apply to the definable twenty-first century dynamic Master Function $C_1(t + 1) = M[K_1(t), K_2(t); K_1(t + 1), K_2(t + 1) + C_2(t + 1)]$ 'hammer', which deduces for Leontief–Sraffa

> *limited substitutability technologies* much the same *qualitative* properties as will hold for J.B. Clark–Ramsey–Solow *neoclassical* technologies. In particular, *non*-spurious marginalisms can be definable for *both* of these technologies—so that heterogeneous factors can (almost everywhere) have respective (marginal productivity!) equilibrium yields equal to ∂ output/∂ input or $\Delta Q/\Delta K_j$ expressions. Given more space, my expositions could have been more complete and less intuitive.

Supply without demand is like one hand clapping. Microscopic examination of Sraffa's (1960) 100 pages will detect little discussion of how a shift of consumer tastes away from durable goods might influence equilibrium profit rates. Nor do those pages contain nuanced analyses of how Robinson Crusoe's time preference for corn today rather than next year might alter materially equilibrium interest rates.

Linear programming paradigms à la George Dantzig (1948) applied to intertemporal scenarios shout out the need for tastes-demand equations to provide the *complete* equations of dynamic and static competitive equilibrium. Tersely—too tersely—I touch upon this vital problem.

Samuelson (1966) expressed sincere gratitude to Sraffa, Robinson, Garegnani, Pasinetti, Bruno–Burmeister–Sheshinsky, Kurz–Salvadori, Bliss, Schefold, Metcalfe–Steedman, Morishima and many others who corrected my earlier errors prior to and post publications on the complexities of intertemporal economics. In my considered opinion, an early Nobel Prize shared by Robinson–Sraffa–Harrod would have added lustre to Stockholm's first-decade choices.

3 Why 'natural prices' cannot be defined in the 1750–2006 era

For a few pages only, Smith (1776) exposited the Labour Theory of Value simpliciter. Suppose that to produce $q_1 = 1$ beaver, three units of L_1 Labour were needed; but to produce $q_2 = 1$ of deer, ten units of L_2 Labour were needed. Then Smith could cogently deduce:

$$P_2/P_1 = (10 \text{ of } L_1)/(3 \text{ of } L_2) = 3^1/_3, \text{ independently of tastes.} \quad (1)$$

This 'natural price' would hold true whatever might be the volatility of changing consumer tastes for the goods. Thus, when everyone always spends ninety percent of income on beaver consumption and ten percent on deer consumption, $3^1/_3$ would hold; $3^1/_3$ would also still hold if tastes changed so that all spent fifty–fifty percent on those goods. What holds for these two goods would hold also for three goods (deer, beaver, quail) or for N goods if all were producible out of Labour alone with constant unit labour costs *independently of scale*.

Suppose Smith also had reported that each $q_3 = 1$ of corn needs only two units of (homogeneous) Acres of Land, $A_3 = 2$; and that each $q_4 = 1$ of sugar needs exactly A_4 = ten units of Land. Then canny Smith, soft-pedalling the Labour

Theory of Value, might sign up for the Cantillon–Henry George, Samuelson (1959) 'Dated-Land-Content Theory of Value':

$$P_3/P_4 = 1 \text{ Acre}/10 \text{ Acres} = 0.1, \text{ independently of tastes.} \tag{2}$$

How would Smith, the embryonic general-equilibrium theorist, deduce competitive price ratios *for all these four goods at a time*? Not yet could he do better than

$$(P_2/P_1;\ P_3/P_1,\ P_4/P_1) = (3^1/_3;\ ?;\ ?), \text{ or}$$

$$(P_1/P_3,\ P_2/P_3;\ P_4/P_3) = (?,\ ?;\ ½). \tag{3}$$

What Smith still lacks among other things is the (Land Rent)/(Worker Wage) ratio = R/W. Until economists Smith or Ricardo know *Distribution* they generically cannot know *Values*. And vice versa! It is a circle, but it can be a virtuous general equilibrium circle. Also, do note that both Equations (1) and (2) do indeed obey *constant returns to scale*.[3]

The equations missing are what the Sraffa I knew never seemed to like very much: demand tastes, volatile as they sometimes are. So let us skip back to young J.S. Mill when he was successfully completing and perfecting Ricardo's (1817) comparative advantage trade of Portugal's wine for England's cloth. Eschewing the metaphysical fuzziness of marginal utilities, John Bull Mill *objectively* could postulate that (say) we all spend twenty-five percent of our disposable incomes on each of (q_1, q_2; q_3, q_4), denoted by

$$\theta_i = p_i q_i / \sum_{j=1}^{4} p_j q_j = \frac{1}{4},\quad i = 1, 2, 3, 4 \tag{4}$$

We still, however, will be missing needed essential data on available exogenous supplies of total Labour and total Acres of Land: here are such $\hat{L}$ and $\hat{A}$ data:

$$\begin{aligned} &\text{exogenous } \hat{L} = L_1 + L_2 = \text{say } 100 \\ &\text{exogenous } \hat{A} = A_3 + A_4 = \text{say } 50 \end{aligned} \tag{5}$$

All my exogenous numerical data have been put into bold type.

Given Equations (4) and (5), *all* the unique classical competitive equilibrium real prices (factor and goods) are determinate and calculable by simplistic linear substitutions. Smith, Ricardo and any reader should be up to the task. Do do it. And see how by 2 + 2 = 4 arithmetic, *all* the (non-natural) real prices are altered by changes in demand tastes and in relative factor supplies.

To save space, I will make the same point about the *generic* impossibility of natural classical prices by replacing the above 4-good scenario with a terser 2-good scenario involving only goods 1 and 3. All exogenous parameters remain the same, except that now Mill has consumers spending fifty:fifty percent on

labour-produced q_1 and on land-produced q_3. We will still be left with two ratios, two intrinsically non-natural real prices. Here in a nutshell is their demonstrated fatal lack in invariance.

Write $\hat{m}_3 = \hat{m}_1 = ½$ for Mill's exogenous p_3q_3/p_1q_1 ratio of expenditures. Write $\hat{L}/\hat{A}$ for exogenous relative factor supplies. And write $\hat{\Pi}_1 = \hat{3}$ and $\hat{\Pi}_3 = \hat{1}$ for respective technical cost coefficients. Then:

$$\begin{aligned} R/W^* &= (\hat{m}_3 - \hat{m}_1)/[\hat{L}/\hat{A}]; \quad (P_3/P_1)^* = [\hat{\Pi}_3/\hat{\Pi}_1](R/W)^* \\ &= [\hat{\Pi}_3/\hat{\Pi}_1](\hat{m}_3 - \hat{m}_1)/[\hat{L}/\hat{A}] \end{aligned} \tag{6}$$

Equation (6) is the QED for how 'unnatural' classical prices had to be *generically*. Only singularly—implausibly singularly—could changes in $\hat{m}_3/\hat{m}_1$ tastes or in $\hat{L}/\hat{A}$ factor supplies leave intact the natural prices nominated in Sraffa (1926) and 'approximated' by Stigler (1958). The point is so simple as to be almost banal, were it not for its prolonged neglect in the commentator literature.

4 Those occasional classical cases where goods might be *a*temporally producible by fixed proportion 'doses' of labours and land

Sometimes when early scholars did not know how to impute the separate shares of Labour and Land, they would posit that 1Q of Corn might require, say, a 'dose' of 2 from Labour *cum* three acres from Land. It was not hard to realize that in such a *single* activity case, no determinate fractional sharing of the harvest between landowner and labourers could come solely from the side of technology and costs. Extraneous demand or supply relations might cut the Gordian Knot at any fractional point between zero percent to labour wages and 100 percent to land rents or 100 percent to Labour and zero percent to Land.

Still, both in the early Anglo-Saxon and Germanic literature, there grew up a fairly sophisticated understanding of joint products or joint inputs. This is why it will be instructive for my future *temporal*-economic discussions of *heterogenous* capitals, first to work out here the easier to understand *a*temporal marginalist scenarios where a Q of corn output gets produced by doses of, say, (homogeneous Land; homogeneous male Labour, homogeneous female Labour), denoted by $(X_0; X_1, X_2)$. 'Threeness' is important pedagogically. Why? Because Sraffa's (1960) temporal analysis of Labour and Wheat and Iron does have the three-ness that Labour *cum* scalar K would lack. I will explicate how a Master Function is definable (in general!) for $Q(t) = F[X_0(t); X_1(t), X_2(t)] \equiv$ for short $F[X_0; X_1, X_2] = Q$. It will then remain only a short step to explain how more complicated Master Functions can apply both to everywhere differentiable Clarkian production functions and also (!) to limited substitutability, non-neoclassical, Leontief–Sraffa production functions for intertemporal input/output relations.

Given three exogenous atemporal Xs, determinate sharing of Q between them will be attained only when all three Xs *attain* supply and demand equilibrium distributive prices, (land rent, real male wage, real female wage) ≡ (y_0^*, y_1^*, y_2^*), such that the three distributive shares will then be:

$$\frac{y_0 X_0}{Q} + \frac{y_1 X_1}{Q} + \frac{y_2 X_2}{Q} = 1 \tag{7}$$

Only with scale-returns *constancy* will this addition to unity obtain. It will still remain an impossible puzzle when only a single A dose is technologically known. But distribution may become definitely unpuzzled when *three* sufficiently different A or B or C usable doses are known to every would-be competitive entrant into the Q industry.

Table 4.1 summarizes succinctly the following known-to-all A, B, C and D alternative sub-techniques.

Instead of employing the usual Leontief–Sraffa input/output coefficients of the form $a_{land,corn}$ = land input/corn output, Table 4.1 presents the equivalent technical data *normalized* to *unity* acres of the (homogeneous) Land. Readers should be grateful for this numeraire convention, because it minimizes diagrammatic excursions into the third dimension. Instead, the $[X_1/X_0, X_2/X_0]$ Euclidean plane can, by means of definable triangles or polygons, convey to the eye the whole intuitive story. Also, I put plentiful 0s and 1s in Table 4.1 as a crutch to inexperienced readers. Feel free to add = ±0.001 to 1's singular 0 coefficients, thereby altering quantitative results by itsy-bitsies only. (For Clark–Douglas neoclassical disciples, their everywhere differentiable technology of the form $Q = X_0^{1/4} X_1^{1/4} X_2^{1/2}$, will after normalization of X_0 to unity, become more transparent two-dimensional $Q = X_1^{1/4} X_2^{1/2}$.)

To derive the post-Sraffian atemporal *local* functional relations—which will within each specified triangle prove to be (surprisingly!) *linear*—between Q and $[X_0; X_1, X_2]$, if we wish to we can initially ignore any *breakeven* equations such as those in Sraffa (1960). I will first demonstrate how a purely engineering approach can give my sought for Leontief–Sraffa *linear local* production functions, whose partial derivatives will, as non-spurious marginalisms, cogently pin down distributive real wage rates and Land's rent.

Table 4.1 Atemporal sub-techniques to produce Q from $(X_0; X_1, X_2)$ direct factors of labour, male and female labours

A: 1_0^A of X_0^A & 0_1^A of X_1^A & 0_2^A of $X_2^A \rightarrow Q^A = 1^A$
* * * * *
B: 1_0^B of X_0^B & 1_1^B of X_1^B & 0_2^B of $X_2^B \rightarrow Q^B = 3^B$
C: 1_0^C of X_0^C & 0_1^C of X_1^C & 1_2^C of $X_2^C \rightarrow Q^C = 4^C$
D: 1_0^D of X_0^D & 1_1^D of X_1^D & 1_2^D of $X_2^D \rightarrow Q^D = 5^D$

5 Pure-engineering full-employment Leontief–Sraffa locally linear non-spurious atemporal production functions

Figure 4.1 will usefully diagram the Table 4.1 scenario. When Crusoe (or society) has no positive Labours at all, we begin at the origin marked by A, because only A in Table 4.1 is then useable. A alone produces a paltry 1 of consumable corn. Knowing B and C would then be still not-yet-useable knowledge. At A all the corn harvest goes to landowners' rent.

At any point within the ΔABC, positive Labours now add to society's corn harvest. And in doing so, no longer does rent get all of the product. Society can optimally use all three (X_0; X_1, X_2) at their fully employed levels. When that gets done, rent no longer receives all of the Q^{ABC} product. Do not cry for the 'now-exploited' landowners. Why not? Socialists cried when the addition of capitals for Labour to work with generated a 'profit' or interest return to capitals—a 'vile subtraction' from Land's original deserving rent. Alas, all wrong. Landowners get more when workers sufficiently grow in numbers. However, throughout ΔABC land-rent of y_0^* will still remain at the low 1^A level of unit corn: this for the reason that some of the unit acreage still gets no Labours to work with and all acres must share their paltry rent rate. However, the newly created increment of total corn Q will be awarded competitively to males and females. Awarded equally? No. Table 4.1 shows that males are uniformly less productive than females; so y_1^* for males will be only three-quarters of y_2^* for females.

What ethical preceptor decided that possible violation of St. Thomas Aquinas' 'distributive justice'? The market has no heart and no conscience. Voracious would-be arbitragers by trial and error can clear all market supplies and demands solely at:

$$(y_0^*, y_1^*, y_2^*)^{ABC} = (1^*, 2^*, 3^*)^{ABC} \tag{8}$$

Why that? Because to the knowing eye, one perceives in Table 4.1 that everywhere inside ΔABC:

$$Q^{ABC} = 1^*X_0 + 2^*X_1 + 3^*X_2 \tag{9a}$$

$$\Delta Q/\Delta X_0 = 1^* \text{ an acre's rent} \tag{9b}$$

$$\Delta Q/\Delta X_1 = 2^* \text{ for male Labour's incremental productivity} \tag{9c}$$

$$\Delta Q/\Delta X_2 = 3^* \text{ for female Labour's incremental productivity} \tag{9d}$$

$$Q^{ABC} = \sum_i y_i^* X_i = \sum_i (\Delta Q/\Delta X_i)(X_i) \tag{9e}$$

Before looking for new $\Delta Q/\Delta X_i$ expressions, readers should test their own economist intuitions about comparative statics.

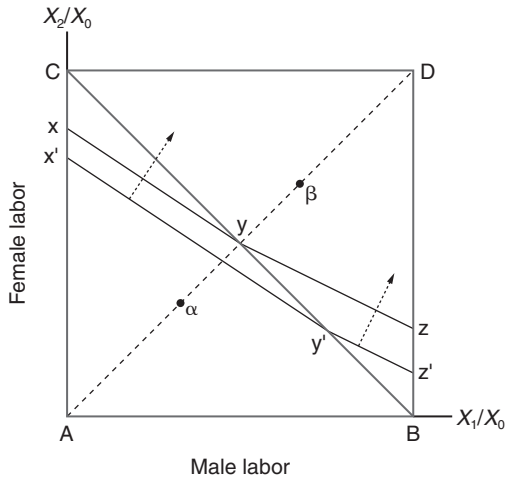


Figure 4.1 Where land with heterogeneous male and female laborers produce corn atemporally by alternative sub-techniques. *Notes:* Near the A origin, when Labour densities per acre of Land are light, full employment of inputs ($X_0 = 1$; X_1, X_2) can take place in, and only in, ΔABC. When populations of X_1 and X_2 crowd each acre further, full employment can be sustained only inside ΔBCD – where D outcompetes A in working with B&C sub-techniques. Inside each triangle, Table 4.1's data do generate linear (!) *non*-neoclassical, non-spurious marginalisms: $Q^{ABC} = 1*X_0 + 2*X_1 + 3*X_2$; and $Q^{BCD} = 2**X_0 + 1**X_1 + 2**X_2$. In *qualitative agreement* with post-Clark neoclassical production functions, these pre-1870 Leontief–Sraffa limited-substitutability functions do comply with 1814 West–Malthus–Ricardo Laws of Diminishing Returns: *ceteris paribus*, $\Delta^2Q/\Delta Q_i^2 \leq 0$, etc. Note that X′Y′ and XY are parallel straight lines when inside ΔABC *and* when inside ΔBCD. Note that the wider space between them in ΔBCD compared to ΔABC confirms Ricardo (1817) and Hicks (1939) diminishing returns: it does take *larger* factor-input increments to generate the *same* ΔQ when factor intensity (vectorally defined) is greater (QED).

1 If both X_1 and X_2 rise while X_0 is constant, what *must* (!) happen to Land's y_0^{**}? Assuredly, if anything, rent must rise.

$$(y_0^{**})^{BCD} \geq (y_0^{*})^{ABC}$$

2 At the same time that y_0^{**} rent rises, it is a safe bet that *at least one* of the real wage rates soon falls. And maybe *both* y_1 and y_2 might fall, as in Table 4.1.

Actually, leisurely perusal of Table 4.1 nominates the marginalist's bet:

$$y^{**} = \Delta Q/\Delta X_1 = (5-1)/(1-0) = 1** < 2* = y^{*} \tag{11a}$$

$$y^{*} = \Delta Q/\Delta X_1 = (5-3)/(1-0) = 2** < 3* = y^{*} \tag{11b}$$

$y_0^{**} = 1^{**}$; $y_1^{**} = 2^{**}$, residually (11c)

A perfect take-home exam paper would deduce for each of ΔABC and ΔBCD the non-spurious locally linear production functions already recorded in Figure 4.1's legend

$$Q^{ABC} = 1*X_0 + 2*X_1 + 3*X_2,\ 0 < (X_1/X_0) + (X_2/X_0) < 1 \tag{12a}$$

$$Q^{BCD} = 2**X_0 + 1**X_1 + 2**X_2,\ 1 < (X_1/X_0) + (X_2/X_0) < 2. \tag{12b}$$

In the large because of technology's convexity, somewhat like revealed preference, there will have to be:

$$0 \geq (\Delta X_0)(\Delta y_0) + (\Delta X_1)(\Delta y_1) + (\Delta X_2)(\Delta y_2). \tag{13}$$

6 Sraffa-type break-even approach to atemporal equilibria

How might a Sraffian foot soldier try to determine the above correct (y_0^*, y_1^*, y_2^*) and $(y_0^{**}, y_1^{**}, y_2^{**})$ Ricardian distributional corn rent and corn wage rates? Armed only with Sraffa's (1960: part III) incomplete weapons, if clever he/she will try to find, for A and B and C—or for B and C and D—three (atemporal!) break-even equations such as the temporal break-even equations in Sraffa (1960: ch. 2).

Bravo! Here is what Table 4.1 does mandate. For ΔABC's interior points, $(X_1/X_0, X_2/X_0)^{ABC}$, with $P_{corn} = 1$ as numeraire:

$$A*\bar{1} = y_0\underline{1}^A + y_1\underline{0}^A + y_2\underline{0}^A \tag{14a}$$

$$B*\bar{3} = y_0\underline{1}^B + y_1\underline{1}^B + y_2\underline{0}^B \tag{14b}$$

$$C*\bar{4} = y_0\underline{1}^C + y_1\underline{0}^C + y_2\underline{1}^C \tag{14c}$$

For ΔBCD's interior points, $(X_1/X_0, X_2/X_0)^{BCD}$, one similarly writes:

$$B**\bar{3} = y_0\underline{1}^B + y_1\underline{1}^B + y_2\underline{0}^B \tag{15a}$$

$$C**\bar{4} = y_0\underline{1}^C + y_1\underline{0}^C + y_2\underline{1}^C \tag{15b}$$

$$D**\bar{5} = y_0\underline{1}^D + y_1\underline{1}^D + y_2\underline{1}^D. \tag{15c}$$

Readers who have persisted with me this far can verify for themselves that only

$$(y_0, y_1, y_2)^* = (1^*, 2^*, 3^*)^{ABC} \tag{16a}$$

$$(y_0, y_1, y_2)^{**} = (2^{**}, 1^{**}, 2^{**})^{ABC} \tag{16b}$$

can clear all markets and kill off arbitragers' profit opportunities for Equations (14) and (15) (QED).

Much as the chicken pox virus can plague an adult's life forever with herpes, Sraffa's early antipathy toward general equilibrium slowed down his progress toward understanding non-spurious marginalisms. The last several paragraphs, with their $\Delta Q/\Delta X_i$ expressions, can perhaps constitute an expositional triumph to convert some borderline post-Sraffians. Skeptical Joan Robinson was a tougher mind, asking: 'Come, come, Samuelson, what can you hold constant when only one of numerous inputs gets varied?' Tables 4.1 and 4.2, with their pedagogically clever useful spray of zeros would only elicit her scornful veto. What she could not be made to understand—at least not by me—is that simultaneous equations do do the same job that those zeroes and ones could do.

Here is the BCD story in Table 4.1 and Figure 4.1, told by my merely solving the three full-employment linear equations for Land, male Labour and female Labour. This version eschews even mention of $\Delta Q/\Delta X_i$ expressions. (Readers can re-tell the ABC story once they do understand this BCD story.)

Any endowment vector, $\hat{X}_0; \hat{X}_1; \hat{X}_2)^{BCD}$ *inside* ΔBCD can be fully employed when each of the following three linear relations is satisfied:

$$\text{Land: } X_0^B + X_0^C + X_0^D = \hat{1} = \hat{X}_0, \tag{17a}$$

$$\begin{aligned} &\text{Male Labour: } X_0^B 1_1^B + X_2^C 0_1^C + X_0^D 1_1^D = \hat{X}_1, \\ &0 < (X_1/X_0) + (X_2/X_0) < 1 \end{aligned} \tag{17b}$$

$$\begin{aligned} &\text{Female Labour: } X_0^B 1_2^B + X_0^C 1_1^C + X_0^D 1_2^D = \hat{X}_2, \\ &1 < (X_1/X_0) + (X_2/X_0) < 2,\ 1 > (X_1/X_0) < 1. \end{aligned} \tag{17c}$$

By subtracting (17b) from (17a), you deduce:

$$X_0^C = 1 - \hat{X}_1 \tag{17d}$$

By subtracting (17c) from (17a), you similarly deduce:

$$X_0^B = 1 - \hat{X}_2 \tag{17e}$$

Residually, then,

$$X_0^D = 1 - [1 - \hat{X}_1 + 1 - \hat{X}_2] = \hat{X}_1 + \hat{X}_2 - 1 \tag{17f}$$

Now the last three output entries on the right of Table 4.1 can show exactly what $Q = Q^B + Q^C + Q^D$ must be:

$$Q^{BCD} = Q^B + Q^C + Q^D \tag{18a}$$

$$X_0^B 3^B + X_0^C 4^C + X_0^D 5^D \tag{18b}$$

$$-3^B[1-\dot{X}_0]-4^C[1-\dot{X}_1]-5^D[\dot{X}_1+\dot{X}_2-1] \tag{18c}$$

$$-2^{**}+1^{**}\dot{X}_1+2^{**}\dot{X}_2 \text{ or } \dot{X}^{**} \equiv 1 \text{ (QED)} \tag{18d}$$

Note that selfish Darwinian competition wiped out any still 'arbitrageable' profits, after the market was indeed led—as if by an Invisible Hand—to the 'maximal linear (non-spurious) first-degree-homogeneous production function' in Equation (18d). Whenever a Table like 4.1 (or like 4.2 to come) involves no visible *ceteris paribus* $\Delta Q/\Delta X_i$ experiments, that is of no consequence at all. Simultaneous equations à la (17) and (18) above generically generate the locally linear non-spurious Sraffian marginalisms.

Why bother to supplement the engineering approach by its equivalent Sraffa-type break-even approach? The main reason was to prepare readers for the temporal heterogeneous Ks cases to come. For them, as will be shown, Sraffa (1960)-type 'missing break-even equations' do generically fail to exist. Only in singular scenarios will his defined stationary states generate equality between Wheat's 'own rate of interest', r_1^*, and Iron's 'own rate of interest', r_2^*. Instead of a 'missing' equation, Sraffians will be faced with one break-even equation too many! Sad. But that is the way the cookie crumbles.[4]

7 Temporal heterogeneous capitals relate how to the atemporal $Q(t) = F[X_0(t); X_1(t), X_2(t)]$ model?

Mr. Etula has produced for me the following Leontief–Sraffa Table 4.2, whose likenesses and differences with atemporal Table 4.1 will become apparent to diligent readers.

Table 4.2 can provide for Sraffa (1960: part III), alternative sub-techniques that are known ways to produce gross Wheat output: call them **a**, **b**, and **c**. And it likewise postulates as known **A**, **B** and **C** alternative ways to produce gross Iron. For simplicity, Table 4.2 involves no joint products. Instead it has only

Table 4.2 Alternative ways for labour & wheat & iron inputs at t to produce at t + 1 wheat & iron gross outputs

Wheat:	**a**	$\underline{1}^a$ of $L(t)^a$ & $\underline{0}_1^a$ of $K_1(t)^a$ & $\underline{1}_2^a$ of $K_2(t)^a \rightarrow Q_1(t+1)^a = \overline{4.2}^a$
	b	$\underline{1}^b$ of $L(t)^b$ & $\underline{0}_1^b$ of $K_1(t)^b$ & $\underline{2}_2^b$ of $K_2(t)^b \rightarrow Q_1(t+1)^b = \overline{5.3}^b$
	c	$\underline{1}^c$ of $L(t)^c$ & $\underline{0}_1^c$ of $K_1(t)^b$ & $\underline{3}_2^c$ of $K_2(t)^c \rightarrow Q_1(t+1)^c = \overline{6.35}^c$
Iron:	**A**	$\underline{1}^A$ of $L(t)^A$ & $\underline{1}_1^A$ of $K_1(t)^A$ & 0_2^A of $K_2(t)^A \rightarrow Q_2(t+1)^A = \overline{4.2}^A$
	B	$\underline{1}^B$ of $L(t)^B$ & $\underline{2}_1^B$ of $K_1(t)^B$ & 0_2^B of $K_2(t)^B \rightarrow Q_2(t+1)^B = \overline{5.3}^B$
	C	$\underline{1}^C$ of $L(t)^C$ & $\underline{23}_1^C$ of $K_1(t)^C$ & 0_2^C of $K_2(t)^C \rightarrow Q_2(t+1)^C = \overline{6.35}^C$

'circulating capitals', $K_1(t)$ and $K_2(t)$, that are used up at t and must be replaced at t + 1. Any excess of $Q_i(t + 1)$s above needed $K_i(t + 1)$ to equal $K_i(t)$ will be positive final consumption of Wheat or Iron, namely, $C_i(t + 1)$. By dimensional convention, I keep Labour's L always at unity:

$$L_1(t) + L_2(t) \equiv L(t) \equiv 1 \equiv L(t+1) \tag{19a}$$

As in Sraffa (1960: part I), readers can here *at first* assume that there is known only a *single* way of producing Wheat and a single way of producing Iron: say, **a&A**; or **a&B**; or.… In atemporal Table 4.1, when but *one* intertemporal technique had been known, distributive pricing was seen to be indeterminate. So it is here too in the temporal scenarios. Sraffa puts the matter nicely: we then face a 'missing equation'.

To coordinate with Sraffa's (1960: 11) price = costs exposition, I duplicate the **a&A** numerical data from Table 4.2 and write out Sraffa's two break-even equalities, which ensure that real prices, P_1/W and P_2/W, do exactly equal real unit costs calculated as the sum of input costs—$L(t)\&K_1(t)\&K_2(t)$ costs, where outlays on each of the Ks do always earn the same (safe!) rate of *interest* or *profit*, r:

$$\begin{aligned} &4.[illegible]\,P_1 = [illegible]\,W + [illegible]\,P_1(1+r) + [illegible]\,P_2(1+r) \\ &4.2[illegible]\,P_2 = [illegible]\,W + [illegible]\,P_1(1+r) + [illegible]\,P_2(1+r) \end{aligned} \tag{19b}$$

Equations (19b) are manifestly but *two* equations in *three* unknowns: $(P_1/W, P_2/W; r)^*$. If a little birdie told us the true equilibrium value for any one of the three—say for r^*, or for one of $(P_j/W)^*$—then we Sraffians would face no 'missing equation' and could calculate (19b)'s possible distributive pricings.

8 Digression on 'a way not taken': Böhm–Fisher–Ramsey's intrinsic impatience time-preference to define missing equation(s)

For whatever reason, 1925–83 Sraffa revealed a general distaste for relying on subjective demand-tastes variables. None of his 1960 words relates to the classical and neoclassical objectively observable propensity of ordinary humans who may prefer a half loaf today to two loaves next year. By contrast, Irving Fisher or Pigou or Ramsey—or for that matter Nassau Senior or Böhm-Bawerk—usefully proposed scenarios where the typical family acted systematically as if it objectively applied, say, a five percent exponential per period discount parameter, $1/(1 + \delta)^T$ = say $1/1.05^T$ discount factor to all economic metric values pertaining to T periods ahead in the future.

Then voila!, with the stroke of the pen, we have located the missing equation:

$$1 + \delta = (1 + r)^* = 1.05,\ r^* = \delta = 0.05 \tag{19c}$$

We eclectic Sraffians, therefore, can put this (1 + r)* into Equations (19b) above. At sight Equations (19a) and (19b) enable one to write out for Ricardo *all* his needed competitive distribution parameters:

$$1 + r^* = 1 + \delta = 1.05^*,\ r^* = 5 \text{ percent per period} \tag{19d}$$

Solving (19b) one deduces Ricardo's trade-offs:

$$\begin{matrix} (W/P)^* = 3.2 - r^* = 3.2 - \delta; \\ = 3.20 - 0.05^* = 3.15^* = (W/P_1)^*; \end{matrix} \tag{19e}$$

(The singular equality of real Iron wage rate and Wheat wage rate is of course solely due to the singular symmetries posited in Table 4.2.) Here I have followed Sraffa's convention of letting W/P_j stand for the real wage paid *post factum* to workers, at t + 1 and not at t. Classical savants thought it more realistic to have rentiers 'advance' to workers their wage at time t. And, of course, on such advances rentiers would insist on the same r* interest rate as is earnable on *all* of their non-wage investment outlays.

Instead of plucking exogenous δ out of the air, a sage Modigliani could utilize his excellent life-cycle saving scenario, where supply and demand between (1) retired folk of all ages and (2) working-age folk of all ages, would just balance out at a market-clearing r_1^*. In such a special model a society could even be a strictly egalitarian classless society. (Also, there could be multiple equilibria.)

See Mathematical Appendix for a generalization of Ramsey's (1928) *scalar* capital flow model of optimal saving to Leontief–Sraffa discrete-time paradigms of *heterogeneous* capitals. For positive or zero δ, it is differences between r_1 and r_2 that get wiped out in the asymptotic final dynamic equations where $r_1 = \delta = r_2$.

9 Piero's preferred way

Sraffa (1960: part III) went some limited steps toward seeking missing equations by another route—namely, by combining a triad such as **a&b&A** or **b&A&B** sub-technologies. Briefly, too briefly, I will presently sketch here how use of *four* sub-techniques simultaneously—say **a&b&A&B** or **b&c&A&B**—could generate *non*-neoclassical marginalisms that are definitely non-spurious and that do maximize permanent levels of final Wheat or final Iron. To do this in a few limited words will force me to temporarily only sketch some genuine stationary-state subtleties.[5]

Table 4.2's data do not tell their own story. Those technological data, *when augmented by exogenous demand-tastes data* of several different contemplated Robinson Crusoes, can be shown to lead to quite different alternative post-Sraffian distributions-of-income equilibria.

Consider a Crusoe who wants only Wheat as a final utility good. That is but the first of many different possible patterns of taste. He of course differs from a second Crusoe who wants only Iron as a final good.

A third demand pattern worth exploring could be for a Crusoe who, à la J.S. Mill (1848), always spends any of his income fifty–fifty percent on the two goods. Or spends two-thirds on Wheat and one-third on final Iron; or spends one-third on Wheat.… A fourth demand pattern could be for a Crusoe who has symmetric *linear* utilities. He would allocate his unit L = 1 optimally among his (K_1, K_2) input endowments so as to maximize $C_1 + C_2$ consumptions.

A fifth demand pattern is for a Crusoe who seeks as final consumption a fixed dose of both Wheat and Iron. His cornered utility function could be, say, Min[C_1, C_2]. For him 3 of Wheat and 3 of Iron would be indifferent to 3 of Wheat & 300 of Iron; and be indifferent to 300 of Wheat & 3 of Iron. Almost certainly, given any flexibility of input allocation, this Crusoe will equate consumptions for Wheat and Iron: $C_1 = C_2$.

Figure 4.2, which is perhaps the most important part of this article, does present diamond quadrilaterals near the main diagonal that apply Table 4.2's sub-technologies to a Crusoe with the above fifth pattern of equal dose Iron–Wheat tastes. I accepted Erkko Etula's nomination of this pattern, because it is in a genuine sense the demand pattern *most different* from the post-1870 *neo-classical* differentiable utilities of Jevons–Walras–Menger. Also, it does best utilize the simplifying skew symmetries of Table 4.2 and Figure 4.2. However, the $C_2 \equiv 0$ case is perhaps the easier one to talk about initially.

10 The purely engineering equations of competitive equilibrium

The competitive auction market has no mind; no heart; no will. What drives it is the selfish desire of input owners to end up with most possible command over Wheat and Iron outputs. In stationary equilibrium solely when four sub-techniques come into use simultaneously will nothing be left on the table for eager myopic arbitragers to scoop up?

What I am sketching is what a second edition of Sraffa (1960) might have included in a new part III or IV. Generically, two heterogeneous capitals achieve maximal permanent outputs of goodies only when *four* viable sub-techniques get used. With techniques feasibly adjusted to the exogenous endowment vector, supply and demand market clearing will mandate that unit supply of L gets divided into (L^a, L^b, L^A, L^B) uniquely so as to leave none of the three inputs (L = 1, K_1^e; K_2^e) *unemployed* while at the same time consumers' spending evokes the gross Qs that permit maximal desired Cs.

I now spell out here the determining linear equations, necessary and sufficient, for characterizing competitive distribution equilibrium for 1750–1870 classical regimes and 1960–2007 non-neoclassical regimes. Figure 4.2's α point inside a′bAB has exact (K_1/L, K_2/L) coordinates of $(0.3, 1)^{\alpha}$. For β, coordinates are $(0.5, 1.5)^{\beta}$. Side by side, here are the respective four linear relations:

For α:

$$L^a + L^b + L^A + L^B = 1$$

For β:

$$L^b + B^c + L^A + L^B = 1,$$

L fully employed (20a)

$$[illegible] \quad (20b)$$

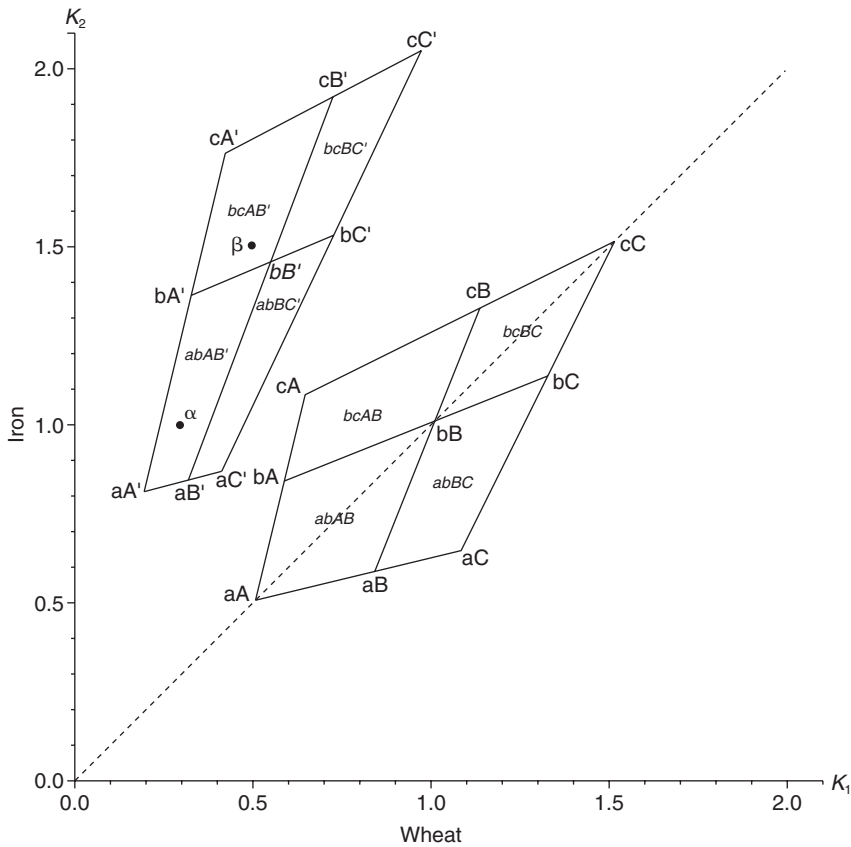
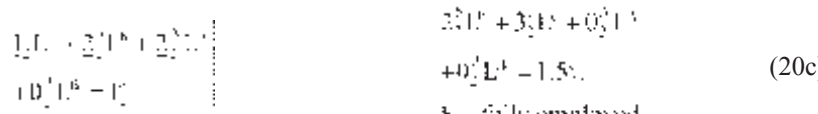


Figure 4.2 Where heterogeneous wheat and iron are produced by themselves and labour. *Notes*: The four diamond-shaped quadrilaterals northwest of the main diagonal do map *all* the full-employment endowments that can sustain stationary equilibrium under Table 4.2's known technological data when Wheat is Crusoe's *sole* desired final consumption good: $C_1(t + 1) > 0 \equiv C_2(t + 1)$. When Crusoe's demand tastes have changed so that C_1 and C_2 are to be equal and be maximal, Table 4.2's data will generate the four diamonds near the main



$$\overline{4.2}^{A}L^{A} + \overline{5.3}^{B}L^{B} = 1^{e}_{2}\overline{4.2}^{A}L^{A} + \overline{5.3}^{B}L^{B} = 1.5^{e}_{2}, C_2(t) \equiv 0 \tag{20d}$$

Solved out by any of many elementary substitutions, and after the fractional Ls are entered into Table 4.2's appropriate rows, one finds spelled out the two locally linear Leontief–Sraffa production functions reported in Figure 4.2's lengthy legend. At last, Joan Robinson's query: 'When you claim to measure $\Delta C_1/\Delta L$ or $\Delta C_2/\Delta K_i$, what variables are you controlling in your alleged *ceteris paribus*; and which variables are varying?' I write out the appropriate answer for her:

For abAB endowments:

$$C_1 + 1*C_2 = 3.1*L + 0.1*K_1 + 1*(0.1)*K_2 \tag{20e}$$

$$\partial C_1/\partial L \equiv \Delta C_1/\Delta L = 3.1* = (W/P_1)*, \text{ real Wheat wage} \tag{20f}$$

$$\partial C_1/\partial K_1 \equiv \Delta C_1/\Delta K_1 = 0.10* = \text{own Wheat interest rate } r^*_1 \tag{20g}$$

---

diagonal: only inside those four are the fully and permanently employed endowment points.

To understand Leontief–Sraffa non-spurious marginalisms, it will suffice to contemplate just two of the eight diamonds: say, a point like α in a′bAB; and a point like β in adjacent b′cAB. At each such point, stationary maintained equilibrium is reached by mindless avaricious Darwinian competitors. Equilibrium is reached only where the four allocated Labour fractions achieve permanent full-employment of the three total inputs; and satisfy also Crusoe's objectively specified $C_1 > 0 \equiv C_2$ demand conditions. Equations (20), simple linear equations, do suffice to determine unique $(L^a, L^b, L^A, L^B)^*$ fractions for α; and for β, unique $(L^b, L^c, L^A, L^B)^{**}$ fractions. Entering such a known foursome into the indicated rows of input and output numbers in Table 4.2, we do end up with non-spurious marginalisms: $(C_1 + \pi^*C_2)^{abAB} \equiv (C_1 + \pi^*0) = (C_1 + [P_2/P_2]^*0) = (C_1 + 1^*0) = \rho^*_0L + \rho^*_1K_1 + \rho^*_2K_2 = 3.1^*L + 0.10^*K_1 + 0.10^*K_2 \equiv (W/P_1)^*L + r^*_1K_1 + \pi^*r^*_2$. Here $\pi^* \equiv 1$ is due solely to singular (!) skew symmetry. *Generically* at β (the more interesting case), $(L^b, L^c, L^A, L^B)^{**}$ translated into Table 4.2 ends us up with $(C_1 + 1.03^{**}0)^{bcAB} = 3.2^{**}L + 0.1355^{**}K_1 + 1.03^{**}(0.0172)^{**}K_2 \equiv (W/P_1)^{**}L + r^{**}_1K_1 + (P_1/P_1)^{**}r^{**}_2$.

As expected, more of both K_1/L and K_2/L does raise real wage from 3.1* to 3.2**. Instead of lowering both r^{**}_1 and r^{**}_2, going to β from α does lower Iron's own interest rate from 10 percent per period to 1.72 percent per period. However, as economists' intuition permits, Wheat's own interest rate actually rises from ten percent to 13.55 percent at β. To test and confirm comprehension, readers can put their own new α′ and β′ points in any other adjacent diamonds, thereby deducing similar comparative statics.

The moral of Table 4.2's tale is that generically r^*_1 and r^*_2 will differ (QED).

$$-\partial C_1/\partial C_2 = -\Delta C_1/\Delta C_2 = 1^* = (P_2/P_1)^* \tag{20h}$$

$$\partial C_2/\partial K_2 = \Delta C_2/\Delta K_2 = r_2^* = (\Delta C_1/\Delta K_2)\pi^* \tag{20i}$$

The point that needs stressing is how very much demand tastes do matter. A secondary point is important too: a pro-Sraffian, who believes strongly that the world empirically has but few viable alternative sub-technologies, ought to expect distributive shares to jump around volatilely in a way that econometricians do not find to occur. Bowley's Law of fairly constant distributive shares is a reported econometric approximation, but even the systemic trends away from it do seem remarkably stable.

11 Epilogue

I had hoped on this occasion to provide a fairly complete synthesis of the Master Function methodology of recent Samuelson and Etula (2006a,b,c) and Etula papers in the pipeline. However, resistance from various journals' peer reviewers slowed down the publication programme, as did compulsory service in the Finnish army for MIT–Harvard graduate student Erkko Etula.

My final words are directed toward the unusual phenomenon of a lone autodidactic researcher who for a third of a century occupied his limited spare time toward one grand original purpose. In the annals of the many corners of science and scholarship, there are at least a few such known cases. Far fewer though are the subset who did in the end succeed in adding significantly to posterity's canon of agreed-upon wisdoms.

Piero Sraffa has been a notable case in point. From age 27 years onward, he became preoccupied with the complexities of intertemporal capital theory. This began before he was singled out by the Royal Society to compile the definitive editions of David Ricardo's papers. The Great Depression and Second World War (during which he was interned in Britain as an alien from an enemy country) interrupted and slowed down his major theoretical research programme. But still he persisted.

The great Albert Einstein offers some limited parallelisms. From 1905 to 1925 again and again Einstein initiated revolutions on many different physics fronts: special relativity, Brownian motion, post-Planck quantum physics, general relativity (this latter crowned him as successor to Isaac Newton himself!).

And then, almost as an anti-climax, in the last third of his life, his past-earned self-confidence led him away from the mainstream of 1930–2007 physics. Tirelessly, and with able young collaborators, Einstein pursued his own paths to try to unify relativity and quantum theory. The little progress he made turned out to be definitely not in the direction that actual living physics was going. His was a gigantic struggle, but also in it there was a definite element of pathos. His attempted refutations (addressed to Niels Bohr) of an ultimate probability basis to physical laws involved ingenious thought experiments—virtually *reductio ad*

absurdum experiments. Alas, precisely what he expected readers to regard as ridiculously implausible are today's well-documented 'entanglement' phenomena that may generate the future's miniature powerful computers.[6]

One thinks too of Ramanujan, the poor and tubercular Madras clerk who was discovered through the mail by Trinity College's great mathematician, G.H. Hardy. Together they made beautiful transcendental music that Hardy could not possibly have done alone. In a burst of romanticism, Hardy once hypothesized that Ramanujan's environmentally induced lack of much of established maths freed his mind to soar where the academy never dreamed of. However, later, in a more sober moment, Hardy recanted, admitting in effect: 'How much more glorious Ramanujan's accomplishment could have been had he had the good health and full advantages of a superior training in all of modern mathematics'.

By temperament, Piero Sraffa preferred to originate in his own way. Help he did get from Frank Ramsey. (One 1928 note someone sent me from Ramsey to Sraffa sketched out matrix equations of not-yet-discovered Dantzig (1963) linear programming and Kuhn–Tucker non-linear concave programming.) Cambridge's great mathematician Besicovich also was an acknowledged helper. But it was learned from Mrs. Besicovich how frustrating it was to give help to a friend who never fully revealed what his targets were. In my small way (and long before I learned only at the 1958 IEA Corfu meeting that Sraffa was about to publish a book on capital theory), I would often say in talking with him things like: 'All you need for this are the Hawkins and Simon (1949) determinant inequalities to assure a surplus economy'. He brushed aside any such prattle about this and other well-known Kuhn–Tucker concave programming dualities or Richard Bellman intertemporal generalizations of the calculus of variations. I am sure he never cracked the pages of the Dorfman *et al*. (1958) book that I sent him. Understandably, he wanted to do his way whatever he was to do.

I respect and salute Piero Sraffa. He added colour, but beyond colour he did add light to the not-so-dismal science of economics.

Like Pliny the Younger of Rome, I have to apologize for the lengthiness of this analysis on the grounds that I lacked the time to make it shorter.

Acknowledgements

Without Harvard–MIT student Erkko Etula, I could not have written this paper. Were it not for the need to make so many Sraffa–Samuelson *ad hominem* remarks, this could have been a joint Samuelson and Etula article. His laptop computer ground out the many diagrams and numerical wages and rents and input allocations. Editor Janice M. Murray triumphed over a many-time revised, untidy manuscript. As usual, Robert Solow pruned errors from my scribbles. All imperfections have been mine alone and inevitably their number will not be zero. All the definite faults herein trace to my own logical and empirical imperfections.

Mathematical appendix

Post-Ramsey (1928) optimal saving for heterogeneous capitals: Neoclassical technologies and Leontief–Sraffa limited-substitutability technologies

Ramsey (1928) used the following kind of model for Robinson Crusoe's optimal time profile of 'abstaining-saving':

$$\text{Max}_{K(t)} \int_{0}^{\infty} e^{-\delta t} U\{F[K(t)] - \alpha K(t) - K'(t)\}, \; \delta \geq 0, \alpha > 0 \tag{A1}$$

where α is a positive durability-of-K parameter and where positive δ measures Crusoe's objectively observable 'impatience' or 'systematic time preference' parameter.

Were Crusoe's δ to be zero, starting from low initial K(0), he would opt to 'abstain' from some current C(t) in order to attain growth in K(t) toward the optimal $K(\infty)^g$ 'golden-rule K^g,' which will give him permanent (perpetual) maximal $C(\infty)^g$. In agreement with Schumpeter's (1912) heuristic conjectures, at $K(\infty) = K^g$, the safe interest rate $r(\infty)g = 0$ will denote euthanasia of the rentier capitalists—until some new Schumpeterian innovations come along.

For Crusoes with positive δ impatience, less is saved in initial and later years, so that the ultimate $[K^\delta, C^\delta]$ asymptote is accordingly lower. K^δ and r^δ each relate *inversely* while r^δ grows with δ. I skip Ramsey's cogent proofs.

Ramsey (1928) used a neoclassically differentiable concave F[ ] production function with *scalar* K. Samuelson and Etula (2006b) also dealt with scalar K but in the Leontief–Sraffa context of *limited substitutability*. I omit the cogent proofs and explications to leave room here for the scenario with *heterogeneous* Wheat and Iron capitals.

Both for the differentiable neoclassical technology and the Leontief–Sraffa limited substitutability technology, I begin with the following definable Master Function. It is for a Crusoe whose objective demand tastes are for Wheat only as a final good—for $C_1(t + 1)$. For Crusoe, always $C_2(t + 1) \equiv 0$. Therefore:

$$C_1(t + 1) = -K_1(t + 1) + F[L(t); K_1(t), K_2(t); K_2(t + 1) + 0] \tag{A2a}$$

$$\equiv M[K_1(t), K_2(t); K_1(t + 1), K_2(t + 1)], \text{ for } L(t) \equiv 1 \tag{A2b}$$

$$\partial M/\partial K_i(t) > 0 > \partial M/\partial K_i(t + 1), \; i = 1, 2. \tag{A2c}$$

Equations (A2) hold both for neoclassical functions like Cobb–Douglas, or for any Leontief-Sraffa technology like that in Table 4.2, where their M function will lack two-sided partial derivatives on definable boundaries of regions in the $[K_1/L, K_2/L]$ two-dimensional plane.

This appendix's ultimate purpose is to deduce that almost all $[K_1/L, K_2/L]$ stationary states will generate *unequal* 'own-Wheat and own-Iron rates of interest.' That is:

$$\partial K_1(t+1)/\partial K_1(t) = 1 + r_1 \neq 1 + r_2 = \partial K_2(t+1)/\partial K_2(t). \qquad \text{(A3)}$$

Our Crusoe seeks to maximize over an infinite lifetime, from t = 0 to t = ∞, the present value of all his future *discounted* concave utilities:

$$\text{Max} \sum_{t=0}^{\infty} [1+\delta]^{-t} U[C(t+1)], \; U'[\;] > 0 > U''[\;] \qquad \text{(A4a)}$$

$$\text{Max} \sum_{t=0}^{\infty} [1+\delta]^{-t} U\{M[K_1(t-1), K_2(t-1); K_1(t), K_2(t)]\}; \text{[illegible]} \qquad \text{(A4b)}$$

For this infinite sum κto be maximal, Crusoe must for every T pick [$\mathbf{K}_1(\mathbf{T})$, $\mathbf{K}_2(\mathbf{T})$] to optimize the sum of the following two adjacent expressions:

$$\underset{K_1(T),K_2(T)}{\text{Max}} \cdots + [1+\delta]^{-T} U\{M[K_1(T-1), K_2(T-1); K_1(T), K_2(T)]\} \qquad \text{(A4c)}$$

$$+[1+\delta]^{-T-1} U\{M[K_1(T), K_2(T); K_1(T+1), K_2(T+1)]\} + \cdots \qquad \text{(A4d)}$$

$$= \text{for short } \underset{K_1,K_2}{\text{Max}} [1+\delta]^{-T} \Phi(K_1, K_2; K_1, K_2; \overline{K}_1, \overline{K}_2]. \qquad \text{(A4e)}$$

A necessary condition for such a maximizing ‘extremal’ path is that:

$$\{\partial / \partial K_i(T)[1+\delta]^{-T} \Phi(\underline{K}_1, \underline{K}_2; K_1, K_2; \overline{K}_1, \overline{K}_2) = 0, \quad i = 1, 2 \qquad \text{(A4f)}$$

For i = 1, 2, Equation (A4f) boils down after a cancellation of the $[1 + \delta]^{-T}$ factor to:

$$1+\delta = \frac{U'\{M[K_1(T), K_2(T); K_1(T+1), K_2(T+1)]\} M_i[K_1(T), K_2(T); K_1(T+1), K_2(T+1)]}{U'\{M[K_1(T-1), K_2(T-1); K_1(T), K_2(T)]\} M_{2+i}[K_1(T-1), K_2(T-1); K_1(T), K_2(T)]} \qquad \text{(A5a)}$$

Recall that Equation (A5a) uses the subscript notation:

$$\partial f(x_0, x_1, \cdots, x_N) / \partial x_i = f_i(x_0, x_1, \cdots, x_N), \; i = 0, 1, \ldots, N \qquad \text{(A5b)}$$

Equation (A5a) gives twentieth century Kuhn–Tucker–Bellman extremal conditions that are analogous to eighteenth century Euler–Lagrange extremal conditions for standard calculus of variations problems.

In Crusoe’s asymptotic terminal stationary state, the U′ { } terms in Equation (A5a) will cancel out, and the equations become:

$$1+\delta = -\frac{M_i[K_1^*, K_2^*; K_1^*, K_2^*]}{M_{2+i}[K_1^*, K_2^*; K_1^*, K_2^*]}, \; i = 1, 2 \qquad \text{(A6a)}$$

$$= \partial K_1(t+1)/\partial K_1(t) - 1 + r_1 - 1 + 0 \text{ (QED)} \tag{A6b}$$

The dynamic generalized vectoral 'deepening of capital' story deduced here for both neoclassical and Leontief–Sraffa heterogeneous (!) capitals thus does affirm precisely what Joan Robinson used to deny as mere trickle-down flap-doodle capitalistic apologetics. She expected only the likes of Nassau Senior, Eugen von Böhm-Bawerk, A.C. Pigou and Irving Fisher to believe such nonsense. I invite Sraffian friends to audit unmercifully these Ramsey–Samuelson syllogisms as applied to Robinson (1956) and Sraffa (1960).

This appendix will be complete once I deduce generically that only a razor's edge of [K_1/L, K_2/L] endowments can generate *equal* 'own Wheat' and 'own Iron' interest rates.

First the easy, old-hat, neoclassical case. Consider the following generic specimen of a neoclassical Master Function:

$$C_1(t+1) + K_1(t+1) + 0 + K_2(t+1) = L(t)^{\frac{1}{2}} K_1(t)^{\frac{1}{2}} + L(t)^{\frac{3}{4}} K_2(t)^{\frac{1}{4}} \tag{A7a}$$

This specimen happens to involve the kind of Sraffa (1960: part II, III) joint production of Wheat and Iron by the same L(t).

For L(t) ≡ 1, Equation (A7a) becomes:

$$C_1(t+1) + K_1(t+1) + K_2(t+1) = K_1(t)^{\frac{1}{2}} + K_2(t)^{-\frac{1}{4}} \tag{A7b}$$

$$\therefore \frac{\partial K_1(t+1)}{\partial K_1(t)} = \frac{1}{2} K_1(t)^{-\frac{1}{2}}, \quad \frac{\partial K_2(t+1)}{\partial K_2(t)} = \frac{1}{4} K_2(t)^{-\frac{3}{4}} \tag{A7c}$$

$$\frac{1}{2} K_1(t)^{-\frac{1}{2}} \neq \frac{1}{4} K_2(t)^{-\frac{3}{4}}, \text{ generically.} \tag{A7d}$$

However, for what singular endowment point (K_1^e/L, K_2^e/L) will equality of own rates, $r_1 = r_2$, be possible? My post-Ramsey dynamics mandates solving for:

$$\frac{1}{2} K_1(t)^{-\frac{1}{2}} = \frac{1}{4} K_2(t)^{-\frac{3}{4}} \tag{A7e}$$

Therefore, equal r's can occur only on the razor's edge:

$$(K_1^e/L) = 4(K_2^e/L)^{\frac{3}{2}} \tag{A7f}$$

For a similar proof of inequality of own rates when technologies are of limited substitutability, any motivated Sraffian can specify at random **a&b&C&D** alternative numerical known ways of producing permanently positive C_1 and C_2.

Generically, this will define locally the following (linear!) Sraffian production function:

$$\begin{aligned}&[C_1(t+1)+K_1(t+1)]+\pi^*[C_2(t+1)\\&+K_2(t+1)] = b_0^*L(t)+b_1^*K_1(t)+b_2^*K_2(t)\end{aligned} \quad \text{(A8a)}$$

If, and only if, the following singular equality holds, will:

$$b_1^* = b_2^* / \pi^* \quad \text{(A8b)}$$

$$= (1 + r_1)^* = (1 + r_2)^* \quad \text{(A8c)}$$

In the generic case, almost never will this happen. Table 4.2, contrived for me artfully by Erkko Etula's L.P. Dantzig programme, exhibits in Figure 4.2 four diamond-shaped regions: two diamonds, e.g. the ones that surround the 45° diagonal in the (K_1/L, K_2/L) space, do exhibit equality $r_1^{abAB} = r_2^{abAB}$ because of imposed skew-symmetry. However, inside the other two diamonds, symmetry is broken and (generically):

$$b_1^* \neq b_2^* / \pi^* \quad \text{(A8d)}$$

Note that changing every Table 4.2 coefficient at random by ever so little as $+\frac{1}{-100}$ will generically negate (A8c)'s singular equality (QED).

What can suffice to defang differences in own rates

It could be the case that a rational Robinson Crusoe systematically applies a δ_1 impatience parameter for Wheat consumptions *different* from his δ_2 impatience parameter for Iron consumptions.

Specifically, replace Crusoe's (A4) by the following:

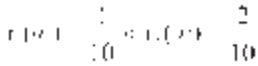
$$\text{Max}\sum_0^t \left[1+\frac{1}{10}\right]^{-t} U_1\{C_1(t+1)\} + \sum_0^t \left[1+\frac{2}{10}\right]^{-t} U_2\{C_2(t+1)\} \quad \text{(9a)}$$

The simplest example to explicate the point could be the following Leontief–Sraffa or neoclassical Master Function that holds when each Q_j and C_j uses only itself as an input along with a fixed-supply Labour specific only to it: say female Labour, $L_1 = 1$ for Wheat and male Labour, $L_2 = 1$, for Iron.

This implies the following Master Function:

$$Q_1(t+1) + Q_2(t+1) = F^1[K_1(t)] + F^2[K_2(t)] \quad \text{(9b)}$$

Were this to obtain, then in Crusoe's ultimate steady state, he will end up with:



(9c)

Applying this loophole to the 1776–2006 time preference literature, I can contrive legitimacy for *any* (K_1/L, K_2/L) in a dense region as a stationary state.

From the standpoint of behavioural economics introspection, why could not my time preference for Wheat and for Iron consumptions significantly differ? Most people's time preference for, say, dancing probably does exceed their time preference for jogging or doing the dishes (QED).

Abstract

Proofs are given that only singularly can real 1750–2007 competitive price ratios be 'natural', in the sense of being invariant under changes in demand tastes. Proofs are given that both 1750–1870 discrete technologies or 1890–2007 continuum technologies, *with convexity properties sufficient for arbitrage-proof supply-demand equilibria*, will be 'intertemporally Pareto optimal', immune to leaving any deadweight (inefficient) losses on the table. Sraffa (1960), ignoring the vast post-1945 linear and non-linear programming mathematical literature of Danzig, Kuhn–Tucker–Bellman, von Neumann, Ramsey literature does not quite arrive at attainable distribution solutions. Where it tolerates *increasing* or *decreasing* returns to scale, there can be no *competitive* equilibria. When its matrix equations do obey first-degree-homogeneous functions, the book's stress on Basics or non-Basics is an irrelevancy leading to bizarre novel interpretations of Ricardo.

Old age overtakes us all. Alas, Sraffa's proposed critique of twentieth century political economy we will never be able to know.

Keywords

Non-spurious marginalisms for limited-substitutability or smooth differentiable technologies, 'Master Functions' (cornered or smooth), scales-return constancy for competition, generic *inequality* of *own rate of interest*!

Notes

1 It was I who had advised the Cambridge Press to definitely publish an American edition of the 1960 Sraffa classic. It tells us something about the vagaries of fashion in a science's evolution that demand for Sraffa (1960) has by now so dried up as to force it out of print.

2 Early on I would shift conversations away from present-day mixed economies. She had become impatient with the Senior–Böhm–Fisher view that, even in the absence of Schumpeterian innovations, generalized accumulation of capitals by motivated saving decisions to sacrifice some of today's consumption in trade-off for more permanent future consumption could raise real wage rates while lowering safe interest rates. She dismissed that as leak-down flap-doodle concocted by apologists for capitalism. Successively, she came to admire Leninism-Stalinism, Castroism, Maoism and in the end

North Koreanism. Therefore, I would shift analytic discussion away from contemporary economic history. Innocently, I would ask: 'Joan, how should Mao act to elevate China's real per capita incomes?' Without hesitation, she would reply: 'First he must select the investment projects with the highest relative yields. That done, go on to further projects with lower yields.' Sweet it was to be able to agree on some things. Neither Joan nor Piero ever bothered to rebut in print the pre-1935 neoclassical versions of capital deepening of Ramsey (1928). When I challenged her in this regard by describing single K—contemptible 'Leets'—she lost interest. Nor did my citing of the heterogeneous Ramsey-type neoclassical scenario in Samuelson and Solow (1956) or Samuelson (1960) pique her interest.

3 Suppose Sraffa lets Smith posit *increasing* scales returns for beaver: $q_1 = L_1^2$ say. And let him posit *decreasing* scales returns for deer: $q_2 = \sqrt{L_2}$ say. From that quagmire no 1776 or 1926 or 2007 economist can infer a coherent or plausible competitive P_i/P_j formula.

4 See Mathematical Appendix, which among other things does correct some remarks in Samuelson and Etula (2006a) alleging necessary equality of own rates of interest.

5 Sraffa (1932), in his polemic against Hayek (1931), importantly originated consideration of 'own rates of interest in Wheat,' r_1^*, and 'own rates in Iron,' r_2^*. Keynes (1936: ch. 17) comments on this somewhat obscurely, as pointed out in Pigou (1936) and Samuelson (1937, 1939). Generically, for most exogenous (K_1/L, K_2/L) endowments, $r_1^* \neq r_2^*$! So to speak this serves as a signal for the system to *leave* the stationary state and proceed with generalized Ramsey (1928) dynamics. See the present Mathematical Appendix that handles for Ramsey heterogeneous capitals produced over finite discrete time periods, t and t + 1.

6 Richard Feynman, Einstein's worthy successor, has termed 'entanglement' as the *essential* weirdness of quantum theory; weird and inexplicable but irrefutably present.

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5 Samuelson's misses

A rejoinder*

Pierangelo Garegnani

1 Introduction

Readers will probably have joined me in being disappointed at the lack of stringency in the Reply to my *Samuelson on Sraffa and the Classical economists*—henceforth, respectively, Samuelson (2007) and Garegnani (2007)—published jointly in the last issue of this Journal. The antipathy for adversary procedures in scientific discourse advanced by Samuelson (2007: 243) has in effect resulted in a failure to comment on the specific criticisms of his work raised in my article. Thus, in the course of this Rejoinder we shall notice that no answer is given to my rebuttal of the necessity of constant returns for the validity of Sraffa's and the classical economists' theory of prices and, connectedly, to my denial of a dependence of their theory on demand and supply on neoclassical lines.

But also missing are the answers on two other basic issues raised in my article. The first concerns the classical analysis of non-wage income in terms of the difference between the output and the consumption necessary to its production—in terms, that is, of the social surplus often indicated as a characteristic feature of Ricardo, and the classical economists: the question at the basis, for example, of Sraffa's interest in a 'standard commodity', against which Samuelson has often argued (section IV in my 2007). The other issue regards the classical economists' theory of wages, the heart of their analysis and of its structure, as I have contended (Garegnani 2007, section I and II) and textually supported by the numerous well known 'puzzles' which Smith and Ricardo's theory of wages raise for modern interpreters (Garegnani 2007, section Vc), or by my criticism of Samuelson's interpretation of chapter XXXI 'On Machinery' of the *Principles* (Garegnani 2007, section Vd).

However the right spirit of scientific discourse does emerge when, in the Reply, we read that: 'if my […] syllogisms are found to be erroneous the present exercise will have been valuable in helping establish where the truth does *probably* reside' (Samuelson 2007: 244, italics in the original), a statement to which I would commit myself too, while taking it in the broadest sense suggested by the weighty word 'truth'.

2 The other paradigm of economic theory

The argument of my article had a twofold aim. The first and central one was to contend the existence in the early classical economists—and in Sraffa's (1960) *Production of Commodities*—of a theoretical approach to distribution and relative prices alternative to the dominant one ultimately founded on the substitutability between factors of production. My second aim was to recall the present state of the critique of this latter theory that has accompanied the resumption of the classical approach. That second aim was suggested by the 'doom' Samuelson (2000: 115ff.) had predicted for the critique.

On the first point, the Reply is laconically unrepentant. Indeed, by means of its 'Master production function' it focuses on enlarging on that interpretation of Sraffa and the classical economists in terms of a 'discontinuous' version of neoclassical demand and supply, which I had briefly rebutted (Garegnani 2007, para. 24), as contrasting with the evidence we find in those authors.

On my contention that what we trace in their writings is instead the alternative approach to distribution and prices outlined there, the Reply states: 'It is a myth that there ever did exist a plausible classical paradigm in which competitive price ratios […] were invariant to changes in objective consumers demand' (Samuelson 2007, p. 2). But in my article, what I deny the existence of in the classical economists is not that variance: it is the neoclassical interpretation of it advanced by Samuelson. As argued in para. 1 and 32 of Garegnani (2007), the latter interpretation depends, among other things, upon the assumption of a full employment of labour: it accordingly clashes with Ricardo's and Smith's admission of permanent labour unemployment. Connectedly it clashes with their wage theory, so remarkably and significantly indigestible to modern interpreters (Garegnani 2007, para. 28–31). The Reply is silent on these contradictions to its interpretation.

We also read in the Reply:

> It is textually dubious that post-1870 neoclassicism […] differed from classicism […] importantly because the former linked purely competitive supply and demand with constant returns to scale, whereas the earlier group definitely did not have to do so.
>
> (p. 2)

But no answer is given to my basic contention of a classical separate determination of outputs, which are then implicitly treated as 'intermediate data' in determining prices and to the corresponding textual evidence. This treatment makes constant or variable returns irrelevant in price determination, as distinct from their centrality in the analysis of capital accumulation and growth (Garegnani 2007, para. 5–7). Outputs as 'intermediate data' in price determination are in fact essentially what Samuelson himself attributes to Ricardo when he blames him for taking as given the position of the no rent land in his theory of relative prices (e.g. Samuelson 1998: 462ff.). And, more generally, this Classical difference

from later theory on returns was certainly important enough to compel Marshall to attribute to Ricardo an absolutely rigid demand for 'corn' and, somewhat contradictorily, constant returns for all other commodities (Garegnani 2007, para. 6). This separate determination of outputs was indeed the simple, general way out, which, owing to their different wage theory, the Classical economists could take, and instinctively took, out of what the Reply itself calls the 'quagmire' of returns in the theory of value (Samuelson 2007: 247, n. 3).

On the other hand, the above treatment of outputs as 'intermediate data' provides the answer to the Reply's alleged 'impossibility of natural classical prices' (Samuelson 2007: 248), due to that 'until economists Smith and Ricardo know *Distribution* they generically cannot know Values' (Samuelson 2007: 247, italics in original). This may seem surprising in a Reply to Garegnani (2007), where the difference between the two theoretical approaches is traced to their different theory of distribution. Indeed for the particular problem to which the Reply refers in the passage above—i.e. the necessity of consumer demand for knowing the ratio between the wage and the rent of land on which competitive normal or natural prices depend—I had argued (Garegnani 2007, Section I) that, given a real wage determined by its own circumstances, the separate determination of those outputs, and therefore their widely recognized treatment as givens, allowed Ricardo—and by a more rigorous procedure, Sraffa—to determine rents and their ratio to wages.[1]

In conclusion, the Reply does not appear to answer my central contention, namely, that the theory of wages of Smith and Ricardo, generally admitted to differ from the modern one, does in fact entail a different determination of outputs and, thereby, an altogether different content and structure for the theory—in which, for example, supply and demand take on their original Smithian meaning of single quantities, not functions, relevant for the analysis of the gravitation of actual prices towards natural or normal levels, which are determined separately from those two quantities (Garegnani 2007, para. 8).

3 The critique of the dominant paradigm

With regard to the second, more critical aim of my article, the Reply is silent on my contention that the 'Hicksian' or (as more commonly indicated) neo-Walrasian reformulations of the theory (Garegnani 2007, para. 35) are emerging to be as dependent on the inconsistent conception of capital as a single magnitude as the traditional pre-Hicksian formulations were (Garegnani 2007, para. 37). It therefore also emerges that the same deficiencies, which allowed the early 1960–70 stage of the capital controversy to dismiss in a few years the formulations given by Marshall, Clark, Wicksell, Pigou, Robertson—and, in effect, by all other mainstream authors until those decades—also undermine those contemporary reformulations. And, the reformulations in question are those to which, as we shall presently contend, the Master production function of the Reply does in fact pertain.

What we find is only an indirect reference to the critique, when the Reply rejects 'the view that only stationary states [i.e., it appears, the 'normal positions'

of Garegnani 2007, para. 34] are deductively tractable' (Samuelson 2007: 4). It is essentially the rejection of the traditional normal positions in favour of the Hicksian (neo-Walrasian) temporary or intemporal equilibria—a rejection imposed by the abandonment of the conception of the given capital endowment as a single magnitude on which the normal position has to rest in neoclassical theory in order to allow for the required adjusted physical composition of its capital endowment—i.e. for its (riskless) uniform effective[2] rate of return on the capitals' supply prices[3] necessary, I have argued, for its 'persistency' and therefore its potential correspondence with observation (Garegnani 2007, para. 36).

Samuelson's passage above continued with: 'My use here of twenty-first century dynamic Samuelson Etula Master Function will rebut that claim', where the reference to a dynamic analysis may be intended to answer, in particular, the Marshall passage I used for my argument (Garegnani 2007, para. 34): 'dynamical solution in the physical sense of economic problems are unattainable' (Marshall 1898: 39), where, however, Marshall was surely not referring to any absence of abstract tools like the Master function for such solutions. What he judged unobtainable was a dynamics with the possibility of correspondence with observation that we can have in the physical sciences (Marshall's immediately preceding example was astronomy). In economics, he in fact continued, one has instead to be content with 'such rude and imperfect approaches to dynamical solutions as we may be able to attain to', i.e. essentially, the comparisons of the normal positions (Garegnani 2007 para. 36). But in neoclassical theory, and here comes the central point, this would require a consistent notion of the capital endowment as a magnitude susceptible of changing its physical 'form', though not 'its quantity'.[4]

In fact, the Reply provides no argument for its rejection of the normal position (and we shall see in Appendix 1, how some misunderstandings appear to underlie Samuelson's position on the matter). I can here, therefore, only refer the interested reader back to Garegnani (2007, para. 35) or to (Garegnani 1976) for a discussion of the issue.

4 The Master production function

As for the Reply's own argument, I can say that I very much welcome its Master function—once, of course, it is ascribed where it essentially pertains, namely, to the contemporary reformulations of neoclassical theory and not to the Classics or Sraffa, who do not know, for example, of the equality postulated there between factor demands and factor supplies, or indeed of any such 'demands' and 'supplies'.[5] I do in fact welcome the Master function because of its dedication to pinning the little of factor substitution remaining in the Hicksian reformulations of the theory down to earthy numerical examples. Indeed, once the capital endowment is expressed as a physical vector, as is done there, it should be evident that, for the components of the vector, the continua of alternative methods of the traditional production functions cannot exist and that, accordingly, only the finite number of alternative methods of the Master function has any plausibility left.

As will be argued in Appendix 1, the Master function does indeed highlight an element that, perhaps because of its less strictly analytical nature, has gone unduly unstressed in the course of the capital controversies so far. The function makes, so to speak, tangible the near absence, in those reformulations, of the factor substitutability on which the theory must however continue to rest for its validity—a third main difficulty besides the emerging continuing dependence on 'capital', the single productive factor and the methodological one recalled above concerning correspondence with observation. Like its near homonym, Samuelson's (1962) own Surrogate production function of old, the Master production function might therefore end by making the inherent weaknesses of contemporary theory more evident.

Some question begging seems indeed to underlie the optimistic contrary opinion, struck in the conclusion to the Reply, when we read: 'a pro-Sraffian who believes strongly that the world empirically has but few viable alternative subtechnologies ought to expect distributive shares to jump around volatilely in a way that econometricians do not find to occur' (Samuelson 2007: 262).

But the dispute is about validity of the theory, and its contradictions with reality are clearly an element for the critique of the theory and for an alternative to it, not for its posteriori validation. The passage constitutes, indeed, as clear a confirmation as we may wish of my general point (Garegnani 2007, para. 1, 8, 23, *passim*) about Samuelson's tendency to take neoclassical demand–supply as facts and not as a theory, subject to disproof like any theory.

Appendix 1: On the 'Master function'

The Master function and factor demands

For my purpose here, I must first give a brief account of what I understand to be the Master function of the Reply. The function supposes the existence of a discrete number of alternative methods for the (direct and indirect) production[6] of goods, which the consumers are assumed to demand in fixed proportion (apparently in order to reflect the absence of utility analysis in the Classical authors to whom the function is ascribed).[7] The quantity of labour available is taken as a unit so that both outputs and the remaining factor endowments—which the Reply's Table II takes to consist only of the several capital goods—are directly measured as proportions to labour.

Neoclassical competitive interaction between consumer demand and factors supply governed by given endowments would then lead for an equilibrium to what we may call 'cluster' of such methods sufficient to define by their coexistence the price position of the economy (think, for a simple example, of a labour and land only economy producing corn and cloth, where the method of production of cloth plus two methods for producing corn on homogeneous land suffice to determine wage, rent and cloth-to-corn price). Given a sufficient number of factors (in particular capital goods) common to the coexisting methods of each 'cluster', changes in the proportions in which the latter are activated, define,

'areas' of factor endowments (the factors are assumed to be rigidly supplied), which could be fully employed with the methods of the 'cluster' in question (cf. Samuelson 2007, Figures 1 and 2), so that any endowment in the 'area' would be compatible with equilibrium at the prices defined by the 'cluster'. Looking at it from the side of each factor, the proportionate changes in the activation of the methods would describe a horizontal segment in its demand, where equilibrium for the system could be achieved if the endowment of the factor happened to fall in that segment.[8]

If the available alternative methods allow for several such 'clusters' and they are also such as to have a sufficient number of capital goods in common also between the clusters, then we shall have for each factor a decreasing step demand function, each horizontal segment corresponding to one of the clusters. Prices between those of the clusters, i.e. between those corresponding to the horizontal demand segments for each factor, would be indeterminate: but this indeterminacy would be bounded by the adjacent horizontal segments and it could only occur for factor endowments lying exactly on the edge between two endowment 'areas'.[9] That indeterminacy would therefore mean, above all, that prices move in jumps as changes in tastes or endowments shift the economy from one cluster to another.

The Master function's marginal products

The Reply however does not explicitly consider the above demand functions. It focuses instead on the equivalent fact that—given a sufficient number of capital goods common between the methods forming each cluster, and between those of the several clusters—a key assumption for the whole construct—then the methods of each cluster will make it possible to define what Samuelson envisages as 'marginal products' of the factors. They are obtained by changing the proportions in which the methods are activated so as to keep constant the quantity of all factors but the one in whose 'marginal product' we are interested. By construction, the result will coincide with a horizontal segment of the above step demand of the factor.

It should be noted straightaway, however, that the 'marginal products' resulting from the Master function are different from the traditional ones, founded on intellectual experiments of the individual producers. Thus, their equality with the respective factor price is not a direct entailment of producer maximization: it is simply a result of the competitive price equations, corresponding to the methods of the clusters.[10] These equations, it is then important to notice, would be sufficient to determine the price position of the economy, even when capital goods differ between methods enough as not to allow for any such marginal products.[11] Now, this dependence on the price equations is in contrast with what happens for the traditional marginal products which, since they entail no coexistence of methods, do add conditions to the price equations of the products, whose number would otherwise be insufficient for the determinacy of the price system. The difference between the two kinds of marginal products is, on the other hand,

implied by the Reply itself, when it is remarked that the marginal products of the Master function are 'locally linear' (Samuelson 2007: 25), i.e. that (i) they are linear within each cluster and (ii) the function giving them must be redefined for each such 'cluster'. Both such properties are alien to the traditional marginal products.

A first limitation of the Master function

I take the essential analytical point of the Master function to lie in exhibiting, in a concrete way, factor substitution such as is possible in the Hicksian reformulations of the theory (section 4 of the text above). Two observations seem however sufficient to indicate how the Master function helps us to realize the strict limitations of that substitutability. The heterogeneity of the capital goods between alternative production processes—which deprives any 'smooth' factor substitutability of sense, even as an approximation, as a factor—also drastically reduces the discontinuous substitutability analysed in the Master function.

The first such observation, which has been aired already in this Appendix, is that there is no reason why the methods of the clusters should have in common a sufficient number of kinds of capital good, as is assumed in the Reply. Indeed, elementary observation suggests that this is not (relevantly) the general case. But then, not only the Reply's Master function and its marginal products will not exist, but it will be difficult to envisage factor demands leading to plausible non-zero prices for the factors (whose scarcity Samuelson seems here to correctly take as known from experience rather than as results of the equations).[12] The possibility of a reswitching of techniques will then also arise and it will no longer be true even that a given cluster of techniques defines only one economically significant price positions of the economy.[13]

A second limitation of the Master function

The second observation is that even when, as in the examples of Table II in the Reply (p. 19), the number of capital goods common to the methods are sufficient for step demand functions (and 'marginal products') of the factors, even then the possibility of substitution would generally be very limited. This is well exemplified by the mentioned clusters of Table II, where the change in the composition of consumer demand from 'wheat only' to 'wheat and iron', illustrated by moving between the two sets of quadrangles of Fig. 2 in the Reply (p. 260), shows that none of the endowments ensuring non-zero factor prices for the case 'only wheat', does the same for the case 'wheat and iron'.

To avoid consumer choice leading, in this way, to zero prices of the factors of the initial endowments, in particular zero gross prices of capital goods services, the endowment 'areas' compatible with full employment under different compositions of consumer demand should broadly coincide, as it was possible to suppose in the case of the traditional continuous production function based on 'capital', the single magnitude supposedly susceptible to changing its 'form'.[14]

The Master function then makes us 'touch', so to speak, how difficult it is to satisfy that condition, when adopting a vectorial notion of the capital endowment.

A missed implication of the Master function

We have so far seen the Master function as a tool for the analysis of factor substitution in what are in effect the contemporary reformulations of neoclassical theory. But Samuelson's treatment of the Master function also brings to light in a rather striking way what was listed in section 4 of the main text as, the second basic deficiency of contemporary neoclassicism besides the emerging continuing dependence on a 'quantity of capital': namely, the undermining of the possibility of correspondence between theory and observation.

As indicated in an old comment of mine (Garegnani 1976) on Samuelson's work, by expressing the capital endowment as a physical vector Samuelson was bound to refer to a position of the economy where the effective rates of return[15] on the capitals' supply prices do differ—to refer, that is, to a position that the economy moves away from, rather than tending towards, just as it would move away from a position in which wages for the same quality of labour happened to differ or prices of commodities happened to differ from the respective supply prices.

Samuelson, however, incorrectly labels that divergence of effective returns as it emerges, e.g. from the Table II cluster *b*, *c*, *A*, *B*, corresponding to endowment β (Samuelson 2007: 260), as a divergence of 'commodity own rates of interest' (p. 261n), a phenomenon that is instead the mere result, when it occurs, of considering within the equilibrium conditions future changes in relative prices[16] (of which there is in fact no trace in the Reply).

This incorrect labelling, then, appears to lead Samuelson into an incorrect determination of the corresponding equilibrium. While the divergence of own commodity rates of interest in no way affects the equality or inequality between the price of a commodity and its costs or supply price, a divergence of the effective rates of return on the capitals' supply prices prevents that equality for the capital goods giving a lower such return than one or more others. The nature of savings as demand for the single commodity 'future income' (cf. e.g. Garegnani, 2000, para. 12, 22–3) is indeed such that, as is on the other hand well known, arbitrage imposes the corresponding single price, i.e. the effective rate of return on the supply price of the capital good having the highest such return, also on the remaining capitals by lowering their 'demand price' sufficiently below the respective supply price.[17] Now, we find no trace of such a lower demand price in the case of equilibrium b where prices remain equal to their supply prices and, accordingly, iron's effective rate of return is 1.72 percent vs. 13.55 percent for wheat (Samuelson 2007: 261n), a situation that no arbitrage would allow in the economy.

This incorrect determination of capital goods' prices has, in turn, consequences because the capital goods with a demand price below supply prices will evidently not be reproduced and the physical net product of the Master function

will be entirely notional. The impermanence that prevents such equilibria from being a possible basis for correspondence with observation does then appear here in the 'tangible' fact that the capital endowment will soon have to change, so as to allow, for example, for iron production and, hence, at one remove, wheat production as well to occur. And with it Master function and price system will also have to change.

Notes

1 As for the final influence of the Reply's consumer demand on this distribution, we already saw the question in connection with Samuelson's first passage above.
2 By 'effective', we mean the single rate of return on capital which underlies its multiple 'nominal' expressions dependent on the numeraire, occurring when future changes in relative prices enter the equilibrium conditions.
3 The condition can be stated, if we prefer, as the equality between the demand prices of the capital goods and the respective supply prices: (cf. Appendix 1, p. 584, below. The capital goods referred to are of course always those pertaining to the dominant techniques).
4 As Hicks put it *a contrario* when—still basing his analysis on normal positions and, therefore, on capital as a single magnitude—he described as follows what he called the 'short period marginal product of labour': 'the additional production due to a small increase in the quantity of labour *when not only the quantity, but also the form of the co-operative capital is supposed unchanged'* (Hicks 1932: 20, our emphasis) then dismissing the whole notion by: 'It is very doubtful if the conception can be given any precise meaning which is capable of useful application' thus adopting a marginal product of labour in which the 'quantity of capital' is constant, while its physical 'form' changes.
5 In connection with this attribution we also read in the Reply (p. 22): 'Sraffa (1960, part III) went some limited steps towards seeking missing equations along the lines [of the Master function]' but in Sraffa we find the coexisting methods of the Master function when distribution between wages and profits happens to allow for them, with the distribution, therefore, determining their coexistence and not vice versa.
6 The Reply seems to take the Master function as including the traditional continuous production function (Samuelson 2007: 9–10), so that also in a context of discrete alternative methods we should take the function as a single one taking as many forms as there are 'clusters' of methods. Here I shall refer exclusively to the discrete case on which the Reply is in fact focused.
7 Nothing, however, prevents us from dropping that assumption and letting the proportion of the goods demanded by consumers depend on their relative prices: when we refer to the Master function as pertaining to the contemporary reformulations of the theory, we evidently envisage such a completion of the Reply's treatment of the function (see also footnote d).
8 This factor demand segment, as well as the whole step demand function of which it is a part, are traced on the assumption that the remaining factors in the endowment are fully employed and, more generally, that equilibrium holds in all markets except the one of the factor in question (the income of the owners of the factor is accordingly assumed to correspond to the quantity demanded and therefore actually sold by them, and not to the quantity supplied at the assumed prices. As may be easily realized this makes a disequilibrium in the single market of the factor in question compatible with Walras' law: (cf. the notion of 'general equilibrium demand function' in e.g. Garegnani 2000: 201, n.15)).
9 It may however be noted that the vertical segment would not be such if we introduced—as we saw the Reply makes a point of not doing—a physical composition of

consumption which would change, and change factor demands accordingly, as commodity and factor prices move from cluster to cluster. This however will again be so only if the methods of production of the alternative consumption goods had a sufficient number of capital goods in common. This indirect substitution operating through consumer choice will accordingly be subject to limitations parallel to those we shall see below for the factor substitution operating through alternative techniques.

10 Since the constant prices of the 'cluster' must equal costs, a change in the proportions of the alternative methods allowing for a unit increase in the employment of the variable factor, will result in an increment of the value of the aggregate output which is necessarily equal to the service price of the factors. And when that value increment is referred to a single commodity, that service price expressed in terms of that commodity will have to equal the physical increment or 'marginal product' of the factor.

11 See, for example, the case of the techniques assumed by Samuelson in his (1962) Surrogate capital article such that a consumption good is produced by labour and a capital good, specific to the technique, in turn produced by itself and labour. Each couple of such techniques can form a cluster determining at least one series (cf. note h below) of real wage, interest rate, and the consumer good prices of the two capital goods specific to each techniques. These clusters will not allow for a Master function and its 'marginal products'.

12 'Relevantly' in the sense that changes in the proportions of the factors thus physically defined, when at all possible, are likely to be dominated at the corresponding new prices by alternative methods requiring changes in the kinds of capital goods employed.

13 Cf. the clusters of two techniques of the 'Surrogate capital' kind mentioned in footnote *f* above. A reswitching between two such techniques (cf. e.g. Garegnani 1970: 413, Fig. 2) entails of course two points where the techniques coexist, thus defining two series for wage, interest rate, and the prices of the two capital goods.

14 Cf. the Hicks (1932) passage quoted in n.4 of the text above.

15 For the adjective 'effective', attributed to that rate of return see n.2 of the text above.

16 If a commodity *A* is going to rise in price by 1 percent relative to *B*, over the year of the loan, arbitrage will evidently tend to impose for a loan denominated in *A* an 'own' interest rate, which is 1 percent lower than that for a loan denominated in *B*. This divergence clearly has nothing to do with capital goods in particular, and even less with the returns over their supply prices resulting from the Master function.

17 See e.g. Walras' distinction between 'prix de vente' and 'prix de revient' of the 'capitaux proprement dits' (1954 e.g. 2771ff.).

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