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In Favor of Rigor *and* Relevance

A Reply to Mark Blaug*

Heinz D. Kurz and *Neri Salvadori*

ABSTRACT: The paper discusses Mark Blaug's recent criticisms of "Sraffian economics". It is shown that none of the criticisms stand up to close examination. Blaug commits a number of elementary blunders and mistakes the mathematical form of an argument for its content. He variously contradicts himself and puts forward bold contentions that cannot be sustained. The paper concludes with an obvious plea for rigor and relevance.

Mark Blaug has written another paper in which he attacks "Sraffian economics". He characterises his piece as "a new version of an earlier effort (Blaug, 1999), [that] extends and hopefully deepens the argument" (Blaug 2009, 219 n. 2).[1] A careful scrutiny of the paper shows that Blaug reiterates once again his previous criticisms, adds a few new ones, but does not enter into a serious discussion of the replies to his earlier efforts (see Garegnani 2002 and Kurz and Salvadori 2002a; 2003, chapter 2). Answering him in detail would necessitate repeating again our counter-arguments. We spare the readers this and ask them to consult our earlier replies to Blaug. Instead, we provide an overall assessment of Blaug's various attacks on "Sraffians" and draw readers' attention to a significant change in direction in his recent one. In addition we refute a number of contentions and remarks by Blaug.

Our comment is composed as follows. We begin with a characterization of Blaug's respective efforts (Section 1). Next we refute his contention that Sraffa's analysis is a species of Walrasian general equilibrium theory (Section 2). We then show that he is wrong in contending that "Sraffians" have not dealt with a number of problems tackled by the classical authors (Section 3). Finally we scrutinise and reject his contention that "Sraffian economics"

* We are grateful to Tony Aspromourgos, Pierangelo Garegnani, Christian Gehrke, Geoff Harcourt, Gary Mongiovi and Luigi Pasinetti for valuable comments on an earlier draft of this paper. Any remaining misconceptions are, of course, our responsibility.

1 In the following all isolated page numbers refer to Blaug (2009).

is not fruitful economics (Section 4). We conclude with an obvious plea for rigor *and* relevance (Section 5).

1. A general assessment of Blaug's repeated attacks

As regards Blaug's various criticisms of the "Sraffian" view, the following observations are apposite.

1. Blaug has already been given the opportunity in this journal to answer his critics; see Blaug (2002). Apparently, he feels that his rejoinder was not effective. This is hardly surprising because Blaug did not attempt to counter the objections of his critics.

2. Scrutiny of his new effort reveals that the situation has not changed. Once again Blaug merely reiterates his previous criticisms, adds a few new ones, but neglects to answer his critics. He seems to feel that repeating his story often will render it credible.

3. The attentive reader will notice that Blaug has changed the direction of his critique. Originally his main concern was that of a historian of economic thought who had to watch over the faithfulness of the interpretations of earlier authors. He criticised Sraffa and his followers for having "misunderstood" the classical economists, especially David Ricardo (see the title of Blaug 1999). Now the reader is told after several pages of criticising "Sraffians" that the difference between his and their interpretation of the classical economists is only "a question of emphasis" (232). Much ado about a trifle? In our comment on his alternative conceptualization of the characteristic features of the classical theory of value and distribution in Blaug (1999) we concluded that "Blaug in the end endorses a special version" of the Sraffian interpretation (Kurz and Salvadori 2002a, 234; 2003). He now fully confirms our judgement.

4. While his new paper shows Blaug on the retreat in the field of the history of economic thought, he is on the advance in the field of contemporary economics, broadly speaking. He puts forward a new criterion in terms of which he wishes to evaluate the existing research traditions, fields and approaches by asking, which of them arrive at "socially and politically relevant conclusions" (219); whether "Sraffian economics" is relevant "to our understanding of the real world and … to the major concerns of modern economists" (221); and whether it is

relevant “to the policy concerns of modern economists” (224). How to approach such difficult questions?

5. Blaug’s key to an answer is found in the following postulate: “There is a trade-off in economics (and elsewhere) between rigor and relevance: the more we achieve deductive certainty in our arguments, the less likely it is that we will achieve socially and politically relevant conclusions” (219). This trite contradistinction is popular with some economists (and especially with undergraduates). It is typically invoked by those who emphasize the relevance of what they are doing but do not insist on the rigor with which it is done.

6. Unless the contradistinction is taken in a practical, common-sense fashion, it is of no use at all. This is demonstrated by Blaug’s subsequent disquisition (220-22). He arrives at the conclusion that “neither rigor nor relevance seems quantifiable even in ordinal rather than the more demanding cardinal terms” (221) and that a “quantitative incommensurability hangs over the trade-off between rigor and relevance” (222). After having debunked the scaffolding designed to give solidity to his overall construction, he concludes: “Still, *we may agree* that there is a spectrum of qualitative alternatives in that trade-off that ranges from much rigor and little relevance to little rigor and much relevance” (222; emphasis added). Yet what Blaug wants the reader to agree to is not this abstract and vague proposition, but rather a concrete and specific version of it: “The trade-off between rigor and relevance is pervasive throughout economics and it is avoided only in one or two extreme examples. The point of my paper is that Sraffian economics is not one of these” (224). If there are only one or two exceptions to the rule, as Blaug contends, we may assume that he is at odds not only with “Sraffian economics”, but with economics in general, setting aside the exceptions.

7. This becomes clear in his paper. Comparing the space devoted to “Sraffian economics” on the one hand and to other traditions or fields or approaches in economics on the other, by far the larger part of the paper deals with issues that have no direct or clear bearing on “Sraffian economics”. Blaug passes (mostly negative) judgements on, inter alia, Social Choice Theory, General Equilibrium Theory, the Coase Theorem, Public Choice Theory, Evolutionary Economics, Institutional Economics, Game Theory, Mechanism Design Theory, Austrian Economics, Radical Economics and Marshallian Partial Equilibrium Analysis. Much of contemporary economics he considers to be as irrelevant as “Sraffian economics” when it comes to understanding the capitalist system and elaborating policy conclusions. In the light of this, we wonder what prompted Blaug to choose the title of his piece.

8. Given Blaug's seeming disenchantment with much of modern economics, it is perplexing to see that he wishes to assess "Sraffian economics" in terms of "the major concerns of modern economists" (221). Has he not emphasized that these concerns are largely dubious or misleading and at least "irrelevant"? To the extent to which they meet with his approval, because they are "policy concerns", is it enough to observe whether an analysis leads to "socially and politically relevant conclusions"? Should one not also, and indeed first and foremost, be concerned with the quality of such conclusions, whether they manage to realise the sought objectives, what these objectives are, whose interests they further and whose not, etc.? It is ironic to see Blaug assess alternative economic analyses in terms of a criterion that lacks any qualitative dimension, and this in the midst of a deep economic crisis in which, as *The Economist* wrote, the reputation of mainstream economics "has taken a beating" and some of the policy recommendations of modern macroeconomics and of finance theory are seen as a source of the trouble.[2] To be relevant (in Blaug's sense) may simply mean to be wrong and harmful. What matters is not, as Blaug writes, whether an economic analysis generates "socially and politically relevant conclusions", but rather whether these conclusions are broadly right or precisely wrong. On this problem Blaug's paper offers nothing.[3]

9. This brings us to one of Blaug's main convictions which we consider to be fundamentally mistaken. He characterises Walrasian general equilibrium theory (GET) as "undoubtedly rigorous but, alas, totally irrelevant: it has no empirical content and is incapable of answering any practical question that an economist might want to pose" (222). Alas, things are quite different, at least as regards simplified versions of the theory. So-called computable general equilibrium models have been (and still are) fashionable among large parts of the economics profession and have exerted considerable influence on economic policy. For the past two decades macroeconomics has been under the spell of the rational expectations paradigm. The macroeconomic models used by many central banks, governmental research departments and international institutions involved in advising on economic policy matters are of the "Dynamic Stochastic General Equilibrium" (DSGE) variety (see De Grauwe 2008). These models assume that agents are rational and understand the world in all its complexities. They are typically representative agent models in which the agent is taken to successfully maximise

2 See the cover story of the July 18th 2009 issue of *The Economist* entitled "Modern Economic Theory. Where it went wrong – and how the crisis is changing it", p. 11.

3 As "Sraffians" have not been involved in shaping the policies that led to the current worldwide economic problems, there is little fear that Blaug will put the blame on them.

utility over an infinite time horizon.[4] How could this agent and thus the economic system as a whole ever err and get into trouble? All systemic problems and risks have been assumed away. In these models there is no room for manias, bubbles and crashes. The problem of the distribution of wealth and income is effectively put on one side, since it makes no difference whether the representative agent gets his or her income in terms of wages or profits. It is on the basis of such models that policy recommendations have been formulated and put in place in many countries and by international institutions, such as the International Monetary Fund. It is hard to imagine that the "relevance", in Blaug's sense, of the species of GET under consideration has escaped his attention. If it hasn't, will he still cling to his conviction that generating policy advice is invariably a good thing?

10. A word should be said about the term "Sraffian" or "neo-Ricardian economics" Blaug uses. It was first coined by Marxist economists to distinguish Sraffa's approach to the theory of value and distribution, which explains relative prices and income distribution strictly in terms of quantities of commodities and labor, from the Marxist one, which starts from labor values (Rowthorn 1974). In some contributions the term was used in a derogatory manner contending that like marginalist (or "neoclassical") theory it was a variant of "vulgar" economics, dealing with "appearances" only. Marginalist economists in turn occasionally applied the term to the analyses of those critics who, in the Cambridge controversies in the theory of capital, attacked marginalism, especially its long-period version, showing it to be logically flawed (see, for example, Kurz and Salvadori 1995, ch. 14).[5]

Sraffa saw his 1960 book as setting the classical approach to the theory of value and distribution, shedding the weaknesses of earlier formulations and building on their strengths. As he explained in a document contained in a folder dated "End of November 1927" in his unpublished manuscripts, titled "Principio":

> I shall begin by giving a short "estratto" [extract] of what I believe is the essence of the classical theories of value, i.e. of those which include W. Petty, Cantillon, Physiocrats, A. Smith, Ricardo and Marx. This is not the theory of any one of them, but an extract of what I think is common to them. I state it of course, not in their own words, but in modern terminology, and it will be useful when we proceed to examine their theories to

4 So-called "New-Keynesian" versions of the DSGE model adopt the same basic framework, but add price and wage stickiness to it.

5 We note that according to Blaug "Sraffians scored a great victory in the Cambridge capital theory controversies" (237).

> understand their portata [delivery capacity] from the point of view of our present inquiry. It will be a sort of "frame", a machine, into which to fit their own statements in a homogeneous pattern, so as to be able to find what is common in them and what is the difference with the later theories. (D3/12/4: 12)[6]

The later theories to which Sraffa refers were different variants of demand and supply analysis, or marginalist theory. They include the general equilibrium analyses of Walras and Pareto and the partial equilibrium analysis of Marshall. As we know from Sraffa's papers, both published and unpublished, he was convinced neither by the contributions of Marshall, Walras and Pareto nor by later temporary and intertemporal equilibrium models elaborated by J. R. Hicks and others.

11. Blaug, who claims to be concerned with "historical" reconstructions and despises "rational" ones, puts forward a hollow exemplar of the latter when trying to support his contention that "Sraffian economics is general equilibrium theory" (229). He writes: "It is no accident [!] that Sraffa's book was published in the heyday of general equilibrium theory when it served everyone [!] as the model of rigorous economics" (229). If Blaug was concerned with an historical reconstruction of the case under consideration, he needs to spend some time in Trinity College Library, Cambridge (UK), as we did, in order to study Sraffa's papers and library and find out when Sraffa had arrived at which results, and why. He would then see that his above speculation as well as many other statements he put forward concerning Sraffa's contributions are without foundation; they are pure fiction. Historians of economic thought ought to be aware of the usefulness of archival work.

12. Blaug nowhere defines precisely what he means by a "Sraffian" but uses the attribute to suit his case. For instance, on p. 229 Ian Steedman is "a writer deeply sympathetic to the Sraffian enterprise", but on p. 234 he turns into a fully fledged "Sraffian". Luigi Pasinetti, one of Sraffa's major students, is said to have "veered away from the Sraffian camp with his own approach to growth theory (Pasinetti 1981)" (234). Had Blaug read the preface of the book, he would know that Pasinetti considers his own analysis a development of Sraffa's approach.[7] A

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[6] References to Sraffa's hitherto unpublished manuscripts and correspondence follow the catalogue prepared by Jonathan Smith, archivist of the Wren Library, Trinity College, Cambridge (U.K.).

[7] Luigi Pasinetti kindly sent us a letter in which he insisted: "Blaug is of course wrong in asserting that I veered away from Sraffa. I always considered my multi-sector analysis (cum vertically integrated sectors) a development of Sraffa's approach (as now is much more clearly argued in my latest CUP book – *Keynes and the Cambridge*

similarly problematic judgement is passed on Paolo Sylos-Labini. In order to give credibility to his (in itself rather strange) complaint that "Sraffians" have not contributed to certain themes or fields in economics, Blaug re-labels some authors: in case X has/has not contributed to field Y, he or she is not/is a "Sraffian". We are reminded of the old joke in which a pupil is orally examined by his biology teacher about elephants and answers that elephants have four feet like mice and then goes on to talk about mice.

13. Blaug repeatedly contradicts himself. A case in point is the following one. A few pages after he had stated apodictically that "Sraffian economics … is irrelevant to our understanding of the real world" (221) he stresses that "A book like Bowles, Edwards, and Roosevelt's *Understanding Capitalism* (2005) attractively [!] combines Sraffian ideas [!] on 'the production and reproduction of the surplus product' and the profit rate as the driving force of capitalism" (237). How is it possible that something which is "irrelevant to our understanding of the real world" at the same time provides the basis for "understanding capitalism"? Blaug cannot be accused of being overly concerned with consistent arguments. Several other examples of this sort could be added.

14. For someone who insists that reconstructing the ideas of some other authors should be faithful to what these authors have written (a concern we share), Blaug is in places surprisingly careless. Three examples must suffice. In the context of a discussion of the problem of the gravitation of market prices to their "natural" or normal levels, he contends that while Kurz and Salvadori point out "that little is known about the dynamic behaviour of even simple linear production models; nevertheless, they express the hope that the problem will be 'settled in the foreseeable future' (Kurz and Salvadori 1998[a], 20)" (229 n.20). The reader who checks the source mentioned will not find this statement. Has Blaug got the page wrong? No, in the entire book the reader won't find the statement quoted. Has Blaug perhaps confounded some of our books? Yes, he has, but things are worse still. The only passage we are aware of having written that can be related to Blaug's criticism is contained in a book published in 1995. After having pointed out the extreme complexity of the issue at hand

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Keynesians [Pasinetti 2007] –, which Blaug should be advised to read before pronouncing any negative judgement on my relations with Sraffa). Blaug is also wrong in asserting that no empirical research followed." For explicit statements of how Pasinetti sees the relationship between his own analysis and Sraffa's, see Pasinetti (2007, 278 and 302-304).

There is a substantial literature on empirical work on vertically integrated sectors and, for example, on the high tech content of various commodities and the international competitiveness of industries.

(“gravitation”) and the dependence of the results obtained on the specific conditions assumed, we conclude: “It should then be clear that *there is no fear that the issue of gravitation will be settled in the foreseeable future*” (Kurz and Salvadori 1995, 20; emphasis added).[8] Hence we say exactly the *opposite* of what Blaug contends we are saying. This is not only annoying but also raises doubts about the seriousness of the entire enterprise. What is the relevance of a critique that lacks the elementary rigor of not misrepresenting (let alone reversing) the view of the people criticised? Misconstruction is an error surely worse even than historically unfaithful reconstruction?

Another case of misrepresenting our view is to be found in footnote 22. There Blaug asserts that “my definition [of rational reconstruction] is identical to Lakatos’s” (231 n. 22). This he considers enough to criticise the analysis we provided in order to clarify Lakatos’s concept of “rational reconstruction” and how we use this concept (Kurz and Salvadori 2002a, section 2). With no evidence to support his accusation he contends that “the difficulty is that Kurz and Salvadori do not believe [!] that they are reconstructing anything: they are merely expressing what the classical economists themselves would have said if they had known about simultaneous equations” (231 n. 22). A look at section 2 in our 2002 reply to Blaug shows that Blaug’s criticism is unwarranted. As Keynes wrote in his debate with Hayek, we wonder whether Blaug has “read [our] book[s] with that measure of ‘good will’ which [authors are] entitled to expect of a reader. Until he can do so, he will not see what [we] mean or know whether [we] are right. He evidently has a passion which leads him to pick on [us], but we are left wondering what that passion is” (Keynes, CW, Vol. XIII, 243).

Finally, the following case of misrepresentation of Marshall (1890) and Sraffa (1925, 1926) may be mentioned. Blaug asserts: “Marshall thought that increasing returns to scale external to firms but internal to industries … would be compatible with a perfectly competitive market structure” (235). Interestingly, Blaug uses almost the same wording as Sraffa, who, in section 3 of the 1925 paper, reconstructed rigorously Marshall’s theory of the decreasing cost (i.e. increasing returns) industry and then explained, in section 5, that, in order to be rigorous, Marshall’s presentation of increasing returns industries requires an extra assumption, *not* found in Marshall. “It is necessary”, Sraffa insisted, “that the advantages of increased

[8] To avoid a possible misunderstanding, the observation that rates of profit never seem to deviate “too much” from one another has prompted us to start from the “stylized fact” of a uniform rate of profits in much of our work (Kurz and Salvadori 1995: 21). For a discussion of the problem at hand and arguments in favor of gravitation, see Caminati and Petri (1990) and especially Garegnani’s contribution therein.

production in the industry considered should not have repercussions in any way on the other industries." To this he added the condition that must be met in order for this to be true: "The economies of large scale production must be 'external' from the point of view of the individual firms, but 'internal' from the point of view of the industry" (Sraffa 1925, English translation, 362). Sraffa then refers the reader to Marshall's *Industry and Trade* (1919, 188) where it is maintained that this situation is extremely rare in reality, which implies that the ability of Marshall's theory to explain reality is very limited. Blaug, on the contrary, presents a Marshall who in the *Principles* had allegedly already been conscious of the problem Sraffa raised and who, like Samulson (1971) much later, had drawn the attention to a case in which partial equilibrium analysis arrives rigorously at the same result as general equilibrium analysis!

2. Sraffa's analysis – a "species of Walrasian GET"?

In his 1999 paper Blaug (1999, 229) contended that the Sraffian interpretation of the classical economists "reads Smith and Ricardo and Marx through Walrasian-tinted glasses." Now he writes "that Sraffian economics is quite simply a species of Walrasian GET" (226-7). We have already argued in Section 1 (see observation 10) above that this view cannot be sustained. Here we draw attention to some serious misunderstandings on Blaug's part.

The first concerns the necessary ingredients of a theory. According to Blaug, "A telltale sign of Sraffa's indebtedness to Walrasian GET is his careful separation of endogenous and exogenous variables, or variables that are explained within the theory and variables that are taken as given and explained, if at all, outside the theory." (230) Does Blaug think that the separation of endogenous and exogenous variables is not required in general and that it is a peculiar feature of only a subset of theories? Would Blaug draw attention to *any* theory, economic or other, that does not distinguish between variables that are explained or determined by the theory and variables that are taken as given? In his paper Blaug expresses anew how fond he is of Marshall's partial equilibrium analysis (241). As we asked in Kurz and Salvadori (2003, 16 n.7): Does he wish to claim that Marshall's analysis does not have this separation? Marshall's partial equilibrium approach to a single market is no different in regard to this issue: there are two endogenous variables, the market price of the commodity under consideration and the quantity traded, and there are two types of exogenous variables, the parameters of the demand function and those of the supply function. We cannot believe that it is really necessary to explain such elementary things to our critic. Since *any* theory is based on a separation between endogenous and exogenous variables, and since Blaug does not provide any other criterion to distinguish between theories, applying his logic means that *all* theories could be said to be "indebted" to or to be "quite simply a species of Walrasian GET" – clearly an absurd statement.

Blaug also mistakes the *form* of an argument for its *substance* (see also Kurz and Salvadori 2002a, 226). He thinks that any economic theory that is formulated in terms of simultaneous equations (or inequalities) is *ipso facto* a version, or "species", of Walrasian theory. This is a misconception. The mathematical form of an argument is one thing, the content of the argument another. The determinants of the general rate of interest (or profits) contemplated by Walras and Ricardo (and by modern reformulations of their theories), for example, are very different. Most importantly, whereas in Ricardo the competitive rate of return is seen to depend on real wages (or the share of wages), which in the context of ascertaining this rate and relative prices for a given system of production are taken to be an independent magnitude, in Walras it depends on the given endowment of the economy of productive factors, including capital goods.[9] What matters when distinguishing between theories is *which* endogenous variables are explained or determined by means of *which* exogenous variables, and precisely *how* this is done. Blaug has overlooked the important part of the problem, the *content* of a theory, because of an ill-conceived preoccupation with the mere *mathematical form* in which it is presented.

Having said this, the following should be pointed out. Walras (but not modern Walrasian GET; see Kurz and Salvadori 2004) was concerned with explaining the competitive (uniform) rate of profits and thus with a long-period problem. Since *any* long-period theory has to satisfy Sraffa's price equations, it is no surprise that some version of Sraffa's equations may be found in Walras's analysis (Walras 1954). This is the case in the part of the *Elements* devoted to what Walras called "capitalization".[10] For the full story, see Kurz and Salvadori

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9 For a detailed discussion of Walras's misunderstanding and criticism of Ricardo, see Kurz and Salvadori (2002b).

10 Walras's failure to elaborate a coherent long-period analysis in his treatment of "capitalization", was a main motive of Erik Lindahl, J. R. Hicks and Friedrich August von Hayek to abandon the long-period method in favor of interteporal and temporary equilibrium analyses, which, in turn, paved the way towards modern GET. For a summary account of the relevant literature on this development, see Kurz and Salvadori (1995, chapter 14).

(2002b; see also Kurz and Salvadori 1995, 23-5 and 439-41). Clearly, Walras's principal criticism of Ricardo (a criticism already put forward by William Stanley Jevons and rightly refuted by Knut Wicksell) cannot be sustained: Ricardo did *not* commit the elementary error of trying to determine two unknowns with a single equation. There is no underdeterminacy in Ricardo's analysis of value and distribution. As regards Sraffa's equations, these investigate the relationships that must hold between prices and the distributive variables, given the system of production in use. The equations themselves do not yet constitute an explanation of income distribution. Something more is needed, as, for example, in Ricardo a theory of how the real wage rate (or rather the share of wages) is determined, or in Sraffa the proposition that the rate of profits can be considered as "susceptible of being determined from outside the system of production, in particular by the level of the money rates of interest" (Sraffa 1960, 33).

3. Themes in classical economics allegedly not dealt with by "Sraffians"

As we noted in Section 1 above (observation 3), Blaug now plays down differences in interpreting the classical economists between him and the "Sraffians" as "a question of emphasis" (232). This is fair enough, and we, at least, can easily live with it. However, some of his related statements cannot pass without comment. In this section we rebut Blaug's contention that "Sraffians" have not dealt with a number of problems the classical economists were concerned with. In the following section we rebut his contention that "Sraffians" have nothing to say on important contemporary problems. Blaug prides himself with having a comprehensive knowledge of the "Sraffian" literature. However, as will be seen his knowledge is incomplete and many of his charges are unfounded.

Before we turn briefly to the evidence that contradicts Blaug's contentions, the following remark is apposite. His accusation that "Sraffians", including us, do not deal with every problem the classical authors have been concerned with is peculiar. First, we never claimed that this was our intention. Secondly, Blaug omits to tell the reader why it should have been our intention. Thirdly, it is not an especially fruitful task, because several problems have already been solved or were of only short-lived historical interest.[11] We took up what we consider to be fruitful ideas and concepts possessed of a great explanatory potential.[12]

"Sraffians", including us, have dealt with many more aspects of classical (and non-classical) economics than Blaug seems to be aware of. Several of our publications and works we edited have escaped his attention, and those of our writings to which he refers he apparently has not read with sufficient care, because otherwise he could not make some of his allegations.[13] A few examples must suffice.[14] In footnotes we add some randomly chosen references to the writings of other "Sraffians" that contradict Blaug's respective claims. Many more could of course be given, but space constraints prevent us from doing it.

Blaug contends that many important issues in classical economics are not addressed by "Sraffians". These include the following.

(a) *Foreign trade* (225). Not true: see Kurz (1992) on Adam Smith's "vent-for-surplus" argument on foreign trade. "Sraffian historians of economic thought" are said to "hardly [!] mention Ricardo's doctrine of comparative advantage" (232). Not true: see Kurz and Salvadori (1995, 152 and the literature referred to there) and Kurz and Salvadori (1998b,

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[11] Blaug calls Kurz and Salvadori (1998a) "the most detailed Sraffian history of classical economics to date" (230). This is a surprising description, because the volume contains just a selection of previously published papers of ours. It is not, and was never meant to be, a comprehensive history of classical economics. Blaug's generous decription appears to have a single purpose: it allows him, or so he thinks, to point out that several important themes are absent from the volume. Given the nature of the book this should not come as a surprise. Blaug criticizes books for what they do not contain and authors for what they have not written. Shouldn't the object of criticism be what books actually contain and what authors actually have written? Had Blaug been interested in a fairly comprehensive account of classical economics in which all the problems mentioned by him are dealt with, he might have referred to the two volumes of *The Elgar Campanion to Classical Economics* we edited (Kurz and Salvadori 1998b). While in his 1999 paper he referred extensively to the *Companion*, in his 2009 paper he does not even mention it.

[12] We note incidentally that there is something strange in Blaug's attitude towards our work. On the one hand he is keen to convey the impression that our work on the classical authors is misleading, on the other hand he deplores the fact that we have not written more on them.

[13] Selections of our essays are collected in three easily accessible volumes; see Kurz and Salvadori (1998a, 2003, 2007).

[14] We refer to publications and a work edited by us that are in English and easily accessible.

entries on “Autarky versus trade”, “Comparative advantage”, “Foreign trade”, “Specialization and trade” and “Vent for surplus”).[15]

(b) *Malthusian law of population* (225). Not true: see Kurz and Salvadori (1998b, entry on “Malthus, Thomas Robert”; 2002a; 2003, chapter 2; 2006; 2007, chapter 2). In another place Blaug opines: “But the most astonishing lacuna in the Sraffian characterization of classical economics is the downplaying of the importance of the Malthusian theory of population” (231). No one could sensibly dispute that the law of population plays a role in Ricardo’s writings: the fact is obvious. Much less obvious is precisely *what* importance Ricardo attached to it, when and why. Blaug just states the simple fact mentioned. This is not good enough. First, there are several statements in Ricardo (some of which we have cited in Kurz and Salvadori 2002a, 234) that squarely contradict the law, and that are typically ignored by some interpreters, including Blaug. They show clearly that Ricardo was not the ardent advocate of the law that Blaug portrays him to be. Secondly, in discussions with Malthus on the theory of value and distribution it was convenient for Ricardo to accept the law, because it provided him with a firm basis to stand on in terms of a fixed real wage rate. The rate of profits could then be determined residually. Thirdly, the analytical structure of Ricardo’s surplus-based approach to the problem of value and distribution is entirely independent of the law: The rate of profits is determined residually irrespective of whether wages are at some subsistence level or above it. We made these points in our 2002 reply to Blaug’s earlier criticism. He neither answered them in his rejoinder (Blaug, 2002) nor in his most recent paper. This is a genuine “astonishing lacuna”.

(c) *Falling rate of profit*. “Sraffian economists maintain a steadfast silence on the problem [of the cause of the declining rate of profit] and deliberately so. … [T]hey fail in any way, however, to demonstrate that the rate of profit will decline with the continuous accumulation of capital” (226). And later in his paper he accuses us of being “even more unfair [!] to Marx [than to Ricardo] in saying nothing [!] about the technological causes of the falling rate of profit …, which was the lynchpin of Marx’s pessimistic predictions about the fate of capitalism” (231). Not true: see Kurz (1978, 1998), Salvadori (1981), Kurz and Salvadori (1995, 248, 402 and 474; 1998b, entries on “Accumulation of capital”, “Falling rate of profits”, “Marx, Karl”, “Marx, Karl, as an interpreter of the Classical economists”,

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[15] Ricardo’s approach to trade theory has been investigated in a number of papers by Sergio Parrinello, Stanley Metcalfe and Ian Steedman, and Ian Steedman alone; see the references in Aspromourgos (2004).

"Mathematical formulations of Marxian economics", "Profits"; 2006) and Gehrke and Kurz (2006, 119 et seq.).[16]

Blaug presents Ricardo as a technological pessimist, who believed in the overwhelming importance of the Malthusian law in combination with diminishing returns in agriculture and who saw the stationary state as just around the corner. He insists that "the contention that [Ricardo] was centrally concerned, nay obsessed, with the threat posed by the rising marginal cost of growing wheat to feed workers is a point of total agreement among historians of economic thought" (225). This interpretation does not do justice to Ricardo. As early as in *The Essay on Profits* of 1815 he expressed the view that there are no signs pointing in the direction of a deceleration of capital accumulation in the foreseeable future: "we are yet at a great distance from the end of our resources, and … we may contemplate an increase of prosperity and wealth, far exceeding that of any country which has preceded us" (Ricardo, *Works* IV, 34). This view is confirmed in a letter to Hutches Trower of 5 February 1816, in which he concluded from the fall in grain prices since 1812 that "we are happily yet in the progressive state, and may look forward with confidence to a long course of prosperity" (Ricardo, *Works*: VII: 17). And in his entry on the "Funding System" for volume IV of the *Supplements to the Encyclopædia Britannica*, published in September 1820, he stressed that "the richest country in Europe is yet far distant from that degree of improvement", that is, the stationary state, and that "it is difficult to say where the limit is at which you would cease to accumulate wealth and to derive profit from its employment" (Ricardo, *Works* IV: 179).

(d) *Economic growth*. "Sraffian economics has nothing whatever to say about [economic growth in Adam Smith]" (226). Not true: see, in particular, Kurz (1997), Kurz and Salvadori (2003, chs 6 and 7; 2008); see also Salvadori (2003). We are said to "utterly [!] bypass the central message of [Ricardo's *Principles*] about the depressing effect of diminishing returns in agriculture on the general rate of economic growth" (231). Not true: see Kurz (1978, 1997, 1998), Kurz and Salvadori (1992; 1998b, entries on "Growth"; 2006) and the discussion under item (c).[17]

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16 See also Schefold (1976), Steedman (1977) and Garegnani (1987).

17 Blaug seems to have also overlooked the large and swiftly growing literature that combines a classical perspective on production, value and distribution with Keynes's *principle of effective demand* within a growth context, and which shows that effective demand matters not only in the short, but also in the long run. To this literature contributed Pierangelo Garegnani, Amit Bhaduri and Steven Marglin, and Amitava K. Dutt, to mention but a few scholars.

(e) *Division of labour*. Blaug insists that we "totally [!] ignore the opening three chapters of *The Wealth of Nations* on the division of labor" (231). Not true: see Kurz and Salvadori (1995, 17-18; 1998b, "Accumulation of capital", "Division of labour", "Smith, Adam"; 2003, chapters 6 and 7).

(f) *Say's Law of Markets*. "Sraffians" are said to "hardly [!] mention Ricardo's vigorous defense of Say's Law of Markets" (232). Not true: see Gehrke and Kurz (2001) and Kurz and Salvadori (1998b, entries on "Effective demand", "Limiting and regulating principles", "Say, Jean-Baptiste", "Say's law", "Supply and demand").

This should suffice to dispell Blaug's contention that "Sraffians" did not deal with these problems.

4. "Sraffian economics" – fruitful or not?

Blaug contends that "Sraffian economics" is barren and irrelevant "to our understanding of the real world and … to the major concerns of modern economists" (221), and that it has nothing to say on economic policy matters. However, since he also believes that from a classical perspective useful things can be said about the real world and since he now overall endorses Sraffa's interpretation of the classical economists, it would indeed be surprising could he consistently entertain this dismissive stance throughout his paper. As we have seen, he cannot and in fact contradicts himself (see observation 13 in Section 1). However, he nevertheless tries to convey the impression that "Sraffians" have nothing to say on the economic problems of today. To do this, he has to ignore large parts of the existing literature. A look into such easily accessible journals as (in alphabetical order) the *Cambridge Journal of Economics*, *Contributions to Political Economy*, *Metroeconomica,* or the *Review of Political Economy*, not to speak of numerous monographs and articles published in books, would have exposed him to a great many works dealing inter alia with applied and policy oriented work, including such topics as effective demand and economic growth, taxation, the pension system, fiscal retrenchment, monetary policy, the economics of disarmament, employment policy, health care, environmental policy, the economics of waste management, the situation of Indian agriculture etc. Blaug who on various occasions has prided himself with being particularly well read would not even have had to muster the energy to collect easily available information himself. He could have simply consulted Tony Aspromourgos's survey article (Aspromourgos 2004), which provides more than two hundred references to the literature and contains a detailed and highly informative account of the Sraffa-inspired extant economic literature at the beginning of the new millennium, on which problems and by whom.[18] And he might have had a look at the April 2007 issue of the *Review of Political Economy*, which contains a symposium on precisely this question of the practical implications of Sraffa's resumption of the classical approach. What are we to make of Blaug's sweeping statements that ignore the relevant literature?

5. Concluding remarks

None of Blaug's criticisms stands up to close examination. He attributes views to us (and to other authors) we (they) never advocated. He contends that "Sraffian" authors have not written about certain problems, while referring to writings which show precisely the opposite. He commits a number of elementary blunders and mistakes the mathematical form of an argument for its content. He variously contradicts himself in the paper. He puts forward bold statements that are contradicted by the facts.

Interestingly, he no longer maintains that there are fundamental differences between the "Sraffian" interpretation of the classical economists and his own interpretation; the differences, he surmises, are only "a question of emphasis". A careful investigation of Blaug (1958) shows how much the young Blaug was indebted to Sraffa's interpretation of Ricardo. It seems that the mature Blaug has come close to where he once started from.

As regards Blaug's contradistinction of rigor and relevance, it is obvious that an economic analysis that rightly wishes to gain respect should seek to be *both* rigorous *and* relevant. The classical approach to economics, we are convinced, is on the right track.

References

Aspromourgos, Tony. 2004. Sraffian Research Programmes and Unorthodox Economics. *Review of Political Economy* 16.2: 179-206.

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[18] In the meantime many more contributions have been published. In preparation of this reply we have sent letters to many people whom Blaug would in all probability dub "Sraffians" of sorts. They kindly sent us detailed information especially about papers and books published during the past few years that deal with applied and policy related problems. We should like to express our gratitude to them. Due to space constraints we cannot provide the details of their valuable responses to our letter.

Blaug, Mark. 1958. *Ricardian Economics: An Historical Study*. New Haven, Connecticut: Yale University Press.

Blaug, Mark. 1999. Misunderstanding Classical Economics: The Sraffian Interpretation of the Surplus Approach. *HOPE* 31.2:213-236.

Blaug, Mark. 2002. Kurz and Salvadori on the Sraffian Interpretation of the Surplus Approach. *HOPE* 34.1:237-240.

Blaug, Mark. 2009. The Trade-Off between Rigor and Relevance: Sraffian Economics as a Case in Point. *HOPE* 41.2:219-247.

Bowles, Samuel, Richard Edwards, and Frank Roosevelt. 2005. *Understanding Capitalism*. New York: Oxford University Press.

Caminati, Mauro and Fabio Petri, eds. 1990. *Convergence to Long-Period Positions*. Special issue of *Political Economy. Studies in the Surplus Approach*, 6.

De Grauwe, Paul. 2008. DSGE-Modelling: When Agents are Imperfectly Informed. ECB Working Paper No. 897. Available at SSRN: http://ssrn.com/abstract=1120763

Garegnani, Pierangelo. 1987. Surplus Approach to Value and Distribution. In *The New Palgrave Dictionary of Political Economy*, edited by J. Eatwell, M. Milgate, and P. Newman. London: Macmillan, vol. 4, 560-74.

Garegnani, Pierangelo. 2002. Misunderstanding Classical Economics? A Reply to Blaug. *HOPE* 34.1:241-254.

Gehrke, Christian and Heinz D. Kurz. 2006. Sraffa on von Bortkiewicz: Reconstructing the Classical Theory of Value and Distribution. *HOPE* 38.1:91 -149.

Keynes, John Maynard. 1973 ssq. *The Collected Writings of John Maynard Keynes*. 32 vols, managing eds A. Robinson and D. Moggridge, London: Macmillan. In the text referred to as *CW*, volume number and page number.

Kurz, Heinz D. 1978. Rent Theory in a Multisectoral Model. *Oxford Economic Papers* 30:16-37.

Kurz, Heinz D. 1992. Adam Smith on Foreign Trade: A Note on his “Vent-for-Surplus” Argument. *Economica* 59:475-81.

Kurz, Heinz D. 1997. What Could the “New” Growth Theory Teach Smith or Ricardo?. *Economic Issues* 2.2:1-20. Reprinted in Kurz and Salvadori (2003).

Kurz, Heinz D. 1998. Marx on Technological Change: The Ricardian Heritage. In R. Bellofiore (ed.), *Marxian Economics. A Reappraisal*, vol. 2. Houndmills, Basingstoke and London: Macmillan, 119-138.

Kurz, Heinz D., and Neri Salvadori. 1992. Morishima on Ricardo: A Review Article. *Cambridge Journal of Economics*. 16:227-247.

Kurz, Heinz D., and Neri Salvadori. 1995. *Theory of Production. A Long-period Analysis*. Cambridge: Cambridge University Press.

Kurz, Heinz D., and Neri Salvadori. 1998a. *Understanding "Classical" Economics. Studies in Long-Period Theory*. London and New York: Routledge.

Kurz, Heinz D., and Neri Salvadori, eds. 1998b. *The Elgar Companion to Classical Economics*. Two vols. Aldershot (U.K.): Edward Elgar.

Kurz, Heinz D., and Neri Salvadori. 2002a. Mark Blaug on the "Sraffian Interpretation of the Surplus Approach". *HOPE* 34.1:225-236.

Kurz, Heinz D., and Neri Salvadori. 2002b. One Theory or Two? Walras's Critique of Ricardo. *HOPE* 34.2:365-398.

Kurz, Heinz D., and Neri Salvadori. 2003. *Classical Economics and Modern Theory. Studies in Long-Period Analysis*. London and New York: Routledge.

Kurz, Heinz D., and Neri Salvadori. 2004. Von Neumann, the Classical Economists and Arrow-Debreu: Some Notes. *Acta Oeconomica* 54.1:39-62.

Kurz, Heinz D., and Neri Salvadori. 2006. Endogenous Growth in a Stylised "Classical" Model". In G. Stathakis and G. Vaggi, eds, *Economic Development and Social Change. Historical Roots and Modern Perspectives.* London and New York: Routledge, pp. 106-124.

Kurz, Heinz D., and Neri Salvadori. 2007. *Interpreting Classical Economics. Studies in Long-Period Analysis*. London and New York: Routledge.

Kurz, Heinz D. and Neri Salvadori. 2008. New Growth Theory and Development Economics. In Amitava Krishna Dutt and Jaime Ros, eds, *International Handbook of Development Economics*. Cheltenham (U.K.): Edward Elgar, pp. 207-222.

Marshall, Alfred. 1890. *Principles of Economics*. 8th edn 1920. Reprint, reset. London: Macmillan, 1977.

Marshall, Alfred. 1919. *Industry and Trade. A Study of Industrial Technique and Business Organization*. London: Macmillan & Co.

Pasinetti Luigi L. 1981. *Structural Change and Economic Growth. A Theoretical Essay on the Dynamics of the Wealth of Nations*. Cambridge: Cambridge University Press.

Pasinetti, Luigi L. 2007. *Keynes and the Cambridge Keynesians: A "Revolution in Economics" to be Accomplished.* Cambridge: Cambridge University Press.

Ricardo, David. 1951 ssq. *The Works and Correspondence of David Ricardo*. 11 volumes. Edited by Piero Sraffa with the collaboration of Maurice H. Dobb. Cambridge: Cambridge University Press. In the text referred to as *Works*, volume number and page number.

Rowthorn, Robert. 1974. Neo-Classicism, Neo-Ricardianism and Marxism. *New Left Review* I/8: 63-87.

Salvadori, Neri. 1981. Falling Rate of Profit with a Constant Real Wage: An Example. *Cambridge Journal of Economics* 5: 59-66.

Salvadori, Neri, ed. 2003. *The Theory of Economic Growth: A "Classical" Perspective*. Cheltenham (U.K.): Edward Elgar.

Samuelson, Paul A. 1971. An Exact Hume-Ricardo-Marshall Model of International Trade. *Journal of International Economics*. 1:1-18. Reprinted in R. C. Merton (ed.), *The Collected Scientific Papers of Paul A. Samuelson*, Vol. III, 1972, Cambridge, MA: The MIT Press, 356-73.

Schefold, Bertram. 1976. Different Forms of Technical Progress. *Economic Journal* 86:806-19.

Sraffa, Piero. 1925. Sulle relazioni fra costo e quantità prodotta. *Annali di Economia* 2:277-328. English translation by John Eatwell and Alessandro Roncaglia in Luigi L. Pasinetti, ed., *Italian Economic Papers*. Bologna: Il Mulino and Oxford: Oxford University Press, 1998, pp. 323-63 and in Heinz D. Kurz and Neri Salvadori, eds, *The Legacy of Piero Sraffa*. Cheltenham (U.K.): Edward Elgar, 2003, Vol. 1, pp. 3-43.

Sraffa, Piero. 1926. The Laws of Returns under Competitive Conditions. *Economic Journal* 36:535-50.

Sraffa, Piero. 1960. *Production of Commodities by Means of Commodities*. Cambridge: Cambridge University Press.

Steedman, Ian. 1977. *Marx after Sraffa*. London: New Left Books.

Walras, Léon. 1954. *Elements of Pure Economics*. London: George Allen & Unwin. English translation by W. Jaffé of the definitive edition of Léon Walras, *Eléments d'économie politique pure*, 1874. Lausanne: Corbaz. 5th and definitive edn Paris: F. Richon, 1924.