

# Understanding "Classical" Economics:

## A Reply to Mark Blaug\*

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Knowing as I do how much we are influenced by taking a particular view of a subject, and how difficult it is to destroy a train of ideas which have long followed each other in the mind, I will not say I am right ..., and therefore it is possible that five years hence I may think as you do on the subject, but at present I do not see the least probability of such a change for every renewed consideration of the question confirms me in the opinion which I have long held.

Ricardo to Malthus in a letter dated 29 November 1820  
(Ricardo, *Works*, vol. VIII: 311)

### 1. Introduction

In a paper published in *HOPE* Mark Blaug has put forward a critical assessment of Piero Sraffa's interpretation of the classical economists. He entitled his paper "Misunderstanding Classical Economics: The Sraffian Interpretation of the Surplus Approach" (Blaug 1999; in the following all isolated pages given refer to this paper). Blaug's essay is essentially a review

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\* We are grateful to Christian Bidard, Carlo Casarosa, Giancarlo de Vivo, Gilbert Faccarello, Pierangelo Garegnani, Christian Gehrke, Geoffrey Harcourt, Samuel Hollander, Peter Kalmbach, Philippe Mongin, Gary Mongiovi, Bertram Schefold and Ian Steedman for valuable suggestions and comments on an earlier version of this paper. We also benefitted from reading a comment, by Carlo Panico, on a previous version of Blaug's paper, delivered at a Conference in Rome in memory of Giovanni Caravale. We should also like to thank the participants of a session at the annual conference of the European Society for the History of Economic Thought, Graz, 24-27 February 2000, and of a seminar at the University of Rome III in May 2000 for useful discussions, and two anonymous referees for their remarks. The responsibility for any remaining errors and the views presented rests entirely with the authors. Neri Salvadori gratefully acknowledges financial supports from M.U.R.S.T. and C.N.R.

article commenting on the literature inspired by Sraffa's contribution, including some of our works (cf. Kurz and Salvadori 1995, 1998a, 1998b).<sup>1</sup>

Since elements of the uneasiness with the interpretation of "the old classical economists from Adam Smith to Ricardo" (Sraffa, 1960, p. v) under consideration appear to be shared by several historians of economic thought, Blaug's review article offers a welcome opportunity to discuss the matters in dispute. We engage in this debate in the hope and expectation that the differences of opinion may gradually be narrowed and a better understanding of the specificity and fecundity of the analysis of the classical economists emerge. Since the problems dealt with are both important and complex, it appears to be a prerequisite to a fruitful exchange to suppress any inclination to polemics and cheap rhetoric. Setting aside a few instances, we read Blaug's paper as an invitation to discuss the matters in dispute as scholars should discuss them: soberly and with a quest for truth. In this reply we deal only with those objections of Blaug that directly concern our writings or points of view shared by us. The emphasis will be on the classical approach to the theory of income distribution and relative prices.

Blaug's exposition is organized around two methodological issues: the distinction between rational and historical reconstructions, on the one hand, and the "core-periphery" metaphor, on the other. Sections 2 and 3 of the present paper are devoted to short discussions of these concepts. Section 4 deals with another preliminary question, that is, the role of analytical rigor in the classical economists. In this section we challenge Blaug's first main objection to the interpretation we endorse, namely, that the formalization of the classical approach to the theory of value and distribution and the concern with the consistency of an argument amount to "sterile formalism". In the following sections we turn to the substance of his criticism. Section 5 prepares the ground by summarizing what we consider to be the logical structure of the classical approach to the theory of value and distribution. Section 6 addresses Blaug's second main objection which is that the Sraffian interpretation fundamentally distorts the real

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<sup>1</sup> Blaug informs the reader that "everything in this article I have said before (Blaug 1987) but apparently to no purpose, because Sraffian writers have simply ignored my objections" (p. 216, fn. 2). Later in his paper, in the context of a discussion of Ricardo's search for an "invariable measure of value" and its relationship with Sraffa's concept of the "Standard commodity", he admits however: "I said exactly that in 1987 (Blaug 1987, 157). ... Kurz and Salvadori (1998b, 144-45) criticize me, quite rightly, for suggesting that Sraffa's [S]tandard commodity makes prices independent of distribution, which is logically impossible since the standard commodity is only a particular *numéraire*" (p. 227, fn. 11). For a careful critical scrutiny of Blaug's views on Sraffa, see especially Steedman (1975, 1995); see also Salvadori (1977).

concerns of the classical economists. In the light of the evidence put forward from the writings of the classical authorst it is shown that this criticism cannot be sustained. Section 7 scrutinizes Blaug's alternative characterization of the "core" of classical economics and, ironically, shows that he arrives at essentially the same view of the logical structure of the classical approach to the theory of value and distribution as Sraffa. Section 8 contains some concluding remarks.

## 2. "Rational" versus "historical reconstruction"

Blaug defines *rational reconstruction* as "the tendency to view history as a relentless march of progress from past errors to present truths" (p. 213); and *historical reconstruction* as an attempt "to recover the ideas of past thinkers in terms that they, and their contemporaries, would have recognized as a more or less faithful description of what they had set out to do" (ibid.).<sup>2</sup> His discussion culminates in the maxim: "I vote for historical reconstructions as *the only legitimate* occupation of historians of economic thought" (p. 214; emphasis added).

A definition as such cannot be true or false, but it can be useful or not. There are clear indications that Blaug's two definitions are not very useful. As regards the latter, Blaug stresses repeatedly that there is no such thing as a purely historical reconstruction of past thinkers. In one place he writes: "I admit that faithful historical reconstructions are literally impossible and that, of course, every new departure in modern economics leads to an inevitable tendency to construct a historical pedigree" (p. 232). Therefore he is bound to contradict his above maxim. He states that there is "a *perfectly legitimate* role for rational reconstructions" (p. 214; emphasis added); he even admits that there are "rational reconstructions [that] are valid historical reconstructions" (ibid.), although there are only "very few" of them. Clearly, according to his definitions this is impossible: a "rational reconstruction" cannot at the same time be an "historical reconstruction". Blaug does not use his definitions in a consistent manner and quickly distances himself from the maxim he proposes.

Quite different definitions can be obtained by consulting a well known paper by Lakatos (1971b) on the "History of science and its rational reconstruction". In this paper Lakatos first discusses four theories of the rationality of scientific progress and shows how each of them provides a theoretical framework for the rational reconstruction of the history of science. A rational reconstruction makes claims about the history of theories or analytical approaches to certain problems by focusing on how this history *should* have happened if the scientists had

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<sup>2</sup> It is not clear how Blaug thinks it could ever be known what "past thinkers" would recognize as "faithful" interpretations of their works?

been well-informed of the relevant developments, and had been perfectly honest and intellectually acute. It makes *empirical* claims, that is, claims that can (and should) be checked by the actual sequence of events, which is the task of an historical reconstruction. These claims happen to be inspired by a *normative* heuristics. The task of an historical reconstruction therefore involves scrutinizing whether earlier formulations of a theory have indeed been superseded by later ones because of their superiority or because of other reasons (including politico-ideological ones). Paraphrasing the first sentence of Lakatos's essay, which is a paraphrase of a famous dictum by Kant, it could be said that "Economic theory without history of economic thought is empty; history of economic thought without economic theory is blind". In Lakatos's words:

Each rational reconstruction produces some characteristic pattern of rational growth of scientific knowledge. But all of these *normative* reconstructions may have to be supplemented by *empirical* external theories to explain the residual non-rational factors. The history of science is always richer than its rational reconstruction. *But rational reconstruction or internal history is primary, external history only secondary, since the most important problems of external history are defined by internal history.* (Lakatos 1971b, 118; emphases in the original)

Lakatos added: "Whatever problem the historian of [economic thought] wishes to solve, he has first to reconstruct the relevant section of the growth of objective scientific knowledge, that is, the relevant section of 'internal history'. As it has been shown, what constitutes for him internal history, depends on his [economic theory], whether he is aware of this fact or not" (ibid., where we replaced "science" by "economic thought" and "philosophy" by "economic theory"). And later: "Internal history is not just a *selection* of methodologically interpreted facts: it may be, on occasions, their *radically improved version*" (ibid., 119).<sup>3</sup>

We insist with Blaug that historians of economic thought ought to apply the historical method with rigor. However, that method includes what in the introduction to our book we called "rational reconstruction" (see Kurz and Salvadori, 1998b, 1). We used the concept in a way that was inspired by Lakatos's disquisition.

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<sup>3</sup> The second part of Lakatos's essay is devoted to showing that as all scientific theories function as historiographical theories, at the same time they can be criticized by criticizing the rational historical reconstructions to which they lead. But this part need not concern us here.

We should like to add the following observations. First, the ideas put forward by an author are generally neither independent of what earlier and contemporary authors said or wrote nor are they without impact on the ideas of later authors. Ricardo tried to rectify views of Smith he considered erroneous and to solve problems Smith had left unsolved. Ricardo's analysis was the background against which John Stuart Mill and Karl Marx wrote; and so on. The history of economic thought is essentially a history about continuities and discontinuities in economic analysis over time. This requires rational reconstructions of the emergence, development, gradual transformation and occasionally the disappearance of ideas and concepts. It is significant that Blaug's main work on the subject carries the title *Economic Theory in Retrospect* (Blaug [1962] 1997). The book abounds with rational reconstructions (in the sense of Lakatos), and for good reasons.

Secondly, the process of the adoption of ideas and concepts of earlier authors is generally selective and there is hardly ever an adoption which does not also involve some adaptation. Therefore, we were puzzled to see Mark Blaug on the one hand enumerate several themes in Smith, Ricardo or Marx which play no role or only a small one in our reconstruction, and on the other hand accuse us of paying more attention to some themes than the authors mentioned. Analytical progress presupposes selection. We contend that every historian of economic thought, Blaug included, will in his or her interpretation of past authors proceed selectively. It simply does not make much sense to give all the problems an author was concerned with the same weight as he or she did.

Third, all contemporary readers of earlier authors are socialized in terms of some modern theory. This may be an effective hindrance to understanding some earlier authors, especially if the reader cannot refrain from seeing their works too narrowly through the lens of such a theory. The history of economic thought abounds with examples which provide badly distorted pictures of the contributions of earlier authors because of what we may for short call the triumph of modernity over the past. However, this need not be the case. It all depends on how meticulous the interpreter is and on how the modern theory under consideration relates to the earlier conceptualizations. A modern theory which shares a similar outlook on certain phenomena but provides a more correct and sophisticated conceptualization of their interrelation may turn out to be a powerful tool of interpretation, because it allows one to see and understand in some earlier authors what otherwise could not have been seen and understood. We interpret Blaug in this sense when he calls the "Sraffian" rational reconstruction "illuminating, ... capable of affording a springboard for a wholly new style of long-run equilibrium theorizing" (p. 214), "ingenious" (p. 218), "ingenious and even striking" (p. 219), and writes that "the core/periphery distinction is perfectly defensible for a rational reconstruction of classical economics" and "will stand" as such (p. 232). By comparing the literature before and after 1951 there is no doubt that Sraffa's interpretation has rekindled the

profession's interest in the classical authors and has greatly enhanced our understanding of their achievements.

### 3. The "core-periphery" metaphor

Blaug appears to have been deceived by the "core-periphery" metaphor. Garegnani (1984), to whom Blaug refers in this context, did not use the word "periphery". He rather distinguished between a "core" of classical analysis, that is, the theory of value and distribution, which is developed in terms of a variant of data (a)-(d) (see below, Section 5), and what is "outside the core". His basic idea, which appears to be similar to Ricardo's distinction between two spheres of economic analysis (see Section 4), is the following: "The multiplicity of these influences [outside the core] and their variability according to circumstances was in fact understood to make it impossible to reconduct them to *necessary* quantitative relations like those, studied in the 'core', between distributive variables and relative prices and between outputs or techniques and the dependent distributive variables and prices" (Garegnani 1984, 297; emphasis in the original). Hence, Garegnani's distinction concerns relations that can be formulated in precise, quantitative terms, on the one hand, and those that cannot, on the other. Blaug, on the contrary, interprets the metaphor as involving a totally different distinction, namely between what is important and what is not. He attacks the concept of the core against the background of his very different distinction and accuses Sraffian authors of regarding as unimportant or less important what in some classical authors was considered important or very important.<sup>4</sup> Obviously, in order for a criticism of Garegnani's idea of the core to be pertinent it would have to be shown that the classical economists held the view that what according to Garegnani belongs outside the core can also be formulated in precise, quantitative terms, or what lies in the core cannot. Alas, Blaug does not even make an attempt in this direction.<sup>5</sup>

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<sup>4</sup> Blaug contends that "To his credit, [Garegnani], alone among all Sraffian interpreters, denies that the core is somehow superior to or more significant than the periphery ..., but that is merely a fail-safe rhetorical device" (pp. 231-2). Yet he provides no evidence in support of either proposition. We should like to add that we never used the metaphor of the "core".

<sup>5</sup> Perhaps Blaug's misreading of Garegnani's concept of "core" is due to him projecting onto it some of the meaning the word "core" has in Imre Lakatos's philosophy of science. (See Lakatos 1978a; for Blaug's view on Lakatos, see Blaug 1980.) Within

#### 4. Different spheres of analysis in classical economics and the role of formalization

Blaug conjectures that the content of the classical theory of value and distribution is unavoidably betrayed by modern formulations of it because of the latter's concern with analytical rigor and mathematical formalization. In one place he objects to our efforts in this regard: "they simply cannot conceive of analytical rigor except in modern terms" (p. 233, fn. 13). As against this it should suffice to recall the untiring efforts of Ricardo and his followers to elaborate a coherent theory of value and distribution. As a contemporary noted, Ricardo "meets you upon every subject that he has studied *with a mind made up*, and opinions in the nature of mathematical truths" (Ricardo, *Works*, vol. V: 152, fn. 2; emphasis in the original). And in a letter to James Mill of 1 January 1821 Ricardo specified his own point of view *vis-à-vis* Malthus's explicitly as follows: "Political Economy he says is not a strict science like mathematics, and therefore he thinks he may use words in a vague way, sometimes attaching one meaning to them, sometimes another and quite different. *No proposition can surely be more absurd*" (Ricardo, *Works*, vol. VIII: 331; emphasis added).

Against this background the mathematical form of an argument in itself can hardly be taken as involving a break with the classical authors and especially Ricardo. Nor does it imply, as Blaug contends, "read[ing] Smith and Ricardo and Marx through Walrasian-tinted glasses" (p. 229). To classify Sraffa's counting of equations and unknowns as "pure Walras" (ibid.) mistakes the mathematical form of an argument for its substance. Blaug puts forward the following warning: "to pursue ruthlessly the goal of a watertight, mathematically consistent theory of price determination is to fall into the type of sterile formalism that has characterized general equilibrium theory in its modern Arrow-Debreu form" (ibid.). *A formalism is a formalism. Its qualification as "sterile" presupposes forming a judgement on the economic content conveyed by means of the formalism.*

We now come to the crux of the matter. Ricardo, as we have seen, was definitely and positively in favour of analytical rigour and mathematical precision in his "most favourite subject": political economy. Does this mean that he thought that economic laws could indiscriminately be established like mathematical truths with regard to *all* spheres of socio-economic life? The answer is clearly no. Indirect evidence is provided in the *Principles* where there is a striking contrast, in terms of whether strict, quantitatively knowable relations can be postulated, between the analysis of a *given* economic system and the distributive variables

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Lakatos's system, the "hard core" of a research programme is a set of propositions which are preserved from refutation. These propositions generate various theories.

and relative prices pertaining to it (see, in particular, chapter I), on the one hand, and Ricardo's probing into the problem of economic *change* (see, for example, chapter XXI), on the other. However, there is also some direct evidence available showing that Ricardo distinguished between different spheres of economic analysis and the capability of the theorist to establish economic laws within them. In his letter to Malthus of 9 October 1820 Ricardo took issue with the latter's definition of the subject and wrote:

Political Economy you think is an enquiry into the nature and causes of wealth – I think it should rather be called an enquiry into the laws which determine the division of the produce of industry amongst the classes who concur in its formation. *No law can be laid down respecting quantity, but a tolerably correct one can be laid down respecting proportions.* Every day I am more satisfied that the former enquiry is vain and delusive, and the latter only the true objects of the science. (*Works*, vol. VIII, 278-9; emphasis added)<sup>6</sup>

*No law can be laid down respecting quantity.* In Ricardo's writings this is especially reflected by the fact that when discussing the dependence of relative prices on income distribution, Ricardo proceeded in terms of *given output proportions*, setting aside any responses of outputs to changes in relative prices. This does not mean that in Ricardo's view there are no such responses or rather interactions between prices, outputs and income distribution. It only means that there is no reason to presume that the theorist can expect to find general laws expressing their interrelations, as they are postulated, for example, in neoclassical *demand functions*. The economist simply cannot avoid studying the historical particulars of an economic change – whether it is predominantly due to the introduction of a new method of production and whether this affects the system of production as a whole or is

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<sup>6</sup> Malthus replied on 26 October 1820: "With regard to your new definition of the objects of Political Economy, I own it appears to me very confined. ... In the same manner when you reject the consideration of demand and supply in the price of commodities and refer only to the means of supply, you appear to me to look only at half of your subject" (*ibid.*, 286). To this Ricardo responded on 24 November: "I do not dispute ... the influence of demand on the price of corn and on the price of all other things [the reference is obviously to "market prices"], but supply follows close at its heels, and soon takes the power of regulating prices [the reference is obviously to "natural price"] in his own hands, and in regulating it he is determined by cost of production. I acknowledge the intervals on which you so exclusively dwell, but still they are only intervals" (*ibid.*, 302).

confined to a single industry only; or whether it is due to the introduction of an entirely new kind of commodity; or whether it is due to the exhaustion of some natural resources; etc. Increasing returns to scale that turn out to be external to each industry or group of industries, as in Smith's discussion of the division of labour, for example, are a case highlighting Ricardo's "no law respecting quantities" dictum: how could one ascertain *a priori* the evolution of quantities and prices? There are simply no demand functions that could be known by the theorist. This is admitted by Blaug, who even contends "that Ricardo had no theory of how the level of output is determined" (p. 223). Then follows the adjunct: "But then neither did any other premodern economist" (*ibid.*). This must not be read as implying that "modern economists" are possessed of a theory of outputs in conditions of an ever deeper division of labor and the product and process innovations that come with it: neoclassical demand theory presupposes given and unchanging "preferences" in deriving demand functions; however, preferences will certainly not remain unaffected by these changes. Even "modern" economics has little to say about *how* this will be the case.

Things are different as regards *proportions*, Ricardo maintained.<sup>7</sup> Here "a tolerably correct law" can be laid down and has in fact been laid down by him. An important aspect of this concerns the fact that, given the system of production (or technique) in use, once the real wage is known, the rate of profits is determined, with a rise in the former involving a fall in the latter and *vice versa* (see, in particular, Ricardo, *Works*, vol. II: 61-2). This analytical discovery by Ricardo has been acclaimed by virtually all his interpreters; Blaug ([1962] 1997, 96) spoke of Ricardo's "fundamental theorem of distribution". Interestingly, in the context of discussing this "theorem" Blaug showed a pronounced concern with the problem of the internal consistency of Ricardo's argument and explicitly had recourse to a model (by Luigi Pasinetti) in order "to spell out Ricardo's meaning in mathematical terms" (*ibid.*, 97). Incidentally, in that model the rate of profits and the price of corn in terms of gold at any given moment of time are precisely determined on the basis of (a simplified version of) the set of data (a)-(d) discussed in the next section. Hence, there there might appear to be a contradiction between Blaug (1999) and Blaug ([1962] 1997). In Section 7 we shall see that

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<sup>7</sup> What is meant by "proportions" in the present context is clarified in a famous passage in the preface of the *Principles*: "But in different stages of society, the proportions of the whole produce of the earth which will be allotted to each of these classes, under the names of rent, profit, and wages, will be essentially different. ... To determine the laws which regulate this distribution, is the principal problem in Political Economy" (*Works*, vol. I: 5).

any such contradiction is more apparent than real, however, as is Blaug's criticism of our contributions.<sup>8</sup>

## 5. The "classical" approach to the theory of value and distribution

The reader will find not one passage in our writings to support Blaug's contention that we interpret the classical authors as holding that what in their approach to the problem of value and distribution are considered independent variables (see "data" (a)-(d) below) are "determined outside their theoretical system" (p. 228, with reference to the wage rate). On the contrary, he will encounter passages maintaining the opposite:

the concern of the classical economists from Adam Smith to David Ricardo was the laws governing the emerging capitalist economy, characterized by wage labour, an increasingly sophisticated division of labour, the co-ordination of economic activity via a system of interdependent markets in which transactions are mediated through money, and rapid technical, organizational and institutional change. In

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<sup>8</sup> A misunderstanding of Blaug's needs to be cleared up. He makes a mockery of regarding as "a sacrosanct first step" in rigorous analysis "the counting of equations and unknowns to check how many variables we need to take as data". He adds: "Marshall knew better, he kept his general equilibrium theory in an appendix and employed the *ceteris paribus* method of partial equilibrium to practice substantive economics" (p. 230). Marshall, of course, did not know better. When he resorted to his method of partial equilibrium of single markets, he reduced the number of unknowns to two: the quantity and the price of the good under consideration. In order to determine these unknowns he needed two independent equations which he thought he could provide by constructing a "demand function" and a "supply function". The prices and quantities of all other goods he took as given and constant, independently of what happened on the single market under consideration. Therefore, as regards the requirements to be satisfied in order to get a determinate solution there is no difference at all between Marshall's theory and some less partial equilibrium theories. However, as Sraffa (1925, 1926) showed, Marshall's partial equilibrium method cannot be applied other than in exceedingly special cases (see also the next section). What Blaug calls "substantive economics" may simply be wrong.

short, they were concerned with an economic system in motion. (Kurz and Salvadori, 1998b, 3)<sup>9</sup>

We added that "the attention focused on the factors affecting the pace at which capital accumulates and the economy expands and how the growing social product is shared out between the different classes of society: workers, capitalists and landowners" (ibid.).

This concern of the classical economists with an economic system incessantly in travail has never been denied by us or any of the "Sraffian" authors, let alone Sraffa himself, to whom Blaug refers in his paper. We are also not aware that any of the scholars mentioned, including us, has ever advocated the view that in the classical authors the problem of the dynamism of the modern economy, its growth and structural change, is extraneous to the economic theory they elaborated. Hence the problem is not *whether* these "dynamic" issues were a part and parcel of classical economic theory – *of course they were* – but rather *how* the classical economists dealt with them. *This* is the crucial question to which we have to turn.

The ingenious device of the classical authors to see through the highly complex system in motion consisted in distinguishing between the "market" or actual values of the relevant variables, in particular the prices of commodities and the rates of remuneration of primary inputs (labour and land), on the one hand, and "natural" or normal values on the other. The former were taken to reflect all kinds of influences, many of an accidental or temporary nature, whereas the latter were conceived of as expressing the persistent, non-accidental and non-temporary factors governing the economic system. The "gravitation" of market values to their natural levels was seen as the result of the self-seeking behaviour of agents and especially of the profit-seeking actions of producers. In conditions of *free competition*, that is, the absence of significant and lasting barriers to entry and exit from all markets, the case with

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<sup>9</sup> See also Kurz and Salvadori (1999a), where we deal, *inter alia*, with the theories of accumulation and growth of Smith, Ricardo, Torrens and Marx. There it is stressed *vis-à-vis* the so-called "new" growth theory that classical growth theory in general and Smith's in particular are theories of *endogenous* growth. In Smith's discussion of the division of labour the evolution of technology and the growth of labour productivity are insolubly intertwined with the accumulation of capital. Therefore it came as a surprise to us that Blaug could write of his reason to deplore an "utter indifference of Sraffian interpreters to the opening three chapters of the *Wealth of Nations* on the division of labor, a subject they never discuss or even mention" (p. 220). Anyone with access to the books written or edited by us and quoted by Blaug may confirm that this is simply not true.

which the classical authors were primarily concerned, profit seeking necessarily involves cost minimization.<sup>10</sup> Hence their attention focused on what may be called *cost-minimizing systems of production*.

The method of analysis adopted by the classical economists is known as the method of *long-period positions* of the economy. Any such position is the situation towards which the system is taken to gravitate, given the fundamental forces at work in the particular historical situation under consideration. In conditions of free competition the resulting long-period position is characterized by a uniform rate of profits (subject perhaps to persistent inter-industry differentials), uniform rates of remuneration for each particular kind of primary input, and prices that are assumed not to change between the beginning of the uniform period of production (usually a year) and its end. While the classical economists put forward an argument in support of the supposed gravitation of market values around their natural levels, their argument cannot, of course, replace a proper dynamic theory, not least because there are particular difficulties of which the earlier authors were not aware. Nevertheless the long-period method is still recognized nowadays as an important tool of analysis.<sup>11</sup> Analyzing economic change and development in these terms then involves a short cut: the adjustment process to any such position is for simplicity taken for granted.

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<sup>10</sup> Blaug contends that "Kurz and Salvadori (1998b, 16, 53) refer to 'free or perfect competition' as if it were the same thing" (p. 230). The reader will search in vain for evidence in support of this contention on the pages given. We are aware that the two concepts are radically different, in particular that the classical concept does not define the market form and thus competition in terms of the number of agents operating on each side of the market. See again, for example, the subject index of our 1995 book and especially Chapter 1 of it on "Free competition and long-period positions". Blaug's claim that "We read ... Kurz, Salvadori ... in vain looking for so much as a reference to the classical conception of competition" (p. 230) is contradicted by the facts.

<sup>11</sup> As Edwin Burmeister pointed out recently: "It is natural to try to answer the easiest questions first, and it is much easier to study economies in a 'long-period equilibrium' than ones in which the rate of profit is not uniform and is changing over time. Very little is known about the properties of such more realistic economies ... and even the little that is known usually is only about special and quite unrealistic cases (such as the one-good case). Almost nothing is known about the dynamic behavior of the more complex models" (Burmeister 1996, 1345-6).

In his paper Blaug stresses repeatedly that classical economics is characterized by "a particular theory of value and distribution" (p. 233), which is different from the neoclassical demand and supply theory. In our brief summary of what we consider to be the salient features of this theory we shall pay special attention to the contribution of Ricardo.

According to Ricardo the study of the laws governing the distribution of income involved (i) isolating the factors determining that distribution *in a given place and time*; and (ii) investigating the causes of changes in these factors *over time*. Ricardo, we take it, isolated the following factors:

- (a) The set of technical alternatives from which cost-minimizing producers can choose.
- (b) The size and composition of the social product, reflecting the needs and wants of the members of the different classes of society and the requirements of reproduction and capital accumulation.
- (c) The ruling real wage rate(s).
- (d) The quantities of different qualities of land available and the known stocks of depletable resources, such as mineral deposits.

Nobody familiar with Ricardo's writings can deny that to him the actual state of technical knowledge in a given situation constituted a main factor determining the levels of the rate of profits and the rent rates. For instance, when discussing the tendency of the rate of profits to fall, Ricardo started from the assumption of a given technical knowledge and then added that this tendency "is happily checked at repeated intervals by the improvements in machinery ... as well as by discoveries in the science of agriculture" (*Works*, vol. I: 120). Nobody familiar with Ricardo's writings can deny that to him essentially the same applied with regard to the levels of total output, because with diminishing returns in agriculture and mining it matters whether little or much corn is to be produced and little or much ore to be extracted, given the information summarized in (d). As Ricardo stressed: "The exchangeable value of *all* commodities, whether they be manufactured, or the produce of the mines, or the produce of land, is always regulated ... by the most unfavorable circumstances, the most unfavorable under which *the quantity of produce required*, renders it necessary to carry on the production" (*Works*, vol. I: 73; emphases added). Nobody familiar with Ricardo's writings will deny that in his view the rate of profits depends on the level of the real wage rate(s) (see, for example, the evidence provided in Kurz and Salvadori 1995, 472-3; see also Section 6 below). And, most important, unlike his contemporary Malthus and the later marginalist authors, Ricardo did *not*

conceive of the real wage rate (and thus the rate of profits) as determined in terms of demand and supply (see above, Section 4).<sup>12</sup>

Ricardo singled out these factors as the dominant ones determining the rate of profits, the rates of rent and "natural" prices *in a given place and time*. However, at the same time he saw the above independent *variables* as containing the key to the problem of the long-run development of income distribution, that is, which long-run course the rate and share of profits would take, and what would happen to the rates of rent on the different qualities of renewable and depletable natural resources, the overall share of rents and relative prices. In his analysis of capital accumulation and of different forms of technical change Ricardo emphasized the interaction of the independent variables among themselves and of them and the dependent variables. As regards the first kind of interactions, think, for example, of capital accumulation which would entail rising levels of output of many commodities and falling levels of some other commodities due to the gradual exhaustion of certain natural resources, that is, an endogenous change in at least some of the independent variables summarized under (b) and (d). As Ricardo stressed, a swift accumulation of capital might also tend to raise the real wage rate and thus affect (c). A change in wages might then have an impact on the chosen methods of production and the composition of output. Recall, for example, the chapter "On Machinery", added to the third edition of the *Principles*, in which Ricardo stressed: "Machinery and labour are in constant competition, and the former can frequently not be employed until labour [i.e. the wage] rises" (*Works*, vol. I: 395). As regards the second kind of interactions, Ricardo focused attention on the prime mover of economic growth and structural change: technological progress. Blaug (p. 219-20) rightly refers to Ricardo's discussion of different forms of agricultural improvements and of machinery (*Works*, vol. I: ch. II, 79-84, and ch. XXXI).<sup>13</sup>

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<sup>12</sup> It is interesting to note that some early marginalist authors apparently had less difficulties than some present-day commentators to see that the classical economists in fact started from the set of data (a)-(d) when determining the rate of profits and relative prices. See, for example, William Stanley Jevons ([1871] 1965, 268-9), Léon Walras ([1874] 1954, part VII, lessons 38-40) and Knut Wicksell ([1893] 1954, 34-40), who provide some indirect evidence in support of the interpretation given here. In particular, these authors were clear that in determining the rate of profits Ricardo took the quantities of output and the real wage rate as given, and criticized him for this, because according to their fundamentally different theory quantities and prices (including the distributive variables) had to be determined simultaneously.

<sup>13</sup> For a detailed discussion of Ricardo's changing views on machinery, see Jeck and Kurz (1983); for Ricardo's views on technological progress and diminishing returns in

Over time the size and composition of output can be expected to change, reflecting a multitude of influences interacting in a complex way. The availability of new methods of production which make possible the reduction of production costs and prices of known commodities and the introduction of entirely new commodities, or of better qualities of known commodities, would interact with the needs and wants of the different classes of society and thus give rise to new patterns of consumption. Hence, what in the determination of the rate of profits and the rates of rent in a given place and time was taken as given under (b) is bound to change over time, reflecting learning processes on the part of producers and consumers and involving changes in income distribution and relative prices. As regards the real wage rate of common labor Ricardo kept stressing: "It is not to be understood that the natural price of labour, estimated even in food and necessaries, is absolutely fixed and constant. It varies at different times in the same country, and very materially differs in different countries. It essentially depends on the habits and customs of the people. ... Many of the conveniences now enjoyed in an English cottage, would have been thought luxuries at an earlier period of our history" (*Works*, vol. I: 96-7).

Before we enter into a discussion of Blaug's criticisms, it deserves to be stressed that Ricardo's intuition was correct: on the basis of the above data (a)-(d) one can in fact determine in a coherent way the unknowns or independent variables in a given place and time: the long-period levels of the rate of profits, the rents of land and relative prices. No other information or data are needed. This is an important fact in itself. In addition it is to be emphasized that *any* coherent long-period theory of value and distribution must start from a set of data which either implies the set of data of the classical approach, (a)-(d) above, or is equivalent to it. As we shall see in Section 7, Blaug's alternative conceptualization is no exception to this rule.

## **6. Independent variables are still *variables***

Economic theory invariably proceeds by cutting slits into the "seamless absolute whole" (Georgescu-Roegen) of socio-economic phenomena. This involves, among other things, adopting some bold simplifications. This does not mean that anything goes. It only means that there must be a judicious selection of aspects to be dealt with, and how to deal with them, setting aside in a first step of the analysis some other aspects. This selection requires some

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agriculture and their respective impacts on the long-term tendency of the rate of profits, see Kurz (1998); for a discussion of Ricardo's views on agricultural improvements, see Gehrke and Kurz (2000).

intimate knowledge of the corresponding phenomenal domain, and it seems that our high esteem for the achievements of the classical economists concerns no less this knowledge of the subject matter than their analytical skills. The classical approach to the theory of value and distribution in terms of the set of independent variables (a)-(d) exemplifies this. Clearly, none of the classical authors would have denied that outputs, techniques, the distribution of the product and relative prices are interdependent and that each of these sets of magnitudes was bound to change over time. However, in determining the rate of profits, the rents of land and relative prices *in a given economy at a given time* Ricardo and the other classical economists started from *given data* (a)-(d), reflecting in particular the achieved state of the accumulation of capital and technical knowledge, the scarcity of the available natural resources and, last but not least, the relative strength of the parties, "whose interests are by no means the same", in the "dispute" over the distribution of income, as Smith (WN, I.viii.11) kept stressing.

This should make it abundantly clear that it never occurred to us to interpret the classical economists as assuming that the independent variables or "data" (a)-(d) above are data characterizing once and forever the economy under consideration, that is, *historical constants*. Nor are we aware that Sraffa or any of the scholars working in his tradition, including us, has ever written anything that could possibly be misunderstood in this way. Yet, surprisingly enough, this is precisely the interpretation Blaug contends we are advocating. Nothing could be farther from the truth.<sup>14</sup>

It hardly needs to be emphasized that independent variables are still *variables*. The magnitudes under consideration are only treated as known or given in one part of classical theory: the determination of the shares of income other than wages, and relative prices, in given conditions of the economy; in other parts of the theory they are themselves treated as dependent variables or unknowns. In other words, *variables (a)-(d), while magnitudes external to the classical approach to the theory of value and distribution in particular, are magnitudes internal to the classical theory as a whole.*

To see better what we mean it is useful to have a closer look at Blaug's criticism. Following his discussion, we shall focus attention on the independent variables (a)-(c), because (d) seems to be uncontroversial.

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<sup>14</sup> If Blaug were to apply the same kind of reasoning he applies to our interpretation of the classical approach to the theory of value and distribution also to Marshall's partial equilibrium method, he would have to interpret Marshall's *ceteris paribus* clause as involving the assertion that what is taken as given will never change.

### *Technical alternatives*

As regards parameter (a), Blaug maintains that what is meant are the given "fixed coefficients" posited for the production of different commodities "that are written down on the first page of Sraffa's book" (p. 219). To this he adds: "It is perfectly true that the classical economists rarely addressed the problem of the choice of technique, virtually implying that it was usually impossible to choose among a number of technical alternatives" (ibid.). Hence, with regard to an economy at a given time Blaug interprets the classical authors more narrowly than we do, and indeed too narrowly, because they did not turn a blind eye to the problem of the choice of technique: for example, at the very heart of the theory of rent there is the problem of which qualities of land to cultivate from a set of given qualities (extensive rent) or of which methods of production to employ on a given quality of land (intensive rent), that is, a problem of the choice of technique. (See also again the choice of technique problem discussed by Ricardo in the context of the machinery question referred to in Section 5.)

Interestingly, Blaug explicitly admits what we have stated in terms of the independent variable (a). His references to Ricardo's discussion of "agricultural improvements" or of machinery (p. 219) are materially of no import, because we have never denied that *over time* technical knowledge will change. Yet, Blaug's references provide a welcome opportunity to illustrate that our interpretation is faithful to Ricardo's approach to the theory of value and distribution. Take, for example, the latter's discussion of what may be called "land saving improvements" (similarly his discussion of "capital or labor saving improvements") (see also Gehrke and Kurz, 2000). As Ricardo's numerical illustration makes clear, he took as given the amounts of the different qualities of land available in the economy, the real wage rate, the methods of production employed on the lands actually cultivated prior to the improvement, and the methods of production available after the improvement. He stated: "These improvements absolutely enable us to obtain *the same produce* from a smaller quantity of labour" (*Works*, vol. I: 80; emphasis added). Hence he compared two situations defined in terms of the *same* information concerning data (b)-(d), but different information concerning datum (a). (Ricardo left no doubt that this is only a first step in an analysis of the impact of technical change on income distribution and relative prices.) For a similar procedure, see Ricardo's chapter on machinery.

In his discussion relating to "datum" (a) Blaug expresses a view that is potentially misleading or difficult to sustain. We were surprised that in the context of a discussion of Marx's analysis of the "labour process" he could write: "Far from technology being given to capitalists, the choice of technique is at the very heart of the contested terrain between workers and capitalists" (p. 222). Here it suffices to point out that the problem of the choice of technique forms a

centrepiece of Sraffa's analysis (1960, part III) and the literature inspired by it (see, for example, Kurz and Salvadori, 1995).<sup>15</sup> It should also be recalled that Marx's discussion of the "labour process" assumes special weight in the context of an analysis of what he called the production and appropriation of "relative surplus value". He means here the additional surplus value obtained not in terms of a lengthening of the working day, other things being (roughly) equal, but in terms of an increase in labour productivity due to organisational and technological changes. Here specific forms of such *changes* are considered instrumental to the capitalists' interest to alter the balance of power between themselves and the workers in their favor. However, both the classical economists and Marx treated the intensity of work in normal conditions of a particular economy in a particular time as given. Marx, for example, stressed: "The labour-time socially necessary is that required to produce an article under the *normal conditions of production*, and with the *average degree of skill and intensity prevalent at the time*" (Marx 1954, 47; emphasis added).<sup>16</sup>

### *Outputs*

The interpretation of the approach of the classical economists to the theory of value and distribution as starting from given levels of output Blaug tries to counter in terms of the remark: "Come, come: the volume of output, alongside the size of the labor force, is constantly rising in Ricardo" (p. 224). True, output levels (at least of many products) in the year 1817 may be higher than output levels in the year 1776, but in order to ascertain the rate of profits, the rent

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<sup>15</sup> Later in his paper Blaug comments on the fact that Sraffa's equations of production exhibit one degree of freedom which is then removed by taking one of the distributive variables as given: "the problem of not-enough-equations would not even arise if Sraffa had allowed production coefficients to vary" (p. 229). To be clear, to have a number of methods of production to choose from that is larger than the number of commodities the prices of which are to be ascertained is one thing, to determine which of these methods will actually be chosen by cost-minimizing producers is a totally different thing. The latter decision requires information about the level of wages (or, alternatively, the rate of profits). This will be immediately clear to people familiar with the von Neumann model, in which real wages (paid *ante factum*) are taken as given.

<sup>16</sup> While Blaug is correct in stressing the "incompleteness" of the labor contract, especially as regards the intensity of work to be performed, this aspect concerns first and foremost the relative strengths of the different parties involved, workers and capitalists, and would be best treated in the context of a discussion of "datum" (c).

rates and relative prices in 1776 (or 1817), what matters are output levels (and, of course, techniques and the real wage rate) in 1776 (or 1817).

The same misunderstanding reappears in several forms. For example, Blaug tries to ridicule the assumption of given quantities in order to determine the endogenous variables just mentioned in a given place and time with reference to Marx's "law of the falling rate of profit". Marx, he contends, would have been "surprised ... to learn that his extended investigation ... was not proper classical economics because it violated the fundamental postulate of 'given quantities'" (p. 223). Marx was perfectly clear about the fact that any fall (or rise) in the rate of profits between two years, say 1776 and 1817, was due to changes in the fundamental factors at work reflected in particular constellations of data (a)-(d) (see Marx 1959, part III). Blaug also maintains: "There is ample evidence in Ricardo's *Principles* that he had in mind a moving equilibrium" (p. 224; see also p. 226). One can only wonder in which terms Ricardo, according to Blaug, characterized that "equilibrium" at any given moment of time and whether that characterization differs from the one proposed in Section 5. (An answer to this question is given in Section 7.)

At the heart of Blaug's respective criticism appears to be a confusion between attributing a particular analytical method to the classical economists and attributing particular propositions about reality to them. Blaug contends: "Sraffa tells us that there are 'no changes in output' in 'the old classical economists'" (p. 224). This is, of course, not true: Sraffa never maintained that there are no changes in output contemplated by the classical authors. He rather specified that in his book "the investigation is concerned exclusively with *such properties of an economic system* as do not depend on changes in the scale of production or in the proportion of 'factors'" (1960, v; emphasis added). To focus attention on such properties of an economic system does not mean, of course, to maintain that there are no such changes. It only means that these changes are set aside in the respective investigation. What is at stake is a *method* designed to analyse an aspect of the economic system under consideration and not a factual proposition that the system is stationary. This becomes clear in the statement that follows the one just quoted: "This *standpoint*, which is that of the old classical economists from Adam Smith to Ricardo, has been submerged and forgotten since the advent of the 'marginal' *method*" (ibid; emphases added). The latter focuses attention on (marginal) changes in the scale of production and in the proportions of factors. It attempts to determine relative prices and the distributive variables in terms of incremental quantitative changes. This is in stark contrast with the classical method in the theory of value and distribution, which Sraffa was keen to revive: his aim was the determination of the competitive rate of profits and, using the "classical terms" (ibid., p. 8), the corresponding "necessary prices" or "natural prices" or "prices of production". In other words, Sraffa's book, following the classical economists, studies the problem of value and distribution in given conditions by taking the levels of outputs

as known magnitudes.<sup>17</sup> The book is not about accumulation, growth, technical change etc. However, as we know from Sraffa's unpublished manuscripts, it was meant to prepare the ground for an analysis of these important features of the modern economy very much in the same way as Ricardo's approach to the theory of value and distribution in chapters I-IV of the *Principles* was meant to prepare the ground for his discussion of these issues.

### *Wages*

We now come to the independent variable (c). It should be stressed immediately that what the classical economists took as given with regard to a particular economy at a particular time in order to determine the other distributive variables and relative prices was the wage rate of "common labour" and the scale of wage differentials. The latter is considered to be fairly stable over time (see, e.g., Smith, WN, I.x.c.63, and Ricardo, *Works*, vol. I: 20-1). Smith mentioned the following factors accounting for wage differentials: (i) differences in the costs of production of different skills; (ii) the scarcity of particular talents; (iii) differences in the degree to which the laborers' capacity to work can be utilized in different employments; (iv) differences in the trust that must be reposed in the workers; and (v) different risks involved in becoming qualified for the employment to which one is educated. Since we have tried to reformulate Smith's ideas within the framework of modern classical theory of value and distribution (see Kurz and Salvadori, 1995, chapter 11), we may immediately turn to the given wage of common labour.

It is difficult to see wherein precisely Blaug disagrees with us. We share Blaug's emphasis on the classical authors' "attention to the institutional setting" of the problem under consideration (p. 228). There appears to be also no material difference between our view and his that the classical authors "regarded the minimum-level-of-existence wage rate ... as something that

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<sup>17</sup> To start from given levels of gross outputs, designed to reflect the degree of the division of labour reached by a particular economy at a given stage of its development, is therefore not a "myth" invented by Sraffa (p. 222), but rather a premise congenial to Smith's important concept (see Kurz and Salvadori, 1998, vol. I, pp. 325-9). Interestingly, Blaug relates Sraffa's assumption of given quantities explicitly to Smith's analysis. He comments on the former: "As a description of what the *Wealth of Nations* is all about, even the chapters in book I on the theory of value and distribution, this is simply grotesque" (p. 223). It hardly needs to be stressed that that assumption was not designed by Sraffa for the purpose invoked by Blaug. However, we wonder whether in the light of what has been said above the assumption still looks "simply grotesque" to him.

was determined by slowly changing *historical traditions* [conditions?] and which, therefore, *could be taken as given* in analyzing a practical question, like a tax on wage goods" (p. 227; emphases added). Analyzing a practical question presupposed that the theoretical framework was already in place. And indeed it was and allowed the classical authors to ascertain the nonwage incomes (profits and rents) and relative prices in a given place and time, taking the wage rate as a known magnitude. The impact of a tax on wage goods on profits, rents and relative prices, for example, given the real wage rate, could then be discussed at will. As Blaug indicated in the passage just quoted, in the classical authors even the minimum real wage rate was not an absolutely fixed magnitude. And the market wage rate could rise well above that minimum. Thus, Smith and Ricardo kept stressing that in conditions of rapid accumulation capitalists would start bidding up the real wage rate, which, in conditions of unchanged technical conditions of production, would depress profitability (see, for example, Ricardo, *Works*, vol. II: 252 and 264-5). Hence, to take the actual real wage rate as given in a particular place and time in order to determine the actual rate of profits, rents and relative prices in that place and time does not mean to assume that the wage rate will forever remain at that level (any more than Marshall's use of *ceteris paribus* means that the givens are and will remain constant). These considerations should also suffice to dispel Blaug's following suspicion: "to say that [in the determination of the rate of profits, rents and relative prices] the classical economists treated the 'natural price' of labor as exogenous [means that it is] determined outside their theoretical system" (p. 228). This is a *non sequitur*. To repeat what has already been said in the above: In the classical authors the real wage rate is treated as a known magnitude when it comes to the determination of the other distributive variables and relative prices in a given place and time, but it is of course treated as a magnitude to be determined in their theoretical system as a whole, depending, *inter alia*, on cultural, institutional and historical factors. In order to avoid confusion one ought to distinguish between the different spheres of their analyses.

Blaug's final objection in the present context reads: "Besides (and now we come to the crux of the matter), the idea that the classical economists must have taken the real wage as a datum because the logical consistency of their theory demanded it is a perfect example of a rational reconstruction of past theories: it reads Smith and Ricardo and Marx through Walrasian-tinted glasses" (p. 229). This is a misrepresentation, because the argument is *not*, as Blaug maintains, that the classical economists *ought* to have taken the real wage rate as a datum when determining the rate of profits etc., but that they actually *did* take it as such. It would be interesting to see whether Blaug can provide any evidence that in determining the rate of profits, rents and relative prices in a given place and time Smith, Ricardo or Marx did not start from a given real wage rate. In his paper Blaug provides no such evidence and indeed no such

evidence can possibly be provided, whereas it is easy to provide evidence to the contrary.<sup>18</sup> In one place Blaug indirectly appears to admit this when he writes: "Ricardo's question was: how do relative prices change when income distribution varies and, in particular, when technology causes the rate of profit to decline in real time?" (p. 232). Here we have what may be called the purely hypothetical cases of both the "static" and the "dynamic" problem of value and distribution: How does the rate of profits and do relative prices change when there is a change in the real wage rate, in conditions in which the technical alternatives of production are for simplicity taken as given? And how does the rate of profits and do relative prices change when capital accumulates, the population grows and less and less fertile lands have to be cultivated or lands of a given fertility have to be cultivated more intensively, in conditions in which the real wage rate is for simplicity taken as given? We called the two cases contemplated by Ricardo (see, especially, his *Notes on Malthus, Works*, vol. II) purely hypothetical because Ricardo was, of course, very clear that what is taken as given can be expected also to change because of the interaction of the different variables under consideration.<sup>19</sup> To see that the interpretation of Ricardo and the classical economists we endorse is faithful to their writings, we may draw Blaug's attention again to Ricardo's discussion of "agricultural improvements", where the real wage rate in terms of corn is taken as given (otherwise Ricardo could not have considered the capitals employed on the different kinds of land as known magnitudes), or to Chapter 1 of the *Principles* in which the dependence of the rate of profits and relative prices on the real wage is discussed in detail and what Blaug called Ricardo's "fundamental theorem of distribution" is established.

To conclude this section, to take the real wage rate as given when determining the rate of profits, rents and relative prices is certainly not a consistency requirement imposed by us on the classical authors; the premise under consideration is rather encountered in the classical economists themselves. There is every reason to presume that this is so, because, understandably, *they* were concerned with consistent arguments and despised inconsistent ones.

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<sup>18</sup> See, for example, Marx's discussion in chapter XI of volume III of *Capital* of the "Effects of General Wage Fluctuations on Prices of Production", "all else remaining the same" (Marx 1959, 200).

<sup>19</sup> The two hypothetical cases are also discussed by Marx in vol. III of *Capital*. It is interesting to note that in his attempt to establish a tendency of the rate of profits to fall, Marx explicitly assumed a given and constant real wage rate: "Nothing is more absurd ... than to explain the fall in the rate of profit by a rise in the rate of wages, although this may be the case by way of an exception" (Marx 1959, 240).

According to Blaug one must "aim to make historical reconstructions as descriptively accurate as possible" (p. 232). We agree. He adds: "This is an aim of which Sraffians have totally lost sight" (ibid.). Blaug's claim just cannot be reconciled with the above cited evidence.

## 7. Blaug's alternative conceptualization of the "core"

Blaug concludes his paper by asking: "So, is there a 'core' of classical economics?" (p. 232). His answer is: "Obviously, yes if by *core* we mean a *central strand* by which we recognize a work as belonging to 'classical economics', the strand that unites Smith in 1776, Mill in 1848, and Marx in 1867. It is made up, all commentators agree, of a *particular theory of value and distribution*" (pp. 232-3; emphasis added). This contention is dubious, because what can at most be said is that there is a particular classical *approach* to the theory of value and distribution, whereas the specific variants of that approach put forward by Smith, Ricardo and Marx differ in several respects.<sup>20</sup> Thus, when we talk of the "classical" theory of value and distribution we can only refer to the *essence* of the theories put forward by authors such as Petty, Cantillon, the Physiocrats, Smith, Ricardo and Marx, that is, in the words of Sraffa, "not the theory of any one of them, but an extract of what ... is common to them" (D3/12/4, 12).<sup>21</sup>

So what is common to these authors, especially as opposed to the advocates of neoclassical economics? Blaug insists: "First, classical value theory focuses on long-period equilibrium prices characterized by a uniform rate of profit on capital, uniform rates of pay for every different type of labor, and uniform rents per acre for every different type of land; in short, what Smith called 'natural prices' in contrast to 'market prices,' subject to the vagaries of demand and supply" (p. 233). Blaug's specification concerns what was called above the *long-period method*. This is indeed a first characteristic feature of the classical approach to the theory of value and distribution. Yet since this method was essentially adopted also by all major marginalist economists until the late 1920s, including Jevons, Walras, Böhm-Bawerk,

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<sup>20</sup> It seems that Blaug was misled by the vague term "central strand" he uses. Apparently he equivocated between a *substantial theory* and an *analytical approach* to a particular problem. It is the latter that is relevant in the present context.

<sup>21</sup> The reference is to Sraffa's unpublished papers which are kept at the Wren Library, Trinity College, Cambridge. The reference given follows the catalogue prepared by Jonathan Smith, archivist. We are grateful to Pierangelo Garegnani, literary executor of Sraffa's papers and correspondence, for granting us permission to quote from them.

Marshall, Wicksell and John Bates Clark (see Garegnani, 1976; Kurz and Salvadori, 1995, 427-55), we must turn to the *content* of the different kinds of approaches in order to be able to discriminate between a classical and a marginalist (or neoclassical) approach.

As regards the content of the former, Blaug emphasizes that the "natural prices were determined ... *in the context of a technology of production* characterized in physical terms and expressed for practical purposes in hours of labor" (p. 233; emphasis added). The reader may wonder what is the difference between datum (a), which postulates a given set of technical alternatives from which cost-minimizing producers can choose, and a given "context of a technology of production". The latter involves the former (and perhaps something more; see below). And if the "technology of production" were not taken as *given*, how could natural prices or hours of labor expended in the production of the different commodities ever be determined?

Is the long-period method, together with Blaug's version of datum (a), sufficient to distinguish the classical approach from the neoclassical one? The answer is obviously no. The two elements are also present in all versions of traditional marginalist theory. See, for example, the conventional representation of the available technical alternatives in terms of given production functions for different products in Wicksell or Clark or the specification of given methods of production in terms of "coefficients de fabrication" in Walras. Hence, more is needed in order to identify the specificity of the classical approach to the theory of value and distribution.

Blaug is aware of this and adds that "the 'core' of classical economics always involved some version of the labor theory of value" (p. 233).<sup>22</sup> Before we continue with the main argument

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<sup>22</sup> The reader will recall Blaug's earlier statement that the classical authors "expressed [the technical conditions of production] *for practical purposes* in hours of labor" (p. 233; emphasis added). From this point of view the labor theory of value can hardly be said to have been an indispensable element of classical analysis. It was simply a useful tool at a certain stage of the development of the analysis that could be dispensed with as soon as the role performed by it could be assumed by a more correct theory. As Wittgenstein put it, a particular theory may be compared to a ladder that is useful to reach a higher standpoint. However, once this standpoint is reached and a fuller view of the landscape is possible, the ladder may turn out to be an instrument that is inferior to some other device to reach that higher standpoint and beyond and will therefore be dispensed with. The same can be said with regard to the labor theory of value: it was an instrument that provided useful services to the classical economists, but once the problem of the relationship between income distribution and relative prices, given the

two clarifications are needed. First, the quantities of labor embodied in the different commodities cannot generally be determined independently of the *levels of output*. As is well known, in Ricardo the attention concerning the relevant amount of labor needed in the production of one quarter of corn focuses on the conditions of production on the marginal land, which, however, cannot be ascertained independently of the total amount of corn to be produced and the quantities of the different qualities of land available in an economy. Blaug is, of course, aware of this (see p. 224).<sup>23</sup> Hence, in order to determine labor values some information of the kind summarized in data (b) and (d) is needed. Since Blaug does not separately specify these data, we have only one option: in order not to level an unjust criticism against his interpretation of the classical authors, we must interpret his above formula "in the context of a technology of production" as a catch-all phrase involving both independent variables or data (a), (b) and (d). Secondly, we have already mentioned Blaug's attempt to ridicule the casting of problems in the theory of value and distribution in terms of systems of simultaneous equations (and inequalities) (pp. 229 and 233). However, when production is recognized to be circular (rather than unidirectional), as, for example, in Quesnay's *Tableau Economique*, Torrens's and Marx's schemes of reproduction, or Ricardo's discussion of the productive interrelationship between agriculture and manufactures – how can labor values be determined other than in such terms? The fact that for illustrative purposes Ricardo and other authors frequently used simple numerical examples with unidirectional production (of finite duration) must not be mistaken to imply that they were uninterested in giving, as Blaug puts it, "due attention to the interdependencies between markets" (p. 229).

We must now come back and ask: Can the classical theory of value be discriminated from other theories of value, especially the traditional marginalist one, in terms of the presence of "some version of the labor theory of value"? The answer is obviously no. First, none of the authors mentioned by Blaug (Smith, Mill, Marx) was of the opinion that (other than in singularly special cases) relative prices are strictly proportional to the relative quantities of labor embodied in the different commodities, which is the usual meaning of the labor theory of

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system of production in use, had been fully solved, the labor theory of value had not only become dispensable, but actually had to be dispensed with because it did not provide a fully correct picture of that relationship. The fact that they were not possessed of a correct theory of value and distribution might contribute to explaining why, according to Blaug, "both Ricardo and Marx were so obsessed with the labor theory of value" (p. 217).

<sup>23</sup> Blaug even admits that Ricardo assumed a given level of corn output that is independent of the price of corn or rather: "the demand for corn was perfectly inelastic, ... and that is precisely what Ricardo seems to have assumed" (ibid.).

value. Smith restricted the applicability of the quantity of labor rule of exchangeable values explicitly to the "early and rude state of society"; Mill reiterated Ricardo's view that relative prices are not exclusively regulated by the technical conditions of production reflected in hours of labor needed directly and indirectly in the production of the various commodities; and Marx indicated already in volume I of *Capital*, and expounded in some detail in volume III, that (relative) prices are bound to systematically deviate from (relative) labor values, *viz.* the (in)famous "transformation problem". Secondly, many of the early marginalist authors, despite their completely different approach to the theory of value and distribution, can also be said to have held "some version of the labor theory of value". Ironically, some of these authors were stern advocates of the view that with regard to reproducible goods the then novel (marginal) utility theory of value amounted to materially the same thing as the pure labor theory of value. See, for example, William Stanley Jevons ([1871] 1965, 186-9), Philip H. Wicksteed ([1884] 1999, 717-18), Friedrich von Wieser (1884, 159-60) and Eugen von Böhm-Bawerk (1892, 329-30). John Bates Clark insisted: "In the subjective valuations of society, as an organic whole, the product of two hours' labor is always worth just twice as much as is the product of one. Mere labor time is an accurate gauge of the values of different complements of goods" (Clark 1899, 390).

Blaug's above criterion therefore cannot perform the role of a litmus test of what is to be considered as genuinely "classical" in the theory of value and distribution. Before we continue, we ask a question Blaug could (and indeed should) have raised, but didn't. That question lingers at the back of his rather vague notion of "some version of the labor theory of value": Why did none of the classical authors (or J. S. Mill) advocate the pure and simple version of that theory which claims that relative prices are strictly proportional to relative labor quantities (or labor values)? Because at least since Ricardo they knew very well that this would have been strictly correct only in the singularly special case of uniform proportions of direct labor to means of production (or indirect labor) and uniform degrees of durability of fixed capital across all lines of production, or uniform "organic compositions of capital," to use Marx's concept. Blaug is aware of this, and he is equally aware of the fact that in the only interesting, because realistic, case of non-uniform proportions, prices depend not only on the technical conditions of production but also on income distribution. This is made abundantly clear, for example, in sections IV and V of chapter I of Ricardo's *Principles* (see *Works*, vol. I: 30-43), and in part II of volume III of Marx's *Capital* (1959). Clearly, data (a), (b) and (d) (which appear to be equivalent to Blaug's assumption of a given "technology of production") generally do not suffice to determine relative prices and, as the classical authors knew very well, they never suffice to determine the competitive rate of profits. In order to render the theory determinate, something like datum (c) was needed.

We now turn to the way in which Blaug completes his purportedly alternative conceptualization of the characteristic features of the classical theory of value and distribution. He contends that the "core" of classical economics involved also "a more or less detailed analysis of the forces making for capital accumulation and, of course, a thin or thick version of the Malthusian theory of population" (p. 233). The interplay between capital accumulation and the Malthusian population mechanism is discussed in Chapter V of Ricardo's *Principles*. That interplay is invoked by Ricardo in order to argue that the market wage rate tends to move towards the natural wage rate. This involves a particular view of how the real wage rate is determined. Hence, we could say that in his rational reconstruction Blaug's reference to the Malthusian theory of population provides the missing piece in terms of a very special form of datum (c) that renders the theory determinate. Notwithstanding his frontal assault on the set of independent variables (a)-(d) as a characteristic feature of the classical approach to the theory of value and distribution in the main part of his paper, Blaug in the end endorses a special version of precisely that set.

The Malthusian theory of population, we suggest, does not form a constituent part of the classical approach to the problems of value and distribution. Blaug, who, as we have seen, counts Marx – a fierce critic of Malthus – among the classicists, will have difficulties to discern traces of that theory, whether thick or thin, in the latter's analysis. Smith held essentially a bargaining theory of wages, focusing attention on the relative strengths of the parties, "workmen" and "masters", in the conflict over the distribution of the product, with the emphasis placed on cultural, legal and political factors (cf. WN, I.viii). In the case of Ricardo things are particularly complex. While there are references to the Malthusian theory of population, Ricardo's works abound with observations questioning its validity. We have already seen that according to Ricardo "the natural price of labour, estimated even in food and necessaries, ... essentially depends on the habits and customs of the people" (*Works*, vol. I: 96-7). In an "improving society" with the "market" wage rate exceeding the natural rate it is possible that "custom renders absolute necessaries" what in the past had been considered comforts or luxuries, that is, the natural wage is driven upward by persistently high levels of the actual wage rate. Interestingly, in Ricardo's view "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*" (*Works*, vol. VIII: 169; emphasis added).<sup>24</sup> And in his *Notes on Malthus* he insisted that "population and necessaries are not necessarily linked together so intimately": "better education and improved habits" may break the population mechanism (*Works*, vol. II: 169). Hence, in Ricardo we encounter propositions that are decidedly anti-Malthusian.

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<sup>24</sup> We owe this quotation to Pierangelo Garegnani. On a possible reason for Ricardo's inconsistency, see Stirati (1994, 147-57).

Blaug's claim that classical economics "always involved ... a thin or thick version of the Malthusian theory of population" (p. 233) cannot be sustained.

We conclude that Blaug's own reconstruction of the "core" of classical analysis is a variant of the set of data (a)-(d) expounded in Section 5. We have also provided evidence showing that his variant cannot be considered an interpretation that is historically more faithful to what is common to the authors under consideration than the one advocated by Sraffa and economists working in his tradition.

Finally, we should like to stress once again that the classical approach to the theory of value and distribution is alive and thriving. As was stressed above, data (a)-(d) specify its *logical structure* with its *asymmetric* treatment of the distributive variables. An author, or parts of his analysis, may therefore be called "classical" if we encounter this logical structure in the theory of value and distribution put forward by him or her. The approach could only survive because it does not depend on particular historical conceptualizations of some of its elements; more specifically: it does not stand or fall with the validity of the labor theory of value or of the Malthusian theory of population. The approach is entirely independent of these theories. Therefore, the classical approach to the theory of value and distribution should not only be of interest to the historian of economic thought, but also to the modern economic theorist.<sup>25</sup>

## 8. Conclusion

The paper discusses Mark Blaug's recent criticism of the interpretation of the classical economists inspired by Piero Sraffa's work. It is argued that, contrary to Blaug's claim, we have given a faithful interpretation of the classical authors. In particular, it is shown that the latter treated the technical alternatives of production, the levels of output of the different commodities produced, the real wage rate of common labor (and the quantities of the different qualities of land available) as independent variables, or data, when determining the competitive rate of profits, the rates of rent and relative price in a given place and time. However, while these magnitudes are treated as variables that are external, or exogenous, to the classical approach to the theory of value and distribution, they are internal, or endogenous, to the

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<sup>25</sup> In some of the papers reprinted in our book (Kurz and Salvadori, 1998b) we have shown that the logical structure of the classical approach to the theory of value and distribution can also be discerned in more recent contributions such as, for example, the von Neumann model, the non-substitution theorem, and in some of the so-called "new" growth models.

classical theory as a whole. This draws attention to the fact that the classical authors distinguished between different spheres of economic analysis necessitating the employment of different methods. While one sphere is suited to the application of deductive reasoning – this relates to the investigation of the relations between the distributive variables and relative prices, given the system of production – the other sphere requires more inductive lines of reasoning and research – this relates to an investigation of the sources and consequences of economic change, in particular technological progress, economic growth, changing consumption patterns, the exhaustion of natural resources etc.. It was then demonstrated that Blaug's suggested alternative specification of the "core" of classical economics amounts to a very special version of the Sraffian interpretation.

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