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PROFESSOR BLAUG ON UNDERSTANDING CLASSICAL ECONOMICS

Pierangelo Garegnani

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Abstract of the paper

In Blaug's view, the interpretation he labels as "Sraffian" considers the classical economists through the "distorting lenses" of "linear models of production" and overlooks the central importance those authors gave to institutional and political elements. This view appears however to be founded on a misreading. Unlike what Blaug supposes, that interpretation does not take outputs and real wages as ultimate data, but only as what we may describe as "intermediate" data: they are determined, that is, prior to, and separately from, the non–wage distributive variables which are then obtained as the difference or surplus of a given output over the given subsistence–based wages (and means of production) consumed in its production. By ignoring that prior classical determination, Blaug in fact neatly excises from the interpretation he is criticising the very institutional and political factors which he, then, unsurprisingly finds missing, and which are those playing the dominant role in the (separate) determination of wages and outputs. Blaug's detailed criticisms are shown to confirm that basic misreading.

It is further pointed out how the "Canonical" interpretation of classical wages which Blaug shares with Samuelson and other interpreters, and to which he essentially refers in his article, appears to beg the question of the demand and supply distributive mechanism it attempts to trace in Smith and Ricardo. It does so when it has to introduce a neoclassical type of elastic labour demand function to trigger off the wage changes destined to balance the growths of labour and capital — a process which in those classical writers is instead based, we argue, on the varying pressure of labour underemployment ("redundant population") implied in what they often describe as the "proportion of the demand to the supply of labour", two single quantities and not the modern functions which go by that expression.

Blaug's contradictory rejection of the "Sraffian interpretation" — key elements of which, like the so called "corn model" or the recognition of permanent labour unemployment by classical authors, he has long shared — are then traced to his difficulty in conceiving the possibility of a theoretical paradigm alternative to the dominant one, such as it existed in the old classical economists before the drastic break in theoretical continuity which occurred after Ricardo's death.

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PROFESSOR BLAUG ON UNDERSTANDING CLASSICAL ECONOMICS

Pierangelo Garegnani*

I. Introduction

1. Professor Blaug must be thanked for the opportunity which his article (*History of Political Economy*, 1999; unspecified page references are to it) offers for discussing some important issues in the interpretation of the classical economists. He must be particularly thanked for exposing the need to clarify the work done on bringing out that "standpoint of the old classical economists" which has been "submerged and forgotten since the advent of the marginal method" (Sraffa 1960, v). It is work, which has centred on tracing the principles underlying the standpoint, so as to compare them with those of modern theory.

Given the initial general acceptance of Sraffa's interpretation, with the later challenges turning only on Ricardo's early theory of profits, less attention was devoted to relating those abstract principles to the body of classical writings with its different authors and topics. [1] One result of this seems to have been the paradox that an interpretation pointing to the role which the classical economists gave to institutional and historical factors in the division of the product between classes, in capital accumulation or the development of technical knowledge is being criticised as an "amazingly narrow" one which looks at those economists "through the lenses of the linear production model" (233) and ignores their broader social and political interests (217; 234; also 1987, e.g. 442). A merit of Blaug (1999) is that he gives effective expression to this paradoxical criticism, thus offering an important possibility of clarification.

But the need of clarification that Blaug (1999) exposes goes further. The contrast between the seeming "narrowness" of the interpretation and the evident broad character of classical works, has lent credibility to the idea of an

* University of Roma Tre. The essay has been stimulated by Blaug (1999). A shorter version of it has already been published on the Spring 2002 issue of *History of Political Economy*. I wish to thank for their comments Professors R. Ciccone, H. D. Kurz, F. Petri, A. Stirati and participants of the 2000 Conference of the European Society for the History of Economic Thought (E.S.H.E.T.) in Graz where a first draft of this paper was presented.

[1] However works like Aspromourgos (1986); de Vivo (2000); Gilibert (1977); Marcuzzo and Rosselli (1991); Pivetti (1991); Stirati (1994); Vaggi (1987); Vianello (2002) and others have validly dealt with specific classical authors or specific aspects of their work in the light of that interpretation.

interpretation which reads "backwards" into Smith's and Ricardo's texts [2] the theoretical alternative to neoclassical theory which Sraffa has advanced — as if the contrary had not rather been the case, with those texts being the sources to which one had to resort in order to understand Sraffa's terse book (1960).[3] This second criticism also finds effective expression in Blaug (1999) and other works of his, thus again offering a chance for clarification.

2. Professor Blaug, we said, is critical of the interpretation of the classical economists advanced by "Sraffians" (I may note in passing how such collective references may easily, if unwittingly, result in blurring the targets of criticism).[4] He claims that that interpretation based on the notion of social surplus, while adequate for Quesnay and the French Physiocrats (*e.g.* 1987, 439), does not provide a historically valid reconstruction of the work of the British classical economists.

A first reaction to this position is surprise, since in his concrete work on the classical economists Professor Blaug has often acknowledged basic elements on which the "Sraffian" interpretation rests. To give an important example, since (1958) Blaug has pointed out the presence in Ricardo of permanent labour unemployment curable, that is, only by further capital accumulation (par. 20 below). This clearly contradicts a wage determined by the intersection of demand and supply schedules as in modern theory, and is by itself sufficient to raise the question of an alternative theory of distribution in Smith and in Ricardo. More precisely, it is then natural to ask by which mechanism other than labour demand and supply schedules did those authors envisage population and capital as bringing about the subsistence–based wage trend they referred to (par. 14–15 below). And, as we shall see below (par. 20–21), other central elements of the "Sraffian" interpretation have been accepted by Blaug, among which the basic proposition he describes as Ricardo's "Fundamental theorem of distribution", expressed in the so called "corn model", under which a determination of profits as a difference or surplus is in fact acknowledged to be present in Ricardo. [5]

[2] *E.g.* Blaug 1999 (215–16); 1987 (440–441); 1988 (28). See also Peach 1999 (458) and *passim*.

[3] That, at least, has been my personal experience. Stimulated by Sraffa (1951) to a Cambridge Ph.D dissertation (1958) comparing the classical and marginalist approaches to distribution on the question of the measurement of capital, I then found in the classical works I had become acquainted with the basis for understanding Sraffa's (1960) book when it came out (cf. n. 6 below).

[4] Peach, who in (1999) advances essentially the same two criticisms of the "Sraffian" interpretation, provides on the other hand an example of the risks of such collective references: in his article it is at times difficult to trace by which specific author, or by which precise arguments some positions he criticises have in fact been advanced (cf. *e.g.* below Section V, n. 25, and VI, n. 35).

[5] Thus the "corn model" is seen to show that for Ricardo "total profits are a *residual* equal to the total produce–less–rent–minus the wage bill" (Blaug 1962, 86; our italics; similar references to the "corn model" for Ricardo's theory of distribution can be found in O'Brien 1975, 40 and *passim*).

The above contradiction in Blaug's position comes, we shall suggest, from his seeing those classical elements in isolation from each other rather than in the connections which make them aspects of an approach to distribution and prices, basically different from the modern one. Blaug has then to fall back on ascribing to confusion or primitivism the undeniable striking differences of Smith's and Ricardo's analyses from the modern ones (see e.g. par. 14–15 and 20 below). The criticism of the surplus interpretation of Sraffa by Blaug and others authors is ultimately traceable, we shall argue, to the difficulty of conceiving of an approach to distribution other than the one based on demand and supply functions for "factors of production".

3. This difficulty of conceiving an alternative theory finds indeed a first confirmation when in his article Blaug describes the "Sraffian" interpretation as one for which the classical economists addressed the question of

> "how an economic surplus is generated, spent, and augmented from period to period",

where "economic surplus" is described as what

> "is left over after capital consumption has been made good and workers fed" (216; see also 214).

However what was originally meant in that interpretation was more specific, and not at all a question of *subject matter* as might instead be gathered from Blaug's passages. It was question of tracing in Smith and Ricardo a *theory of distribution and relative prices* altogether different from any however primitive form of the neoclassical demand and supply of 'factors'. [6] In that alternative theory the non–wage distributive variables were determined on the basis of the *difference* between a previously determined (net) social product and a previously determined wage governed by workers subsistence. [7]

Aware perhaps of the vagueness left by his definitions, Blaug then focuses his attention on what he describes as the "hard version" of the interpretation, which he traces in particular to my work. He characterises that version by the role of "data" which he says I have attributed to the real wage, the outputs, and technical knowledge in Smith and Ricardo. But what I contended was quite different:

[6] This was what I argued when advancing that interpretation in Part I of the mentioned (1958) Ph.D. dissertation and then in (1960).

[7] On the other hand we do not need to enter here into the further problem of how the surplus was then divided between profits on capital and rent on land according to the several authors: Ricardo's procedure is well known; for the question in Smith and more generally the attribution to Adam Smith of the above notion of surplus cf. VI, n. 35 below.

it was that those circumstances were data for determining the non–wage distributive variables and the relative prices, and not data for the theory as a whole. [8]

The question is really the same just mentioned of a specific theory of distribution determining the non–wage variables as a residual or surplus of the product over the wages. Within such determination, the physical product and the real wages had evidently to appear as givens, [9] but since no economist, least of all Smith and Ricardo, can avoid being concerned with determining product and wages, that really meant that those magnitudes (and the technical conditions with them) were determined previously, and therefore separately, from non–wage distributive variables and prices. They only appeared as data in the purely quantitative "core" of the theory where the relationships between the distributive variables and prices enforced by free competition allowed determining profits and/or rents, as a residual. In that precise sense, those 'data' were, we may say, "intermediate" and not "ultimate" in the theory. [10]

We shall see in par. 7 below *why* that "separate" determination, but we may notice already how the misreading about "data" can be responsible for the reversal in the thrust of the "Sraffian" interpretation we pointed out in par. 1. The socio–institutional richness of the classical authors emerges precisely in the determination of wages, outputs and technical conditions, which Blaug neatly excises from the "Sraffian" interpretation the moment in which he ascribes to it a treatment of those circumstances as "ultimate" data.

4. Section II of our Comment will deal with Blaug's above misreading about data and some related issues. In Section III–V we shall consider Blaug's detailed arguments concerning the role of the technical conditions of production, the outputs and the wage in classical theories. In Section VI the discussion of Smith's and Ricardo's theory of wages conducted in Section V will allow us to go deeper into the determination of outputs, and thus into the essential structure of those authors' analysis. Finally in Section VII we shall try to pull together the threads of our argument, while also following Blaug in an incursion into economic theory, as distinct from its history.

[8] Cf. Garegnani, *e.g.* 1984 (293), where I write of the three circumstances as being known "prior to the determination of the surplus".

[9] It is interesting to notice how the kinship between the above treatment of distribution, and the "corn model" he attributed to Ricardo (n. 5 above), did not alert Blaug to the true terms of the interpretation he criticises.

[10] Of course several other circumstances *e.g.*, the natural resources, were taken as given. However only circumstances that are liable to *change* in a context of interest to the economist, are relevant as "data". We may also note here how the distinction between the two kinds of data is foreign to modern theory where tastes, technical knowledge and endowments have been generally treated as "ultimate" data. There also however those circumstances would turn into "intermediate" data when a determination of them were envisaged as *separate* from the general–equilibrium determination of prices and outputs.

II. The Misreading

5. Before dealing with Blaug's detailed arguments, I should make clear that by "classical economists" we should here mean "the *old* classical economists *from Adam Smith to Ricardo*" to whom Sraffa refers when he defines the "classical standpoint" (Sraffa 1960, v., our italics). In his article Blaug refers instead to

> "the British tradition of classical political economy, starting more or less with Smith in the third quarter of the 18th century and ending more or less with J.S. Mill and K. Marx in the third quarter of the 19th century" (215).

The definition is surprising since the specific purpose of Blaug's article is to discuss interpretations, which are either Sraffa's own, or have been inspired by him, and are therefore advanced for the authors he explicitly indicates as the "old classical economists from Adam Smith to Ricardo", thus e.g. excluding J.S. Mill. And it is all the more surprising when it comes from Blaug, who titled a chapter of his (1958) book "The Half Way House of John Stuart Mill", and should therefore appreciate why Sraffa left that author, for example, out of his definition of the classical "standpoint". [11] Any support Blaug's argument might derive from those other authors would in fact be besides the point. [12]

6. In fact Blaug's misreading of my interpretation of Smith's and Ricardo's theories as to the question of data (par. 3) emerges already from some contradictions in Blaug's own text. Blaug quotes me to the effect that Smith's and Ricardo's way of determining the non–wage shares as a surplus

> "has its logical basis in the consideration of the real wage and social product as being determinable prior to those shares" (217),

but some pages later he characterises my interpretation as entailing, *tout court*, that "three parameters are taken as given in classical economics" (219). Taking

[11] *E.g.* Blaug refers there to J.S. Mill as upholding "an emasculated version of Ricardo's system, while yielding to the anti–Ricardians on a number of crucial analytical questions" (1958, 167). As for Mill's differences from Ricardo on the key question of the determination of wages, cf. *e.g.* par. 15 below. On the generally admitted transitional nature of the work of J.S. Mill and other authors of the period cf. *e.g.* O'Brien (1975, 43) who notes how "by 1831 at the latest it is probably true that no writers [...] could accurately be called Ricardian in the sense that they thought in terms of the corn model" (on O'Brien's notion of Ricardo's "Corn model" cf. I, n. 5 above).

[12] Blaug's definition may partly reflect the misplacement of exegetic domain which we shall consider in par. 8 below: although Sraffa's interpretation regards exclusively the *analytical structure* of the theory, which is widely recognised to change immediately after Ricardo (see the preceding footnote), Blaug often refers to that interpretation as if it concerned mainly subject–matter (see *e.g.* par. 3 above), with respect to which there is instead more continuity for at least another half century.

as determinable prior to other variables, and taking "as given" in the theory as a whole, are clearly not the same thing. And that is of course no mere question of wording: [13] Blaug counters my interpretation for most of his article by quite simply arguing that the classical economists were much concerned with determining wages, outputs or technical conditions of production (sections III–V below) — as if that had been denied by me. Indeed, once the misreading is cleared, much of Blaug (1999) looks like pushing an open door.

7. However the *substance* of the question of "intermediate data", as distinct from its *acknowledgement* as content of the interpretation Blaug wishes to criticize, is less simple, and sends us back to what we began to see in par. 3 about the classical determination of the non–wage distributive variables as a "residual" entailing that product, wage and the technical conditions of production were held susceptible of being determined *separately* from the non–wage distributive variables and prices. As I have argued elsewhere (1990a, 523–4) what that separate determination entailed was essentially a distinction between two fields of economic analysis, which the classical authors had drawn instinctively.

(i) A first field regarded the determination of the non–wage distributive variables and prices by means of the price relations of the "core" which the competitive tendency to uniform rates of wages, profits, and rents imposed in the economy and which, whatever the language, algebraic, arithmetical or literary expressing them, would show quantitative properties general and definite enough to warrant a purely deductive, quantitative analysis. [14]

(ii) The second field covered the rest of the theory, and in particular the determination of wages, outputs and technical conditions, where it was not felt possible to apply the purely deductive methods of the "core" at a level of generality appropriate to the theory. Those methods were there used for what today we would call "models", *i.e.* when dealing with *special* assumptions or *special* problems (think *e.g.* of Marx's reproduction schemes). Given the multiplicity and variability of the factors which were seen to determine the above three sets of circumstances, adopting the deductive, quantitative methods of the "core"

[13] An even more direct contradiction emerges when Blaug writes that according to my interpretation "all the three *exogenous* variables may influence each other" (217; my italics) where the adjective exogenous is in direct conflict with the reciprocal influences there mentioned which are clearly considered *within* the theory and render therefore *endogenous* the variables in question.

[14] In accordance with the notion of surplus (par. 3 above) the classical inquiry into the determination of the non–wage share was mainly conducted by what I have elsewhere called the "surplus equation method" (1984, 313–20) used by Quesnay for his *Tableau* and, essentially, by Ricardo and Marx for their determination of profits by means of labour values. The "surplus equation" we thus find in the "core" of those theories is an expression of the competitive price relations (see the presuppositions of uniform rates of profits and wages) no less than the price equations are. We may note in this connection how in the surplus interpretation, the labour theory of value is seen to play a key role (cf. n. 25 below), unlike what Blaug surprisingly states in his article (217; see also 233).

would have corresponded to introducing, in the apt words of Edgeworth, "arbitrary functions representing not merely not numerical knowledge but ignorance" (Edgeworth, 1881, 4).

The distinction meant that there was no space for the single system of quantitative relations determining prices, distribution and outputs which is postulated in modern theory, *i.e.* that wages, outputs or technical conditions were determined *separately from* and *not simultaneously with* prices and the unknown non–wage distributive variables and had accordingly to appear as what we have called "intermediate data" in the determination of these variables. [15]

We have referred to the distinction between the two fields of analysis as having been drawn "instinctively" by Smith and Ricardo. We can therefore hardly expect to find explicit formulations of it. What we do find are expressions of it, like Ricardo's Table in his (1815) *Essay on Profits* (1951–73; IV, 17), where the corn wage and the corn outputs are taken as independent variables in determining rents and the profit rate — or, more generally, that determination of prices separately from outputs which forced Marshall and the later interpreters to attempt to reconcile Ricardo with modern theory by attributing to him "constant return" in manufacturing and an "absolutely inelastic" demand for "corn" in agriculture, where constant return would evidently not have done. [16] And other expressions of the same procedure will be recalled in parr. 16–17 and 21 below: indeed, as we just saw, the distinction between the two fields of analysis is one and the same thing as the widely admitted attribution to those authors of the determination of the non–wage distributive variables as a difference or 'surplus'. [17]

[15] This appears to be close to what Schumpeter perceived as the "Ricardian vice" consisting of "[bundling up] as large parts [of the general system] as possible [...] so that [they] should be frozen as given" (1954, 472–3). In fact in Ricardo such "parts" were simply held not to be susceptible of the predefined quantitative relations with prices of which Schumpeter postulated the existence on the basis of neo–classical doctrine (the Edgeworth's quotation above explains why such algebraic relations are not at all the same thing as the influences or interactions which will generally exist between those "frozen" parts of the system and the prices and can only be dealt with by separate determination: par. 17 below). It may be incidentally noted how little the notion of a "core" of the theory "squeezes classical economics into a Walrasian box": on the contrary, it indicates that in the classical economists we cannot find any "Walrasian" (and more generally neoclassical) *simultaneous* determination of distribution, prices and outputs (cf. par. 18 below).

[16] Marshall 1920, 814; 833–4 respectively (cf. also II, n.13 above). "Constant return" in manufactures is there meant to be constant relative supply prices as the relative outputs of the commodities in question change (on the difference between this condition and that of constant returns to scale usually applied to production functions cf. Garegnani, 1998, 420 n. 13).

[17] It thus seems odd that Peach should think of finding support for his criticism of the "Sraffian" interpretation in the fact that "it is hard to think of many, if any eighteenth, or nineteenth century writers who start *explicitly* from [such data]" (1999, 453–4; italics in the original). It is however ironical to note that Marx, an author on whom Peach seems to rely for much of his criticism (*ibid.* 458–60; see below, V, n. 25), is one of the few who does so, and with respect to the key such datum, the wage (see par. 13 below). Steedman also notices the lack of explicit mention

8. Blaug's misreading of intermediate data as ultimate ones entails however, we saw (parr. 1 and 3), a second misunderstanding, concerning the "amazing narrowness" which he laments in the "Sraffian" interpretation (par. 1 above). Indeed had I interpreted those three sets of circumstances as *ultimate* data, *i.e.* argued that Smith and Ricardo were *not* concerned with determining wages and distribution, I would have been preposterous, rather than "narrow".

However, something would then have been seriously amiss in Blaug's argument too. At several points of his article he insists on my distinction between what is within the "core" of Smith's and Ricardo's theories and what is outside it (cf. *e.g.* 218–219). But, we may ask, what could ever lie *outside* that "core", if the data in it happened to be ultimate data of the theory? The theory would have coincided with its "core", and the distinction itself would be simply void of content.

It is perhaps in order to temper this contradiction latent in his argument, that Blaug slips, at times, onto a different tack. The tack is that, unlike what the reader may have gathered at first, "Sraffians" do admit the classical economists' concern with determining wages, outputs and technical conditions of production, [18] but that, by excluding such matters from the "core", they take them to have been of secondary importance for those authors. The distinction between "core" and "non–core" would thus have concerned the importance of the subjects treated, and not the question of method we saw in par. 7 above. The claim is already implicit in the word "periphery" that Blaug uses for what lies outside the "core" — a word which, for my part, I certainly did not use, any more than I would use the word "wrappings" for what surrounds the "core" of an apple.

Of course, even on this second tack, Blaug can easily claim a "narrowness" of the "Sraffian" interpretation: it would have been narrow indeed had I ever claimed that wages or capital accumulation were "peripheral" subjects for Smith or Ricardo.

Here however an important new point emerges. It is that Blaug's criticism of the "Sraffian" interpretation may owe much to a sheer misplacement of the domain of that interpretation — which is the logical structure of Smith's and Ricardo's analysis of distribution and relative prices, and not the subjects in their writings. The central concern of those authors for wages, population and capital

of the "core" in classical writers and he concludes that "Classical economics (vaguely defined) is open to more than one legitimate and useful characterisation — and dispute about which is the correct one would be futile" (1998, 119–20). However the central question is whether specific authors, Quesnay, Smith or Ricardo determined the non–wage variables as a residual. If they do, as is generally recognized for Quesnay or Ricardo's "corn model", or, also, Smith (n. 35 below), then as we saw, the "core" is no more than a restatement of that procedure. The discussion is then of some relevance of whether, as argued by Marshall and some contemporary writers, there was in each of those authors "a modern economist trying to be born", as Samuelson (1978, 1415) put it, or we should instead trace in them the basis of an "alternative paradigm" (*ibid.*).

[18] The idea emerges when *e.g.* Blaug admits that some Sraffians "allude" to a classical analysis relating the Malthusian law of population to wages (228).

accumulation has never been in question, and the plane on which the interpretation moves is that of whether the analysis of those subjects was conducted in terms of some however primitive form of the demand–and–supply functions of "productive factors" and products — as was argued by Marshall and is still often contended or implied — or was instead based on the altogether different principles discussed here. [19]

9. Allegations about "data" and "narrowness" are however all that substantiates Blaug's more general reproach against "Sraffians" for not providing the "historical reconstruction" of the classical economists which they purport to offer, and advancing instead what he calls a "rational reconstruction".

A historical reconstruction would be in terms that "the [past thinkers] in question and their contemporaries would have recognised as a more or less faithful description of what they set out to do", whereas a rational reconstruction would "appraise *their* ideas in *our* terms" (213; italics in the original). In particular, Sraffians would provide a rational reconstruction by attempting to read Sraffa's analysis "backwards into the works of Smith and Ricardo" (par. 1 above).

Perhaps the first thing to be observed here is how remarkable it is that Blaug, while admitting the character of Quesnay's and Smith's analyses as surplus theory (1987, 439) should deny it for Ricardo — all the more so in view of the already mentioned "Fundamental theorem of distribution" by which he essentially attributes to Ricardo the determination of profits as a "residual". [20] It is no less remarkable that Blaug should trace a "Whig interpretation of history" characterised by faith in a "relentless march of progress from past errors to present truths" (213), in an author like Sraffa who — having shown the historical accurateness testified by a critical edition of Ricardo generally hailed as one of the best in any subject — declared, from the very first lines of his (1960) book that he was adopting a standpoint "submerged and forgotten"; and, furthermore, subtitled the same book *Prelude to a Critique of Economic Theory.* In

[19] Thus, observations about the fact that "what Sraffians call the core of classical economics does not occupy more than [...] ten per cent of the total [of the Wealth of Nations]" (220), or that "theories of capital accumulation and population growth are more characteristic of the core of classical economics than a theory of value grounded in a linear production technology" (233), or even, more generally, that the "Sraffian" interpretation is "leaving out all the interesting elements that have proved fruitful down the years" (232), appear to be all beside the point. More pertinent would be the point Blaug raises when asking "what does [the surplus interpretation] tell us [about] Ricardo's law of comparative advantage" (1987, 439–440) — apparently envisaging that law as a neo–classical–like tendency to maximise outputs from given endowments. That interpretation would however be no less difficult to support than tracing the same maximizing tendency in the introduction of "machinery" (the analogy with international trade is Ricardo's own: 1951–73, I, 131–2). But in his chapter XXXI "On Machinery" Ricardo argues that by causing labour unemployment machinery may cause a *fall,* and not a *rise,* in the social product, and analogous results may come from trade specialization.

[20] See the quotation in n. 5 above.

Sraffa's view, clearly, the "march of progress" can "relent" and even be reversed for more than a century! [21]

But, coming now more directly to the distinction between a 'rational' and a 'historical' reconstruction of Smith and Ricardo, the case seems to me not essentially different from the one raised by *e.g.* a good contemporary grammar of Classical Latin. The grammar would obviously be in the language of today's grammarians, but if it were a good grammar of that Latin, a writer of that period, once he had learned the language of today's grammarians, would recognise the essential rules that he had followed consciously or unconsciously in expressing himself. And I believe Professor Blaug would agree with me in considering that such a good grammar, if it existed, would constitute a faithful *historical* reconstruction of Classical Latin: it would meet his test of recognition by contemporary authors. The point is, then, that the terms we use in order to reconstruct the structure of a past analysis, which cannot but be "our terms", are not really the question: the question is whether the structure we have thus reconstructed is, as unequivocally as possible, the one which is found to underlie the texts interpreted. This, I believe, is what Sraffa has done in his interpretation. It is also what I have tried to do when I have myself advanced interpretations of those authors.

III. On the Technical Conditions of Production

10. We may now proceed to the detailed points raised by Blaug in connection with my alleged failure to allow for Smith's and Ricardo's determination of technical conditions of production, outputs and wages. On the technical conditions of production, [22] Blaug points his finger at what he describes as the Sraffians'

[21] Some traces of a Whig interpretation might rather be detected in Blaug himself when he objects to "Sraffians" that "to treat demand and supply as [...] words that classical economists would never have employed in the explanation of natural prices is to take their *outmoded language* at its face value and indeed *deny any analytical progress in the history of economics*" (1987, 442, my italics). Blaug seems here to forget that for the "historical reconstruction" he so forcibly defends in 1999, "progress in economics" is hardly an argument for replacing Ricardo's "outmoded" notions of demand and supply with the modern ones *in an interpretation of his work.* (As for the substance of the passage I would stress, for my part, that most certainly the "words" demand and supply were frequently employed by Smith and Ricardo, but with a meaning quite different from the modern one, as Blaug himself contradictorily admits in the very passage: cf. par. 21 below).

[22] Blaug seems trapped by an ambiguity of the language when he writes: "It is perfectly true that the classical economists rarely addressed the problem of choice of techniques" (219) thus trying to explain an alleged "Sraffian" negation of the interest of those authors in the change of methods of production as distribution varies. When referring to "technical conditions of production" as (intermediate) data, I had intended, as I believe is usual under that expression, all known

"historical misrepresentation of technology as an exogenous variable in classical economics"(221),

which he then exemplifies by the

"utter indifference of Sraffian interpreters to the opening three chapters of the Wealth of Nations [on the division of labour], a subject they never discuss or even mention" (220).

In a work of mine which, furthermore, Blaug himself cites in his article, I had however written:

"Classical economists like Adam Smith or Karl Marx had thought that the technical conditions of production, and their evolution over time, were largely determined by phenomena with which economic theory had to deal, just as with wages and outputs"

and I concluded that such a determination constituted

"one of their central concerns (think of the analysis of the division of labour in Adam Smith)" (1990, 127). [23]

However, after that ill–directed finger–pointing, Professor Blaug (222) insists, and approvingly quotes the following old lines from Professor Rowthorn:

"If [...] our aim is to understand capitalism as a mode of production and to [...] reveal the economic law of motion of modern society it is ridiculous to concentrate exclusively on quantities which appear in the equations of Sraffa and other Neo–Ricardians [...]. It is emphasis on the labour–process which characterises

alternative *methods* and not only those which happen to be in use in a given price situation (cf. *e.g.* 1984, 293 and n.). To the extent to which it is present in those authors, the choice of techniques is indeed a typical question of the "core" of their theory, in that its treatment can be strictly quantitative in nature and is a necessary part of the determination of the relative prices and the non–wage distributive variables. Blaug may however have been led into his incorrect interpretation of the given "technical conditions" of production, by his idea that the degree of freedom with respect to distribution in Sraffa and the classical economists is founded on the absence of alternative methods of production (see n. 26 below).

[23] In the same pages I noted in more general terms how "by limiting the scope of the quantitative relations of known general properties to those of the core, the classical economists [...] allowed the study of [...] social and historical factors to be an integral part of economics"(*ibid.* 125–6), something that also seems to have escaped Professor Blaug's attention when in his article he finds the "Sraffian" interpretation overlooking the social and institutional elements of classical works. (Let me take this opportunity for an errata corrige. In 1990 (126) the editor replaced by "*tendentious* exclusion" the bad English of my original phrase "*tendential* exclusion" in which I had however meant to indicate the bias against historical factors embedded in the logical structure of neo–classical theory when it confines the possible influence of those factors to technical conditions, tastes or endowments — "ultimate" data, the study of which falls largely outside economics).

> Marx's analysis, and which, more than anything else, distinguishes him from all main schools of bourgeois economy" (Rowthorn, 1974, 39–40).

Nobody of course is expected to concentrate "exclusively" on the price equations, except perhaps when, with Sraffa, we are drawing out properties of those equations like the phenomenon of the "reswitching of techniques" which had gone unnoticed for a very long time in mainstream theory — the same properties which are basic for criticising what Professor Rowthorn is content to dismiss as "bourgeois economy". As for the particular point of Marx's labour process, I had argued elsewhere (1986, 179–86), with specific reference to Rowthorn's above argument, that it is the very treatment here described as "intermediate data" that allowed Marx to deal with the dependence of the technical conditions on social circumstances, as he does in his analysis of the labour process.

IV. On the Levels of Output

11. As we come to outputs, we find that Blaug brings out the need to clarify an important but frequently misunderstood point in Sraffa (1960). We said that Smith's and Ricardo's treatment of outputs or wages or technical conditions as data in the "core" of their theories in no way entailed that those authors failed to determine the changes of such circumstances within the theory as a whole. This may seem to be contradicted with respect to outputs when, in his (1960) Preface Sraffa writes:

> "no changes in output and [...] no changes in the proportions in which different means of production are used by an industry are considered in this book" (p. v)

and states that this was

> "the standpoint of the old classical economists from Smith to Ricardo".

Outputs, it may then appear, are "ultimate" and not "intermediate" data in the classical "standpoint" since the latter would exclude changes in them, *i.e.* exclude, as Blaug notes, the treatment of output changes *within* the theory. However, as the oddity of this interpretation might lead one to expect, Sraffa's point is quite a different one: it only regards that "core" of the classical theory, with which his 1960 book is almost exclusively concerned — and *not* the theory as a whole.

Let me clarify further. In order to determine prices and distribution the modern economist has to introduce demand and supply functions for "factors" and products, the specification of whose points by means of "marginal products" or "marginal costs", or by other means, necessarily entails the consideration of (potential) changes in outputs, and in the proportions of factors. That implies that *changes* have to be included in the logical relations (equations) which determine *each position* of the economy. In his passage, Sraffa is therefore saying, in effect, that in his book, as in Smith and Ricardo, he is determining prices and the rate of profits independently of any demand–and–supply functions. Accordingly he has no need to include changes in outputs, or in the proportions of factors *within* the logical relations determining prices, unlike what happens with the "marginal method" which

> "requires attention to be focussed on change for without change either in the scale of an industry or in the proportions of the factors of production there can be neither marginal product nor marginal cost" (ibid.).

Nothing "grotesque" (223) of course in all that, once the sense of it is properly understood: nothing to do, that is, with denying the obvious concern of classical authors for capital accumulation or output changes of all sort. [24]

12. This leads us to an issue, which Blaug repeatedly raises in his article. It is that of the "moving equilibrium" which, he says, was overlooked, or at any rate underplayed, by "Sraffians", and which he thinks sets Ricardo's approach apart from the "common run of value theories"(226). The question, Blaug explains, is one relating to Ricardo's pre–eminent concern for "intertemporal" rather than "intratemporal" comparisons of value. And Blaug illustrates his point by an observation from Peach:

> "For most nineteenth century commentators [...] it has been common knowledge that Ricardo was particularly concerned to elaborate the dynamic course of [...] a declining general rate of profit in the absence of a free trade in corn" (Peach, 1993, 6).

It was only in the aftermath of Sraffa's book that, in Peach's view, emphasis came to be placed on Ricardo's

> "alleged concern with the notional division of a given social product between wages and profits, so as to determine the pricing of commodities in static equilibrium" (ibid).

[24] It may appear that the exclusion of "changes in outputs" or in "factor proportions" from the determination of normal prices and distribution is open to doubt with respect to Ricardo's determination of rent. However, Sraffa warns us in the continuation of the passage quoted in the text, that "caution is necessary [....] to avoid mistaking spurious margins for the genuine article"; and he illustrates that with the case of the "extensive margin", where rent results from a *difference* between the coexisting outputs per acre of different kinds of land, and not from a *change* in outputs. The same applies for the "intensive margin" in so far as rent similarly emerges from a difference between the coexisting outputs of two distinct methods applied on the same land (the case to which Ricardo appears to refer in *e.g.* 1951–73, IV, 14–15).

Now, no doubt Ricardo was much concerned with a "declining general rate of profit in the absence of a free trade in corn". And no doubt he also studied such a decline of the profit rate by what Blaug calls a "moving equilibrium" *i.e.*, by the usual method of "comparative statics", here applied to changes in the "intermediate datum" consisting of the level of the 'corn' output. However, for those very comparisons, each position of the economy with respect to the profit rate and *intratemporal* relative prices had to be determined first. It is therefore not easy to see where the contrast lies, between what Blaug describes as the "intertemporal" and the "intratemporal" comparisons of values (226): the two must necessarily go together.

It is also difficult to understand how one can contrast, as Peach does in his passage above, the "dynamic" course of wages, profits and rent noted by "Ricardo's nineteenth century commentators" on the one hand, and Ricardo's division of a given social product between wages and profits emphasised in Sraffa (1960) on the other. The latter is the necessary underpinning of the former: no such "dynamic" course could surely have been traced by Ricardo, without *e.g.* the basic proposition that, contrary to what Smith had thought, given the real wage and the (domestic) corn output, the rate of profit is determined. Indeed, Blaug himself has singled out that proposition as the already mentioned "Fundamental theorem of distribution" by Ricardo (cf. par. 2 above and 20 below).

V. On Wages

13. Blaug opens his section on wages by writing

> "we come [...] to the most disputable of all the three variables that are said to be given in classical economics, the real wage rate" (p. 225).

That "given" wage might have seemed less disputable had Blaug recalled *e.g.* the following passage:

> " the foundation of modern political economy [is] the conception of the value of labour power as something fixed, as a given magnitude" (Marx, 1905, I, 45; 1867, III, 859)

by an author whom Blaug includes among the classical economists and describes as "thoroughly steeped in the concepts and modes of thought of Ricardo" (1988, 29; see also 1999, 215). [25] However the point is that Marx never intended such

[25] Peach (1999, 458–60) apparently disagrees with Blaug here. He believes that it would "emasculate" Marx to follow "Sraffian" interpreters and view his labour theory of value as having what Peach apparently admits to be the basic role of the theory in Ricardo: allowing that is for a

a magnitude to be "given" from "outside [his] theoretical system" (228) — as Blaug would have to claim if he were to interpret the "givens" of Marx as he interprets mine. Marx understood the wage, about which he surely had much to say, as a "given" in precisely the sense I have described by the notion of "intermediate data". And he attributes that procedure to "modern political economy", *i.e.* to Smith and Ricardo. [26]

After that opening, Blaug enters into a discussion of classical *relative* wages — a question which could indeed have provided him with a further good exam-

determination of profits by *measuring* independently of distribution the value of the quantities (product, wages and capital) on which the surplus theory of profits operates. Overlooking passages by Marx stating rather unambiguously just that role (see *e.g.* those reported in Garegnani 1984, 304) Peach rests his negative case essentially on Marx's distinction between "concrete" and "abstract" labour, which he evidently deems the distinctive essence of Marx's theory of value. This is surprising since Marx himself tells us that that distinction of his, is based on Ricardo's own distinction between "Value" and "Riches", as drawn in Ch. XX of the *Principles* in order to dispose, as Marx puts it, "of the trivialities of a J. B. Say" (Marx, 1867, I, 84n). As I have argued elsewhere, the distinction is then developed by Marx in terms of the two species of labour in order to counter Say's successors, the "vulgar economists". By representing value as a *sum* of wages, profits and rents, rather than as something which resolves itself into them, and thus limits them, those economists confused, in Marx's view, the *abstract* labour, producing alone the value of commodities (*i.e.* measuring value independently of its three components) with the *concrete* labour, which *together with capital and land produces the commodities themselves* (Marx *ibid.*; the question is indicated in Garegnani 1984, 304 n. and discussed in 1981, 80–84). The basic difficulty in Peach's argument remains however that he does not explain what is the bearing of his considerations about the two kinds of labour on the way in which Marx has to in fact determine the rate of profits by measuring in terms of labour embodied the relevant aggregates, the object of that "Sraffian" interpretation which Peach intends to reject (par. 8 above). That interpretation has indeed been criticised also from the opposite side for having been tailored on Marx alone. Thus Professor Porta argues that "in Marxian theory the true fundamental problem of value and distribution is to have (as Sraffa says of Ricardo) *a measure of value* which would be invariant to changes in the division of the product"(1986, 444, our italics). Unfortunately, Peach and Porta bring little textual evidence to bear out their opposite opinions about "measuring" value in Ricardo versus Marx. This idea of a qualitative distinction between Ricardo's and Marx's value theories, common to both the above positions, reflects, I have argued, the "submergence" of the classical standpoint with which we are grappling in the text (Garegnani, 1981, 67–76).

[26] Blaug ascribes to "Sraffians" the view that the "classical economists must have taken the real wages as a datum because the logical consistency of their theory demanded it" (229). No references are given for the author(s) of such a view. I would myself have taken the causal relation to go in the opposite direction: that of a wage treated as an "intermediate datum" in Smith and Ricardo because of the considerations we shall see in par. 15 below — logical consistency then taking natural care of itself. Blaug's point appears however to relate to his other surprising opinion that "the degree of freedom" we find in Sraffa's equations of distribution and prices is due to the latter's assumption of fixed coefficients of production (Blaug 1987, 441; 1988, 22; see also above III, n. 22): Sraffa would then find a degree of freedom on his hands to be disposed of by attributing a given wage to the classical economists. The fact that the entire Part III of Sraffa (1960) which is devoted to the choice of techniques does not affect the "degree of freedom" should suffice to dispel any such idea. A demand–and–supply determination of the wage is surely no necessary result of the mere assumption of alternative techniques, as Blaug seems to believe.

ple of the method of "intermediate data" of Smith, Ricardo or Marx. Those authors took the ratios between the wages of different qualities of labour as "givens" for the purpose of the price relations of the "core", then leaving the ratios to be studied separately.

It is only after the excursus on relative wages that Blaug proceeds to the central part of his section, where he contrasts his interpretation of the wage in Smith and Ricardo with the alleged "Sraffian" interpretation of it as a "datum". His *description* of the concrete circumstances affecting wages according to the classical economists is the broadly non–controversial one of a subsistence–based wage trend resulting from the population principle (whether or not that trend is properly represented in terms of the *reversible* functional relations of *e.g.* Samuelson's "Canonical model", or rather by the complex, *irreversible* classical influences acting *e.g.* by changing the accepted level of subsistence along the path). Indeed the description is compatible with the representation of the wage as an *intermediate* datum, [27] the determination of the wage being in fact separable from the logically subsequent one of prices and the profit rate. What controversy can be about is the *mechanism* by which Smith and Ricardo supposed population and capital accumulation to achieve that wage trend. Indeed a difficulty, which does not seem to have been sufficiently attended to, arises here for the interpretation of Blaug and other authors. Summarising it [28] will allow us to go a little deeper into the basis of the conflicting interpretations of the classical economists we are discussing.

14. In effect, the reference to the population principle by Blaug and other authors in their interpretation of Smith's and Ricardo's wages, leaves quite open the key question of the exact mechanism by which those authors supposed population and capital accumulation to trigger off the wage changes adjusting the two to one another — whether by neoclassical–like demand and supply functions, or by some other process.

Unlike the impression often given, the relationships between the wage on the one hand, and the rates of growth of capital and of population on the other, are by themselves of little avail for interpreting in terms of demand–and–supply of labour on neoclassical lines the classical subsistence–based wage trend. For that interpretation the inverse relationship between the wage and the *rate of growth*

[27] We prefer the general expression "normal wage" to the more classical one of "natural wage", because Ricardo often attributes to his "market wage" a persistence which makes it correspond more closely to a "normal wage" than is the case of his "natural wage", determined by subsistence. However, the cultural element of subsistence stressed by Ricardo gives in fact more flexibility than is generally recognised to his position also on the natural wage — as when *e.g.* he writes: "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" (Ricardo, 1951–73, VIII, 169; cf. also *ibid*, II, 115). On the reasons for the apparent rigidity of Ricardo's position on wages cf. Stirati (1994, ch. V).

[28] A fuller discussion of this difficulty is contained in Garegnani 2000a.

of labour employment (capital) cannot replace the similar–looking, but quite different inverse relationship between the wage and the *amount* of labour employed out of a given capital, *i.e.* the proper neoclassical demand function for labour. [29] On the contrary, that interpretation rests on the existence of such a proper demand relationship. Given the labour supply rigidly determined by population, the elastic, proper neoclassical labour demand is indeed necessary if the demand–and–supply mechanism is not to lead to wages falling to zero, when population overtakes capital accumulation, while rising to their maximum for which quasi–rents on capital goods are zero in the opposite case; and, finally, remaining indeterminate when the two rates of growth happened to be in balance. Without the labour demand function preventing those effects, Blaug's interpretation (like that of Samuelson's 1978 "Canonical Classical Model", or of Hicks–Hollander's 1977, etc.) would not only contradict anything to be found in classical texts by zero wages, zero rentals or indeterminacy: it would fail to explain in logic the very feature of the classical economists' works it is intended to rest on, *i.e.* the tendency of the wage to the level for which population and accumulation can proceed in step. [30]

[29] The confusion between the two kinds of "demand" each with the respective "supply" emerges when it is held that "the breakdown of the combined return to the dose between its two components [of labour and capital] would be indeterminate as far as the *demand side* of the problem is concerned [....] The needed conditions come from the *supply side*" (Samuelson, 1978,1416, our italics). The "supply" and the "demand" sides here coupled are disparate being in fact part each of a different couple. The "demand side" causing indeterminacy relates to the proper neo–classical couple based on *absolute* quantities of the labour demanded and supplied out of a *given* population and amount of capital, but the supply is also vertical there and therefore of no use in establishing determinacy. Samuelson's "supply side" is instead that of the couple relating to *increments* of labour where *both* schedules are in fact elastic and contribute to determinacy.

[30] The issue is the one which Samuelson (1978, 1423) describes as "showing that the Canonical system is [...] globally stable for any initial conditions of positive [capital] *K* and [labour] *L*", where reference to the given quantities *K* and *L* makes it clear that here both demand and supply have nothing to do with the relations between the wage and the *increments* of capital and of population affecting the *trend* of the wage over time (n. 29 above). An even clearer recognition of the distinction of roles between the two kinds of relations comes from Frank Knight when he notes that Ricardo's theory "purports to explain and must explain, the division of the joint labour–capital share which actually takes place [...] and not one which merely states [...] a long run tendency" where the latter is that determined by the increments in population and capital. He has then to recognise that the explanation he expects, *i.e. one* along the lines of demand and supply functions, is not there to be found in Ricardo (Knight 1935, 81: cf. n. 31 below). And Samuelson also has to essentially admit that no proper demand function for labour is to be found in Smith and Ricardo. He however ascribes that to the fixed proportions between labour and "capital" in production, which those writers would be assuming, and introduces a vertical labour demand schedule which, coupled with an equally vertical supply defined by the existing population, would entail, Samuelson recognises, zero wages or zero quasi rents, or indeterminacy in the division between the two (1978, 1423). To obtain the classical wage *trend* ensuring that capital and population grow in step he has then to replace the rigid labour demand with the usual elastic neo–classical one on the ground that in "commentaries on current events" Ricardo and Marx

Now, the difficulty of those neoclassical interpretations is that no such elastic, proper labour–demand can be found in either Smith or Ricardo. The non–existence of that demand function and of the corresponding neo–classical mechanism is made clear by Ricardo's admission of permanent labour unemployment (par. 2 above and 20 below). And no confusion should here be caused by the wage–fund theory in the form it took *after* Ricardo, in particular with J.S. Mill. Nor should doubt be cast on the matter by the quite different notions of "demand" and "supply" of labour often referred to by Smith and Ricardo in connection with their "market" wage. As revealed by their reference to the "proportion" between the demand and supply of labour (*e.g.* Ricardo 1951–73, I), and as exemplified by the analogous analysis for product prices, *single quantities* and not *functions* are there meant by those authors when referring to "demand" and "supply" (and we shall see in par. 21 below how Blaug himself has to acknowledge that). "Demand" for labour is therefore the broadly defined given employment possible at the stage reached by the accumulation of capital, (*i.e.* by the "fund for the maintenance of labour": see *e.g.* Ricardo 1951–73, I, 165), and the "supply" is defined equally broadly by the amount of population — the two providing some measurement of labour unemployment or underemployment, but certainly no schedules determining the "natural" or normal wage by their intersection.

15. If this is correct, no mechanism based on labour demand and supply schedules on neoclassical lines can trigger in Smith and Ricardo the wage changes adjusting population to capital accumulation. But then, we may ask, *by what other mechanism* did Smith and Ricardo envisage the adjustment to operate?

I have argued elsewhere that a key to the answer may be found in the numerous passages by those authors, which devoted to wages, have caused puzzles to modern historians of economic thought. I am referring in particular to Smith's observations on the existence of a minimum wage, or about "combinations" of "masters" or "labourers", whether "tacit" or explicit, imparting "advantages" to the respective parties in the determination of what yet appear to be competitive wages.[31] And sim-

admitted variable proportions of labour to "capital" (1978, *ibid*). Hicks–Hollander (1977) and Casarosa (1978), on the other hand, are less careful than either Knight or Samuelson and *assume* straightaway that in Ricardo there is an elastic demand for labour, whether in the form of J.S. Mill's wages–fund doctrine or in some neo–classical form.

[31] Thus *e.g.*, with respect to the passages about minimum wage, "advantages" or "combinations" of "masters" Edwin Cannan wrote: "if the combination of masters has the power of depressing wages with which it is credited, why should it leave the labourers enough to support a family?"(Cannan, 1917, 185). Similarly, Knight interprets the same "advantage" as the negation of any "impersonal mechanism of market competition" fixing wages and comes to the conclusion that "Ricardo's subsistence theory rests on the deeper assumption that the employer capitalist makes the division arbitrarily" (1935, 185). These examples may suffice to indicate how little a closer inspection of the texts of those two classical authors reveals of a determination of the wage by any however primitive demand and supply *functions* for labour (for a fuller discussion of the issue see Garegnani 1990, 118–22, besides 2000a).

ilar difficulties are raised by passages on wages by Ricardo. Those passages may indeed be seen to point in the direction of a real wage determined by what Marshall described (in order to reject it) as "the relative strength of the competing parties". [32] That "strength" can then be seen to be broadly measured in Smith and Ricardo by what they called the "proportion" of the demand to the supply of labour — the two *single quantities*, which thus do not contradict but rather specify what is often called Smith's "bargaining theory" of wages (*e.g.* O'Brien, 1975, 111). [33]

The relative speeds of growth of population and capital, with the resulting varying such "proportion" of the demand to the supply of labour, *i.e.* the varying pressure on wages of labour underemployment in its several forms, can then be envisaged to affect that "strength". Those relative speeds can accordingly be seen to trigger the variations in wages adjusting population to capital accumulation — the "supply" to the "demand" for labour in Smith's and Ricardo's sense — without passing through any labour demand and supply *functions* and the corresponding mechanism along neo–classical lines. [34]

VI. Wages and the Analysis of Outputs in Smith and Ricardo

16. The above separate determination of the wage is central to Smith's and Ricardo's analysis: it leads to a similarly separate determination of outputs and, therefore, to their characteristic determination of the non–wage distributive variables as a surplus. [35] The essential point here is that the absence of demand

[32] Marshall, 1920, Appendix J, 825. For passages by Ricardo pointing in that direction see those on the immediate transfer to profits of a tax on wages (cf. *e.g.* Shoup,1960, 64–77; 140–2) and others discussed in Garegnani 2000a.

[33] Indeed, the way out of these puzzles adopted by several modern interpreters seems to consist of tracing in Smith, where the position of the old classical economists is more fully articulated than in Ricardo, several conflicting theories of wages (cf. *e.g.* Cannan 1917, 184; Knight 1935, 83 and *passim*). Taking it for granted that Smith's and Ricardo's references to demand and supply of labour relate to (elastic) *functions*, and not to the single quantities they in fact refer to, and deriving some comfort in that from the *later* wage–fund theories (par. 14 above), those interpreters expect the tendency to a subsistence–based wage trend to derive from a usual demand and supply mechanism. The contrast between this and passages like those we saw in Smith impose then admitting a second conflicting "bargaining" theory of wages — which, it is here contended, is in fact one and the same with Smith's "demand and supply" correctly understood.

[34] A discussion of how this "relative strength" can help in explaining the role Smith attributed to habits or "combinations" in providing "advantages" to either of the competing parties in wage bargaining is contained in Garegnani 2000a. There it is also argued that these elements were taken by Smith and Ricardo as compatible with a competitive labour market since they allowed for the freedom to buy labour from, and sell it to, anybody one wished, and therefore for that tendency to uniform wages, which constituted the essence of classical labour competition (cf. also Levrero, 2003).

[35] Cf. par. 3 above. As noted by Marx, in Adam Smith the question of the determination of non–wage shares as a residual is complicated by the problems he had to face for his extension of

and supply functions for *labour* is strictly associated with a similar absence of demand and supply functions for *products.*

In a Marshallian long period, the demand functions for products can determine prices only through supply prices, and in neo–classical theory this essentially occurs *via* distribution, *i.e.* the relative demand of factors of production and its effect on their service prices. [36] The classical theory of wages would therefore have in any case deprived of that direct effect on prices any "supply function" linking the rates of wages and profits to the levels of outputs. On the other hand the effects which outputs may have on supply prices through the *alternative route* of increasing returns from division of labour, or decreasing returns from scarce natural resources, were seen by the classical economists to fall largely outside the sphere of the determination of prices, and to be relevant chiefly in an analysis of growth and capital accumulation.

That clearly deprived any conceivable demand functions for products of relevance for determining prices. But it is not all: the classical theory of distribution further undercut any potential relevance of demand functions by doing without the equality between factor demands and supplies which renders determinate the individual incomes and with them the neoclassical individual and collective demand functions for each product.

It is only natural, then, that the notion of a predefined functional relation between prices and quantities demanded of products should have remained foreign to Smith and Ricardo, where it would have had no clear general determining role on prices nor, even, a sufficiently definite basis in individual incomes. This absence of demand functions for products in Ricardo and Smith has indeed been widely if not always transparently acknowledged. [37] The connection with

the Physiocratic notion of *produit net* to manufactures. That extension entailed replacing the essentially homogeneous agricultural quantities of Quesnay's *Tableau* with value quantities, and introducing profits as a second share of the "surplus". In Marx's view, this led Smith to overlook in parts of his work the limit set to the distributive variables by the amount of product to be shared out, thus falling into the logical inconsistency which Marx saw to be the hallmark of the "vulgar economy" (Garegnani 1984, 300–4; indications in Smith of a quasi–Physiocratic view of land rent as the ultimate residual, *after* deducting profits at a rate separately determined by what Smith saw as the "competition of capitals", are discussed in Garegnani 2000a, n. 25). It was Ricardo's achievement to overcome those difficulties by, on the one hand, "getting rid of rent" thanks to Malthus's treatment of it (Ricardo, 1951–73; VIII, 194) and, on the other, providing a measurement of the product independent of its division between wages and profits (Marx, 1867, III, 854; Sraffa, 1951, xxxii). These basic issues in the history of economic thought seem to be overlooked in Peach (1999) when, in rejecting the interpretation of Smith as a surplus theorist, he refers to a determination of profits and rents by Smith in what is there described merely as a "sphere of circulation"(1999, 454; cf. also n. 25 above).

36 Contemporary economists have been reminded of this by the "non–substitution theorem".

37 As Barkai (1967,75) writes, it is "the accepted interpretation" that "a demand relation [...] is [...] alien to Ricardo's approach" (cf. also Stigler, 1965, 449; and par. 21 below). We may incidentally note here how the specific point under discussion between Barkai and Stigler is in fact the same raised in Blaug (1999) when he writes with reference to the "demand" for corn "but if this

their theory of distribution has not however been pointed out engendering in the modern economist the impression of a gap in their theorizing, rather than that of the alternative system that is really there. [38]

Now, the absence of demand functions could not but entail a treatment of outputs radically different from the modern one. The several circumstances on which outputs and their changes depend (level of activity of the economy, [39] technical conditions, distribution of income, individual choices, etc.), which in neo–classical theory are seen to act together *through* the demand functions for products resulting from the system, were instead considered by classical authors — when relevant — in the distinct effects they could have on outputs either directly or through the remaining "intermediate data", namely wages and the technical conditions of production. Changes in outputs [40] faced then the classical economists with a complexity and dependence on the specific circumstances of the changes which quite naturally led those authors to a treatment of outputs by the inductive methods of the second of the two fields of analysis we distinguished in par. 7 above — and therefore, to taking them as "intermediate data" in the "core".

And if the treatment of outputs as intermediate data did away with *demand functions* for products, it equally did away with *supply functions* too and with the Marshallian "returns" affecting their shape. Outputs were taken as given when determining prices. "Returns" would become relevant only where output changes were sufficiently important to be considered and, therefore, chiefly in the theory of accumulation, outside the "core", and according to the circumstances of the changes. Thus *e.g.* Ricardo had no particular need to assume either the constant returns in manufactures or the rigid demand for corn which Marshall and subsequent interpreters ascribed to him (par. 7 above). It is only the presupposition of demand functions that makes constant returns in manufactures, or price rigidity of agricultural demand, appear necessary for separating the determination of prices from that of outputs. [41]

demand varied with the price of corn, the action of diminishing returns on the price of corn might be offset by intercommodity substitution in consumption away from corn" (224). Blaug does not note that the offsetting (*i.e.* the fall in output) can never be complete because it can only occur *if some price rise remains*, a point which also Barkai and Stigler apparently overlook in their discussion.

[38] A fuller discussion of the connection between determination of wages and outputs in Smith and Ricardo, is conducted in Garegnani 2000a.

[39] Of course, the level of aggregate activity and labour employment in the economy may also be seen as an "intermediate" datum in Smith and Ricardo, implied as it is in the levels of the individual outputs and in the stage reached by capital accumulation.

[40] The conventional mathematical presentations make it natural to envisage the determination of outputs and wages in neo–classical theory, or also that of non–wage distributive variables and prices in classical theory, as referring to *absolute* levels, though what we can hope to ascertain are generally *changes only*, and often just the *signs* of those changes. The less formalised classical determination of the "intermediate data" is more naturally envisaged as being *directly* a determination of the changes occurring between the two positions referred to.

[41] See above n. 16. It is interesting to notice how for that attempted reconciliation, Marshall has

17. The above suggests taking now a closer look at the meaning of the expression "separate determination" we have been using for the "intermediate data" of classical theory. The meaning can perhaps be best explained by contrasting the two adverbs 'separately' and 'independently'. A determination of outputs and prices separately from each other does not mean that influences of prices on outputs are excluded, any more than reverse influences of outputs on prices are. As we shall see below (par. 21) normal outputs (equal to Smith's "effectual demands") are in fact determined with reference to a specific price, just as natural or normal prices are determined with reference to a specific output. The separation between their respective determinations only meant that, for the reasons we saw above (parr. 7 and 16), the influence of prices on normal outputs (or "effectual" demands) were not seen to provide any useful general set of quantitative relations (like the demand function of neo–classical theory) entailing a simultaneous determination of the two. Similarly the separate determination of the wage did not imply that classical authors necessarily supposed the real wage to be independent of product prices and obviously price determination was not independent of the wage. The same two–way relation was true for the technical conditions of production. It was also potentially true between any two of the three intermediate data themselves.

The above in effect means that the quantitative relations between distributive variables and prices analysed in the "core" of the theory, were woven into an analysis conducted to a considerable extent by the different methods appropriate to the circumstances affecting the "intermediate data" among which socio–institutional and more generally historical factors played an important part. A reading of the *Wealth of Nations*, or of Marx's *Capital* makes clear what we mean. [42] And with this we have come full circle to confirm what we indicat-

to attribute to Ricardo *two* assumptions; Occam's razor would suggest eliminating instead just *one* hidden assumption: that of the existence of demand functions in Ricardo. As noted already (n. 5 above), the idea of a demand function is *not* one and the same with that of an influence of prices on the quantity demanded. Thus, when Blaug writes "Marshall's [...] diagrams of demand and supply [...] imply nothing whatsoever about the truth or falsity of any particular theory" (1987, 442), he confuses, as is generally done, those two distinct ideas, and thus what are essentially facts (the possible influence of prices on the quantities demanded) with a particular theory of them (diagrams of demand and supply).

[42] The frequently expressed idea that Ricardo did not share Smith's or Marx's openness to broad socio–historical factors can be seriously misleading. A broad distinction is useful in this respect between those economists who are concerned with the elaboration of a theoretical system, like Adam Smith, J. S. Mill, Marx or Marshall, and those who are instead concerned with some central questions arising within an already broadly established system. Ricardo clearly belongs to the second category, and he deferred to Smith for everything that did not abut on the relationships between wages, profits and rent on which he innovated (in this sense see *e.g.* O'Brien, 1975, 43). He can therefore be seen as sharing with Smith an openness to socio–historical factors which is indeed engrained in the system because of its explanation of wages and capital accumulation. (The above distinction may also be useful with respect to Sraffa who, I believe, belongs to the same category as Ricardo and like him essentially defers to the system of Adam Smith, beyond that "core" of the theory with which he, even more than Ricardo, was almost exclusively concerned).

ed at the beginning of this Comment as to how — far from providing an "amazingly narrow" view of the classical economists seen through "the lenses of the linear production model" (233) — the "Sraffian" interpretation uncovers the roots of the historical richness of their works, which Blaug rightly stresses, but in fact leaves unexplained in its analytical basis. [43]

This leads us to note how the "intermediate data", and the reasoning by stages they entail, strikingly respond to that need for "short chains of deductive reasoning" and "specific experience" which Marshall saw to be required by the subject matter of economics (1898;1920, 773) — the same need he tried to satisfy by his method of partial equilibria, a basis for the subsequent success of marginalist theories.

We are perhaps now in a position to appreciate also how the absence of demand and supply functions for products in Smith and Ricardo, far from being a sign of primitivism, should rather be seen as part of a consistent alternative view of how the economy works. High among the advantages of this alternative approach – apart from its freedom from the basic difficulties of the marginalist system to be recalled in the next section (par. 19) – is in fact its independence from the "reversibility" of changes which neo-classical demand and supply relations postulate. It is well known how the difficulties of that reversibility, which a perceived lack of alternatives seems to have led the profession to forget, were deeply worrying to Marshall who acknowledged that "in economics every event causes permanent alterations in the conditions under which future events occur": [44] Smith, Ricardo and their predecessors had instinctively taken good care of that.

VII. Conclusions

18. What we have seen about the classical "standpoint" which Sraffa declares he adopts for his *Production of Commodities* (1960) should have made it clear already that Blaug cannot be correct when he comments on "how thoroughly Sraffa is [...] steeped in Walrasian general equilibrium theory" (229; also n. 15 above). I shall now follow Blaug from history of economics to economics itself for a brief consideration of the question.

Blaug seems here to have been misled by a mere resemblance between the equations in Sraffa (1960) and those in Walras's *Elements*: both authors, it is

[43] Cf. par. 8 above for the misplacement of domains of interpretation which may partly explain Blaug's remark about "amazing narrowness".

[44] Cf. Marshall, 1879 Part II, 12 where he refers to production and supply. In Part I of that work he had however exemplified as follows with respect to demand functions (often believed to be less affected than supply functions by irreversibility): "any [event] that increases the amount of English wares of any kind that are consumed in Germany, leaves behind it a permanent effect in an increased familiarity on the part of German consumers with English wares, and in this and other ways occasions permanent alteration in the circumstances of demand" (1879, Part I, 26).

true, deal simultaneously with the prices of all products in the economy, but the equations they advance differ radically. Sraffa's are simply the algebraic expression of what Blaug himself says classical theory focuses on, namely:

> "long period equilibrium prices characterised by a uniform rate of profit on capital, uniform rates of pay for every different type of labour, and uniform rent per acre for every qualitatively different type of land, in short, what Smith called natural prices"(223),

quite a different matter from Walras's system of equations, which — while including the equations corresponding to those of Sraffa, which are valid for all "normal", long period, competitive prices, classical or neoclassical — are characterised by the equalities between demand and supply of 'productive factors' for which Blaug would search in vain in Sraffa (1960). The use of similar mathematical instruments should not hide the radical difference between the forces underlying the distribution between labour and capital in the two cases: the quasi–natural forces of a substitutability between "factors of production" in Walras, and the institutionally and historically based forces of the classical economists and Sraffa.

19. Unawareness of these differences comes into full evidence when, towards the end of his paper, Blaug appears to charge the classical revival with the same "sterile formalism" focussed, he thinks, on the existence of, rather than the convergence to, the equilibrium, which he sees in the contemporary Arrow – Debreu general equilibrium theory (229).

I indeed sympathise with Blaug's stress on the central importance of showing that prices and outputs "converge to an end–state equilibrium" (229). On occasions I went further and argued that without that "convergence" — which I take to mean a tendency to the position determined by the theory whatever the economy's initial situation — the theory itself cannot but be in question, given the stability of observed reality in the sense relevant here. [45] Blaug overlooks however the very different terms which the question of "convergence" assumes in the two theories and attributes to the classical revival difficulties which only pertain to the neoclassical one.

The true centrepiece of the problem of convergence to a normal position can be traced, I submit, to the way in which the wage is determined. Given a stable wage, competition can be shown to generally ensure the tendency to a uniformity of rates of profits and rents and, thus, the tendency to normal prices and the corresponding configuration of the economy. This suffices for classical theory where the stability of the wage is ensured by its institutional determinants. [46] The problems arise for the neoclassical theory where the stability and uniqueness of the normal wage come to depend on the shape of the labour

[45] Cf. Garegnani 1970, p. 425–6; 1990b, p. 43–4, 70, 76.

[46] Doubts about Smith's conclusions on the tendency of actual towards natural prices,

demand and supply functions. And it is there that we meet the difficulties connected with the "income effects" and, above all, those arising from reswitching and reverse capital deepening which have been pointed out in the course of the capital controversies, [47] and are clearly irrelevant for classical theory, where demand and supply of labour have the altogether different meaning we saw in par. 15 above.

Similarly Blaug fails to see how the "sterile formalism" he laments in contemporary theory has little to do with the classical revival when the terms of the latter are not confused with those of neoclassical theory. That "formalism" has indeed been pointed out by some on the classical side as essentially a symptom of neoclassical unravelling. [48] We come here to the difficulties besetting the notion of capital as a "factor of production". The recognition since Hicks (1939) of the impossibility of consistently conceiving the capital endowment as a single quantity, [49] has brought to treating that endowment as a vector of capital goods in the Walrasian way. This however — apart from depriving of plausibility the factor substitutability on which the theory rests (alternative production processes differ by the kinds rather than the proportions of the capital goods) — conflicted with that uniformity of effective rates of return on the supply prices of the capital goods which is the necessary expression of their mutual perfect substitutability in the eyes of wealth holders, for whom they are simply providers of future income. [50] The imperma-

advanced by Steedman (1984), Nikaido (1985) or Lippi (1990), are discussed in Garegnani (1997, 140, 164–65, 167–69 respectively). We may here note how Blaug writes that "competitive prices are just competitive prices in Sraffa and not a word is wasted on telling us how we got there" (231). However, Sraffa's early reference to the distinction between "market price" and "natural price" (Sraffa 1960, p. 9) would seem quite unambiguous in deferring readers acquainted with classical terminology to Smith's analysis of competition (cf. VI, n. 42 above). Blaug (231) also writes that in 1990 (113) I refer to "perfect competition" characterized, in Blaug's view, by the horizontal demand for the product of individual firms. This is not correct. However, the question was irrelevant there because, when I argued that the notion of "free competition" was common to neoclassical and classical theory, I was simply referring to the competitive equality between prices and expenses of production which is indeed common to both theories. The "perfectly competitive" horizontal demand for the firm is, it appears, a device to which Marshall and neoclassical theory have had to resort to reconcile that competitive equality with the idea of a demand function facing the firm, the companion of the demand function for the industry. It could not, therefore, be there in the classical economists and is in fact not likely to find a place in the present revival of their theory.

[47] Cf. *e.g.* Garegnani 1970, 421–28 and for the similar difficulties in general intertemporal equilibrium, 2000b, 435–9; cf. also par. 19 below.

[48] Cf. Garegnani, 2000b, 406–9.

[49] It may be useful here to recall how in [1939] Hicks wrote "people [...] would often introduce into their static theory a factor of production, capital [...] supposing capital could be treated like the static factors" (1939, 116; the fuller argument is discussed in Garegnani 1976, 31–36) where by "static theory" Hicks chose to refer to the traditional theory, where changes were studied by the then usual comparisons of normal positions rather than by Hicks's "dynamics" characterised by the "dating" of variables, imposed, we shall argue in the text, by the impermanence of the new equilibria.

[50] Initially Walras inconsistently expected his notion of the capital endowment as a vector to

nence of the new equilibria resulting from that conflict, replaces then the persistence of the traditional normal position [51] and undermines the possibility of correspondence between theoretical and observable variables, holding when the latter can be taken as an average of actual values over sufficiently repeated transactions conducted on the basis of (nearly) unchanged data. And the same impermanence imposes introducing future price–changes in the equilibria, and hence a "dating" of the variables, preventing even in principle the relevant repetition of transactions and therefore the above correspondence.

Now these changes have inevitably resulted in the highly complex and highly formalised version of neoclassicism which at present dominates pure theory – very distant from the comparatively transparent terms in which capital as a single factor allowed the founders of the theory from Marshall to Clark or Wicksell to think that *e.g.* the basic properties of the uniqueness and stability of the equilibria would generally follow from the twin principles of decreasing marginal utility and productivity.

It therefore appears that it is the neoclassical recourse to the Walrasian conception of capital, once capital as the consistent single productive factor has resulted not to exist — and not an excessive search of rigour disregarding significance — that has imposed the formalised, opaque and non–realistic version of neoclassicism dominant today in pure theory. [52] In fact that version appears to have become dominant only when the first phase of the capital controversy had rendered unsustainable the neoclassical normal prices of the Pigous, Robertsons, Champernownes openly relying on capital as a single factor.

It is not surprising then, that the "sterile formalism" of modern economic

be compatible with the normal prices of the traditional equilibrium characterised by a uniform effective rate of return on the supply prices of the capital good (on the inconsistency in Walras's original system and on the new Hicksian notions of equilibrium, cf. Garegnani 1960, Part II, ch. II, III and appendix G; see also 2000b, 393 n. for the notion of uniform *effective* rate of return defined under conditions of changes of relative prices over time). The perfect substitutability of capital goods for savers is stressed by Walras himself when he treats savings as demand for the single commodity "perpetual future income" (Walras 1954, par. 33; cf. also Garegnani 2000b).

[51] This change in the setting of the theory is often described today as a move to "general equilibrium theory". However Wicksell (1906) was about general equilibrium no less than Hicks (1939), and so fundamentally were Hicks (1932) or Marshall (1920) taken as a whole, or Pigou (1932), etc. The move has rather been to a *different* notion of equilibrium and therefore of general equilibrium (n. 49 above). Blaug interestingly notes how in the years before the First World War general equilibrium theory (clearly meaning here the Walrasian species) was "generally greeted with unmitigated hostility from the economics profession because too mathematical, too abstract, too impractical" (Blaug, 1996, 77). He ascribes the subsequent shift to general equilibrium to Lange (1936–37) and to the "Socialist calculation debate": however in (1935) Hicks had already advanced the basic elements of the "dated", general equilibrium approach of his (1939) book, apparently in response to the problems of capital he had met in his (1932) *Theory of Wages* and Shove (1933) had pointed out to him (cf. Garegnani 1976, 33–4 and n. 13).

[52] The warning by Malinvaud (1991) about the danger of a "new scholasticism" in the centenary issue of the Economic Journal, seems significant in this respect.

theory lamented by Blaug (1996, 76; also 229) should find no basis in classical theory where no reason exists for abandoning the traditional normal positions of Smith, Ricardo, Marx or Sraffa, and where in any case the absence of the demand and supply equations much simplifies the strictly analytical part of the theory. Blaug reverses cause and effect when he traces contemporary "sterility" not to the theory which has generated the complications, but rather to the mathematical instruments arrived at in order to try and cope with them. Indeed it may be wondered whether the opacity of present–day pure theory has not favoured, so far, rather than undermined, the continued acceptance of the neoclassical demand–and–supply framework (which Blaug shares, and would merely wish to be differently developed, overlooking the failures which have caused the formalism), by obscuring the unchanged difficulties which underlie the approach as such [53] and fostering the comparatively comfortable, if unfounded, belief that the negative results about uniqueness and stability can only stem from the "income effects", long discovered and with which the theory has managed to coexist.

What is however immediately relevant for us here is the general point that Blaug's focus on alternative instruments rather than alternative theories, brings him to view the difficulties of general equilibrium theory as affecting present–day developments of classical theory, where they are entirely absent. The extension is implied, rather than argued, by simply referring to a resemblance between the advanced mathematical tools which have sometimes been used also in the classical camp. Thus *e.g.* Blaug criticises "Sraffians" for not using Marshall's demand and supply apparatus in their analysis of stability (231) which, we may now see, is like reproaching them for relying on a theory where convergence can be supported, rather than on one where it cannot. Indeed, once the differences between the two approaches are clarified, together with the real causes of the difficulties which he perceives, Blaug might come to realise that the "Sraffian minidebate" (231) as he describes it, might hold more long–run importance than he seems to realise now. [54]

[53] On the implications of the difficulties concerning capital for contemporary intertemporal general equilibrium cf. Garegnani (2000b, 443).

[54] Cf. n. 46 above. The above indicates, incidentally, how I would remedy what Blaug once described as the Sraffian "astonishing failure" to explain why "Sraffian economics is better than any other economics" (1988, 45). For those convinced that the approach which became dominant at the end of the nineteenth century has run into basic difficulties, with respect both to premises like those mentioned in the text, and to conclusions like the tendency to the full employment of labour, it is natural to think that the thread has to be taken up where it was left in the immediate post–Ricardian period. All the more so when, in Blaug's own words, that period was one in which "the sharp political edge [of Ricardo's system] clearly worried so many of the minor classical economists" (1987, 438) and "much of the theoretical development [after 1831] was the result of a more or less conscious effort to counter the spread of Socialist ideology" (Blaug, 1958, 140) — not the best background for the basic theoretical choices made at that time and which gradually brought to modern economics.

20. Returning now to the history of economics, we may use our excursus into theory for one observation. Blaug's failure to distinguish Sraffa's analysis sufficiently from the neoclassical theory of Walras is one and the same thing with the difficulty we noted in introducing this "Reply" of recognising the existence of an alternative theoretical approach in the old classical economists (par. 2 above). Before closing, we may take a closer look at the matter.

We saw how Blaug recognises the presence of permanent labour unemployment in Ricardo, a feature basic enough to suggest by itself a theoretical paradigm alternative to the modern one. [55] We have also mentioned Blaug's references to Ricardo's "Fundamental Theorem of Distribution" and "corn model". In this "theorem" and "model" the wage emerges as an independent variable, with the outputs (and therefore the position of the "margin" on the land) and the technical conditions of production also treated as (intermediate) data, so that profits can emerge as a residual or surplus. [56] Indeed it is Blaug himself who elsewhere notices how, unlike in modern theory, "factor pricing in classical economics is invariably explained on different principles from those governing the pricing of products" (1987, 443). Is it not then, that "factor prices" are determined, as in the just mentioned "corn model", with the non–wage distributive variables as the residuum or surplus [57] argued in the "Sraffian" interpretation?

21. However the most striking expression of Blaug's difficulty in envisaging the possibility of an alternative classical paradigm is found when he himself unwittingly traces in those authors the very absence of demand and supply *functions* which he qualifies as a "distortion" when he reads of it in "Sraffian" authors. The distortion, he has argued, would lie in the idea that

> "any appeal to the forces of demand and supply in determining prices is necessarily alien to classical economics and that classical natural prices have nothing whatsoever in common with Marshall's long run normal prices"(Blaug, 1987, 442)

where, as the reference to Marshall makes quite clear, the "forces of demand and

[55] Blaug noted how "Ricardo assumed the existence of Marxian unemployment" (1958, 179) and with evident relevance for the old classical economists in general, he also contended that "in an era when the number of individuals on public relief hovered steadily around [...] ten per cent of population [...] the existence of a hard core of surplus labour must have been taken for granted" (1958, p. 75; Samuelson's attempted denial of Ricardo's admission of permanent labour unemployment in his chapter XXXI "On Machinery" in the *Principles* is discussed in Garegnani 2000a).

[56] Cf. n. 5, above. Indeed Blaug admits that: "there can be little doubt that this [surplus approach] is precisely the nature of the economics of Physiocracy [and] it captures much of the [...] *Wealth of Nations* [...]. On the other hand it does not begin to do justice to dominant features of the Ricardian system". But the only argument Blaug brings there for his thesis on Ricardo is "the law of comparative advantage" in international trade (Blaug 1987, 439–40) about the significance of which cf. above, n. 19.

[57] The "long period equilibrium prices" of Blaug's other passage in par. 18 above, resulting then in the way we see in Sraffa (1960).

supply" are the neo–classical ones and not the quite different classical concepts that we shall presently consider more closely.

A few lines after that passage Blaug has however to notice that Ricardo and Marx

> "propagated the misleading idea that demand–and–supply–explanations only pertain to market prices"

and, what is more that

> "Ricardo had a tendency to think of demand and supply as *quantities* actually bought and sold not as *schedules* of demand and supply prices" [58] (1987; 442, our italics),

a "confusion" in Blaug's opinion.

In drawing the implications of the latter passages for the alleged Sraffian "distortion" we may note first a striking oddity in these passages: Blaug surely knows that the "idea" that "demand and supply explanations" only pertain to "market prices" (*i.e.* to the deviations of the actual prices from natural prices) is systematically expounded by Smith in Ch. VII "Of the Natural and Market Price of Commodities" in the *Wealth of Nations* (cf. *e.g.* his 1987, 440) and, therefore, that Ricardo and Marx's "misleading idea" was simply taken from Smith's treatment of the question.

Equally and even more importantly, Blaug surely knows that it is from that same systematic treatment by Smith that Ricardo got his "confusion" concerning demand and supply as *single quantities.* [59] In fact Smith defined demand as

> "the [effectual] demand of those who are willing to pay the *natural price* of the commodity" (Smith, 1776 I, 49, our italics),

a point, and not a schedule in a price–quantity diagram — with the supply as also the single "quantity brought to market". So, we may observe first that there is there a systematic position of the old classical economists and not some peculiarity of Ricardo and Marx.

But above all how can Blaug then maintain that those two *single quantities* of Smith, Ricardo or Marx were "forces" determining something other than what they said they determined by it, the market prices, *i.e.* the *deviations* from natu-

[58] Blaug's quantities "bought and sold" in this passage are clearly a slip, since they would be identically the same, and not Smith's and Ricardo's two distinct quantities of "effectual demand" and "quantity brought to market" jointly determining the market price.

[59] Recognition of this undermines of course the thesis which Blaug seems to share of a demand–and–supply tradition running from Adam Smith to J.S. Mill, and then on to Marshall, with Ricardo and Marx only as the "classical" authors standing outside that tradition (Blaug 1987, 437–9).

ral prices, and *not* the natural prices themselves that would have required *functions* and not single quantities? And where is the "most grotesque distortion" (1987, 442) which "Sraffians" would be committing when arguing that "the forces of demand and supply" were "alien" to classical economics in determining the "*natural prices*"? [60] Are not Smith, Ricardo and Marx saying exactly that?

But Blaug the historian contradicts Blaug the demand–and–supply theorist further when he writes:

> "In one sense it is of course true that Ricardo had no theory of how the level of aggregate output is determined, much less a theory of why output is made up of one set of goods rather than another. But then neither did any other premodern economist" (p. 223),

which means, I believe, that Ricardo and other "premodern economists" determined neither the levels of aggregate output by the demand and supply functions of factors, nor the composition of that output by the demand and supply functions of products. How did they treat outputs, then, if not by the method I described under the phrase 'intermediate data'?

[60] Blaug had in fact further contradicted that statement when he had written a few pages before about "Sraffa's demonstration that prices in his model are determined independently of demand is eminently classical" (1987, 435).

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