Euro. J. History of Economic Thought 14:2 181 – 242 June 2007

Routledge
Taylor & Francis Group

Professor Samuelson on Sraffa and the Classical economists

The European Journal of the History of Economic Thought, vol. 14, n° 2, 2007

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> 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 answered 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

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The European Journal of the History of Economic Thought
ISSN 0967-2567 print/ISSN 1469-5936 online © 2007 Taylor & Francis
http://www.tandf.co.uk/journals
DOI: 10.1080/09672560701327919

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

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.

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

2 On the notion of 'normal positions' of the economy, see section VIa below.

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.

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).

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

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.

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

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.

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

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.

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

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).

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,

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.

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,

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.

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

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*.

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,

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).

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).

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.

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]

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

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

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.

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

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.

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]

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

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

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.

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

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).

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

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.

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).

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).

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

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:

$$r = \frac{1 - w}{K}$$

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

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.

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]

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).

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 re-enforced 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

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.

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

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).

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.: 1416–7). 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

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.

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

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.

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

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.

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

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.

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

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

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

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.

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.

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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.

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

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.

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

53 Cf. n.48 above.

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

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).

> 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

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

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,

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*.

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]

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

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

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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'.

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

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'.

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]).

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

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

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 Halm 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.

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]

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.

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,

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).

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.

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'.

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: 111–2). 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

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

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]

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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).

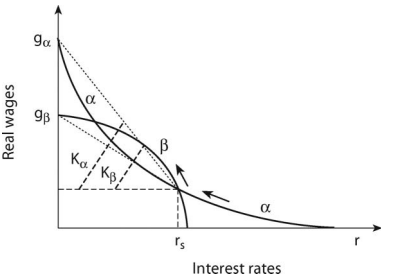

Figure a Wage curves α and β show the relation between the real wage ω and the interest rate r when the corresponding techniques α or β are in use. At point S as r decreases, permanent consumption per worker falls from g_α to g_β and capital per worker in the integrated production of the consumption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

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.

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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).

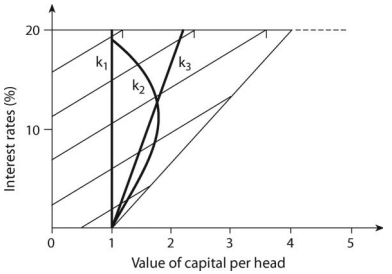

Figure b k_1, k_2 and k_3 are possible relations between the interest rate and the value of capital (as defined for Fig. (a)), and the same is true for any such relation keeping within the shadowed area of the diagram

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

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'.

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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.

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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).[A1] Samuelson (1990a) sees, however, a 'fatal error' in this position and argues that in

A1 A summary of Sraffa's argument is provided in my comment (Garegnani 1990: 284–7).

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'économie 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 re-discuss 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,[A2] 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,[A3] 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

A2 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.

A3 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.

of 1750 – 1821—and are also not present today, when we study the economy along the approach of those economists.[A4]

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 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

A4 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.

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