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A Response to My Critics

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For almost a decade I have been trying to put forward an alternative interpretation of the works of Piero Sraffa, particularly his book (Sraffa 1960). This interpretation of mine was first put forward in a chapter on Sraffa in my first book titled *Theories of Value from Adam Smith to Piero Sraffa* (Sinha 2010) and later in a more comprehensive book on Sraffa titled *A Revolution in Economic Theory: The Economics of Piero Sraffa* (Sinha 2016). Both these works have come under heavy criticisms from the established 'Sraffian' quarters. In this paper, I try to respond to those criticisms.

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Introduction

It is almost a decade now since I have been trying to put forward an alternative interpretation of Sraffa's work (see Sinha 2010) that challenges the dominant interpretation given to it by Pierangelo Garegnani and his associates. My first book came under attack from the Sraffian quarters and now that my second and a more comprehensive book on Sraffa is out (Sinha 2016) the trend appears to be continuing. In this paper I present a response to my critics in the hope of clarifying my position and furthering the understanding of the underlying theoretical issues in relation to Sraffa exclusively. Since my first book, *Theories of Value from Adam Smith to Piero Sraffa* deals with four authors: Adam Smith, Ricardo, Marx and Sraffa, I have decided to deal with the criticisms of other chapters elsewhere (Sinha 2018a). The first and the longer part of the response relates to the critical reviews of my first book (Sinha 2010) and the second part deals with two critical reviews that I have come across of my second book (Sinha 2016). I take this opportunity to first of all thank all my colleagues who wrote reviews of my book, only the critical ones that I have come across are taken up in this paper though. The ones that were in languages other than English, I unfortunately could not read.

Part 1: *Theories of Value from Adam Smith to Piero Sraffa*: Chapter on Sraffa

There are mainly three issues raised by my critics on Sraffa: (i) I'm wrong in suggesting that Sraffa eschews the notion of classical equilibrium of effectual demand and quantity supplied; (ii) the idea of 'commodity residue' that I highlight has no theoretical significance; and (iii) I am incorrect in suggesting that the condition of 'uniform rate of profits' in Sraffa's system of equations is a structural property of his economic system. Below I take up these criticisms one by one.

Point (i): On the question of equilibrium

Typical examples of criticisms on point (i) can be found in the reviews by Tony Aspromourgos (2012), Levrero (2012) and Reati (2012, 2014):

In the Sraffa chapter, Sinha immediately confronts a difficulty (278–9). On the one hand, he wants to read Sraffa's (1960: v) prefatory statement, concerning those readers who are 'accustomed to think in terms of the equilibrium of demand and supply', as referring not merely to marginalist theory, but to any theory in which supplies and demands, in some sense of those terms, exhibit an equality. On the other, Sraffa himself (ibid.) immediately describes his standpoint as 'that of the old classical economists from Adam Smith to Ricardo'. But the classical economists clearly were theorising situations in which commodity demands and

supplies (in some sense) were balanced, while not having recourse to demand and supply functions along the lines of latter-day marginalist theory. Isn't it then obvious that Sraffa must intend by 'the equilibrium of demand and supply', to denote just the latter-day theory? (Aspromourgos 2012, p. 492-3).

While it is true that in *Production of Commodities by Means of Commodities*, Sraffa says nothing about the process that equalizes the different rates of profits in the long period, he explicitly identifies his analysis with "that of the old classical economists from Adam Smith to Ricardo," who certainly did adopt the gravitation mechanism I have just described (see Sraffa 1960: v). It is therefore reasonable to suppose that Sraffa was thinking along those same lines. (Reati 2014, p. 403)

In Chapter IV, Sinha interprets Sraffa's (1960, p. v) statement that his prices do not depend on 'the equilibrium of supply and demand' as if this implies the rejection of any consideration of those prices as 'centres of gravitation' for actual prices—and not simply the rejection, as seems obvious, of the marginalist theory versus that of the 'old classical economists'. (Levrero, 2011, p. 530).

Unfortunately none of my critics bother to tell their reader that I anticipate their argument in the book (see pp. 327-9) and answer them in advance to which they prefer to close their eyes. However, before coming to the arguments and evidence produced in the book, which, I repeat, my critics have not cared to engage with, let me point out that in 1960 when Sraffa's book was published, Garegnani was a yet to be an established entity in the intellectual world and the whole idea of classical gravitation mechanism being radically different from the so-called Neoclassical idea of market mechanism did not even exist. No established historian of economic thought, who Sraffa would have taken seriously such as Cannan, Stigler, Schumpeter, etc., had made this argument and, of course, no pioneer of Neoclassical economics such as Jevons, Walras or Marshall had made Adam Smith's and Ricardo's argument in relation to free competition and the tendency for the industrial rate of profits to equalize to be an issue between them. So, on what grounds Sraffa could have naturally assumed that his readers would distinguish his general statement about 'equilibrium of demand and supply' as 'obviously' applying specifically to the intersections of Neoclassical demand and supply functions and not equilibrium of supplies and effectual demand points?¹ Furthermore, in his Cambridge PhD dissertation submitted in 1959, Garegnani explicitly accepts that 'Smith and Ricardo's theory of price is founded on the assumption of constant returns to scale for manufactures' (Garegnani 1959, p. 29, f.n. 2). It was only after reading Sraffa's (1960) 'Preface', in which Sraffa explicitly states that in his book 'there is,

however, no such assumption made', Garegnani realized that he had a problem to solve, i.e., how to reconcile his understanding of classical economics, which rested on the assumption of constant returns, with the standpoint of Sraffa's book, which claimed to represent the classical standpoint? Garegnani's tentative solution was to put classical equilibrium into Sraffa's book and remove constant returns to scale from classical economics. Not only do I show in the book that Garegnani did not succeed in this attempt but there is enough evidence in his own writings that show that his followers have no reason to believe in the 'obviousness' of this distinction that Sraffa could have naturally assumed that his readers would 'obviously' understand.

For example, Garegnani in 1976 writes:

The study of the permanent effects of changes by means of comparisons between positions of the economic system characterized by a uniform rate of profits was in fact the method used by Ricardo and the English classical economists, when they explained profits in terms of the surplus product left after paying wages at the rate determined by independent economic or social circumstances. *But fundamentally the same method was preserved after Ricardo, across the deep change which the theory underwent in favour of a symmetric explanation of profits and wages in terms of the equilibrium between the forces of demand and supply for labour and capital.* ... It was only in the last few decades that this method, which was centred on 'long-period positions' of the system ... was increasingly challenged: ... this departure from tradition has not been due to weaknesses of the method as such, but rather to weaknesses of the dominant theory of distribution and, in particular, of the conception of capital it relies on. (Garegnani, 1976, pp. 25-26, emphasis added).

So if Garegnani himself believed in 1976 that the idea of 'long-term' equilibrium was common to both the traditions then obviously this could not be the point Sraffa was referring to in his 'Preface' as the 'classical standpoint'; since, according to Sraffa, the reference is to the standpoint that 'has been submerged and forgotten since the advent of the marginal method' (Sraffa 1960, p. v). Surely, Sraffa's reference to 'since the advent of the marginal method' does not relate to Hick's (1939) 'temporary' equilibrium or Arrow-Debreu's (1954) 'inter-temporal' equilibrium. So, on what grounds do Garegnani's followers keep claiming that Sraffa had naturally assumed that not only a deep divide on this score existed but everybody must be so aware of it that they would 'obviously' interpret his reference to be pointing to one 'equilibrium' and not the 'other'?

Now, let me come to the question of constant returns to scale, which is intricately linked with the question of equilibrium in classical economics. We have already noted that in 1959 Garegnani had maintained that classical economics was 'founded on the assumption of constant returns to scale'. After the publication of Sraffa's book (Sraffa 1960) Garegnani dropped this position and developed a

vague idea that the classical centre of gravitation was just an attractive point for ‘market prices’ and it did not need any assumption of constant returns to scale. Elsewhere (see Sinha 2015) I have discussed this issue in great details and given the space constraint here, I refer the reader to that publication. Here I would only mention that when push came to shove, Garegnani accepted that the classical centre of gravitation does assume constant returns to scale:

However, Ricardo treated decreasing returns from land, just as Smith had treated the increasing returns from division of labour: as relevant, that is, only for the comparatively large output changes involved in capital accumulation and growth. Unlike what happens in neoclassical theory, Smith and Ricardo *could therefore leave physical returns to scale quite naturally aside when dealing with relative prices in a given position of the economy, with the kind of comparatively small output changes generally involved in that specific analysis.* (Garegnani 2007, p. 188, emphasis added)

The reader should have noticed that in 1976 Garegnani distinguished the Classical centre of gravitation from the notion of equilibrium of the Moderns on the basis of time allowed to elapse—Classical was ‘long-period’, i.e., allowed a long period of time for the adjustment to work itself out whereas the Moderns allowed a very little time or no time. Now it appears that he thinks the Classical quantity adjustments allow for very small adjustments whereas the Moderns allow large adjustments. One wonders, if Classicists allow for only very small adjustments then why do they need long-period? All this shows that there is something amiss. In any case, the ‘comparatively small output changes’ that Garegnani refers to above are the precise conditions for which the Neoclassical supply functions are well defined—i.e., they are well defined only in the neighbourhood of the equilibrium point, e.g., any large change in output would clearly break the Marshallian assumption of *ceteris paribus*. Thus to suggest that returns to scale are irrelevant to ‘small output changes’ is a red herring. What Garegnani is doing above is accepting, albeit very reluctantly, that the Classical adjustment of ‘market prices’ to ‘natural prices’ needs the constant returns to scale assumption. Now it is a problem for Garegnani’s followers to square it with Sraffa’s claim that he makes no such assumption.

The importance of the *absence* of the assumption of returns to scale was crucial to Sraffa because on it depended the *novelty* of his theory. After stating in the first paragraph of the ‘Preface’ that ‘no question arises as to the variation or constancy of returns’ (Sraffa 1960, p. v), he revisits this issue on the next page:

The temptation to presuppose constant returns is not entirely fanciful. It was experienced by the author himself when he started on these studies many years ago—and it led him in 1925 into an attempt to argue that only the case of constant returns was generally consistent with the premises of economic theory. And what is more, when in 1928 Lord Keynes read a draft of

the opening propositions of this paper, he recommended that, if constant returns were *not* to be assumed, an emphatic warning to that effect should be given. (Sraffa 1960, p. vi).

Here the ‘premises of economic theory’ stands for Marshall’s theory. And in Sraffa’s 1925 paper we read, ‘The case of constant costs, rather than those of increasing or decreasing costs, should be regarded as normal. This must have been Ricardo’s opinion, since he states that commodities which can be produced at constant costs constitute “by far the greatest part of the goods that are daily exchanged on the market”’ (Sraffa [1925] 1998, p. 354). And before this, we were told in the same paper that “It can be said that all classical writers accept implicitly, as an obvious fact, that cost is independent of quantity, and they do not bother to discuss the contrary hypothesis’ (*ibid*, p. 325). Thus Sraffa was quite explicit in 1925 that the Classicists assume constant returns and therefore it becomes a curious problem as to what is the nature of the *emphatic absence* of this assumption in Sraffa’s book in 1960, and how does it relate to what he refers to as the ‘classical standpoint’? I shall presently return to this question—a question my critics never ask.

However before that, let me first reregister some evidence from Sraffa’s book to show that it would be incorrect to assume that the output quantities in Sraffa’s equations are assumed to be in the Classical centre of gravitation. First of all, as already mentioned several times, the ‘Preface’ of Sraffa’s book begins with a clarion declaration:

Anyone accustomed to think in terms of the equilibrium of demand and supply may be inclined, on reading these pages, to suppose that the argument rests on a tacit assumption of constant returns in all industries. If such a supposition is found helpful, there is no harm in the reader’s adopting it as a temporary working hypothesis. In fact, however, no such assumption is made. No changes in output and (at any rate in Parts I and II) no changes in the proportions in which different means of production are used by an industry are considered, so that no question arises as to the variation or constancy of returns. 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 proportions of ‘factors’ (Sraffa 1960, v).

Let us unpack the arguments made in this paragraph. (i) Those readers who are accustomed to think of ‘price theory’ in terms of ‘equilibrium of demand and supply’ would think that the propositions published in the book are based on an *implicit assumption* that constant returns prevail in all the industries. But why would Sraffa think so? It is because bringing in the idea of ‘equilibrium of demand and supply’ to his propositions would *logically* imply the assumption of constant returns, as we have already discussed above (and for more exhaustive discussion see Sinha 2015). (ii) However, his propositions do not rest on the assumption of constant returns. Therefore, thinking in terms of

‘equilibrium of demand and supply’ is not the correct way of approaching his book. (iii) This approach, which is not to think in terms of ‘equilibrium of demand and supply’, is so revolutionary or novel that the first reading of the book may not make any sense to the reader if she does not bring in the *illegitimate* point of view that constant returns and therefore the equilibrium of demand and supply are tacitly assumed. Hence, the reader may assume them as ‘a temporary working hypothesis’. However, a correct understanding of the book must finally enable the reader to jettison this working hypothesis. (iv) The idea of ‘returns to scale’, constant or otherwise, has meaning only in situations when changes in the output are contemplated. His propositions are, however, built on the idea that no such changes take place—neither in the outputs nor in the inputs for Parts I and II, i.e., Chapters 1-11, and only in the inputs in the last Chapter 12.

Secondly, on the very first page of Chapter 1, Sraffa refers to the data of his equations as ‘A year’s operations can be *tabulated* as follows’ (Sraffa 1960, p. 3, emphasis added). Then again he refers to his system of equations on page 22 of the book as the ‘actual economic system of observation’:

Such a relation is of interest only if it can be shown that its application is not limited to the imaginary Standard system but is capable of being extended to the *actual economic system of observation*. (Sraffa 1960, p. 22, my emphasis).

The expression ‘the actual economic system of observation’ should not leave any room for speculation or spin, it definitely does not refer to an *ideal* situation that would prevail in the Classical equilibrium. Garegnani accepts that it is most unlikely to be ever observed, ‘... no economist had previously supposed the economy to ever actually *be* in equilibrium position, or more generally in a position of rest, except by fluke’ (Garegnani, 2012, p.1429). But even this could not deter Fratini (2012), a student of Garegnani, to deny the obvious interpretation:

[Sinha] interprets the expression ‘real system’ that sometimes appears in Production of Commodities as an ‘empirical system of production’ (p. 290) or ‘post factum description of an economy after a production cycle is over’ (p. 307). In other words, in this particular view, the given outputs in Sraffa’s equations are not equal to the quantities in the effectual demand, as is commonly understood, but ‘actual supplies’ (p. 324). The prices that solve the equations are therefore not Smith’s natural prices because they are not and cannot be at the centre of a process of gravitation. ...First, on reading Sraffa’s book, it is easy to see that he uses the expression ‘real system’ as the opposite of the ‘Standard system’. It is not therefore to be understood as an actually observed input–output table, as Sinha claims, but rather and quite simply as the system in which the quantities are given magnitudes, unlike the ‘Standard system’, where the proportions among sectors are instead endogenously determined. (Fratini 2012, p. 103)

I leave the reader to come to his/her own conclusion.

Thirdly, In Appendix B of the book, Sraffa discusses a case of a non-basic good, 'beans', which uses a very large proportion of itself in its production, implying that its rate of profit cannot exceed the ratio of its own net output to its input. Sraffa discusses the problem with the assumption of positive prices for all goods in this case when the rate of profits of the basic goods industries is higher than the one the 'beans' can admit. This problem, however, cannot arise if the system was assumed to be at the centre of gravitation, as the gravitation mechanism would ensure that the 'beans' industry disappears in the process. So, how could Sraffa find it to be a technical problem significant enough to be assigned a full appendix? Though I have raised this question repeatedly in publications and conferences but to the best of my knowledge no 'Sraffian' has ever responded to this.

Furthermore, I point out all the explicit assumptions made by Sraffa in his book, which shows that he is very particular about stating his assumptions even when they are not significant in nature. I also list all the ideas Sraffa thinks he has borrowed from the Classical economics (listed in Appendix D of Sraffa 1960) and show that here again there is no mention of Adam Smith and the idea of centre of gravitation. So the question arises: why would such a careful author as Sraffa tell his readers at the outset not to bring the baggage of 'equilibrium of demand and supply' to his book and then go on to *implicitly* assume it throughout the book—an assumption on which all his propositions would depend?

Now, when we move from Sraffa's book to his unpublished notes, we find more evidence that he did not consider his equations to be in any sort of equilibrium. During the period 1942-44, when Sraffa was working on his 'Hypothesis', which was to show that the ratio of net output to aggregate inputs (i.e., output-capital ratio) remains constant when the actual rate of profits takes values from zero to its maximum. Sraffa for a few months worked on the hypothesis that the statistics of the aggregate of the inputs and outputs of any empirical system could be taken to be in the Standard proportion; and he thought he could defend this hypothesis on the basis of the 'law of large numbers'. Now it is highly unlikely that Sraffa implicitly assumed that people's tastes must also be such that effectual demands would also turn out to be in the Standard proportion. But more importantly, once Sraffa realized that rescaling of an equation system does not affect the mathematical properties of it, he abandoned his previous hypothesis and went on to search for the appropriate rescaling of any empirical system to a Standard system. One great advantage of this procedure, Sraffa thought, was that now he could take account of even 'non-repetitive' systems. By 'non-repetitive' system Sraffa means a system that has some industries with more aggregate inputs

used than outputs produced and hence in this case a pure physical net output or surplus in physical terms is not well defined. Such situations are typical of industries that are being phased out due to emergence of new technologies etcetera and are important consideration in the context of accumulation:

The importance of this question is not its applicability to the arbitrarily transformed system. But the possibility of extending it (Hypo) from a strictly repetitive to a non-repetitive system (e.g. accumulating) where (as in transformed system) not all l.h.{left hand} commodities reappear on r.h.s.{right hand side} (D3/12/35: 42).

Clearly, this is not a case of any kind of 'equilibrium'. Again, during the early period (1927-31) of his theoretical breakthrough, Sraffa referred to his object of analysis as: 'Its object is, as it were, the photograph of a market place' (D3/12/7: 116); and as late as 1968, eight years after the publication of the book, Sraffa, in his response to a letter from a German student, Soltwedel, wrote: 'As regards your own interpretation, I must say frankly that you have gone astray the moment you speak of "equilibrium" or of "elasticity of factor supply": all the quantities considered are what can be observed by taking a photograph, there are no rates of change, etc.' (C 294: 3, dated 1.3.68). Clearly, no one would use the metaphor of a 'photograph' for something that does not physically exist, as the idea of the centre of gravitation'. Furthermore, as late as 1957, Sraffa had thought of starting the 'Preface' to his book in this manner:

This is not proposed as a complete system of equilibrium. The data assumed are not sufficient to determine either distribution or values. Only the effects of hypothetical, arbitrarily assumed extra data (such as wages, or the rate of profits) are discussed. {...} It is offered as a preliminary and there is no a priori reason why, on the basis of it, an equilibrium system should be built: there is some room left for it, as this is confessedly indeterminate; but the question is whether there is room enough for the marginal system. (D3/12/46: 20, dated 2.4.1957).

As a matter of fact, the beginning of the original sentence was 'This is no system of equilibrium' then the 'no' was crossed out and in its place was inserted 'not proposed as a complete'. All this is yet not an exhaustive list of evidence in support of my position, but I hope it is more than sufficient to rebut my critics on this crucial point.

Now, let us look at the evidence my critics repeatedly present against my position as the trump card that overrides all evidence on the other side. It turns out to be a singular quotation that appears on page 9 of Sraffa's book and at the first sight appears to support my critics' position. Here is an example from Levrero:

Indeed Sraffa (1960, p. 9) indicates that his prices are identical to classical long-period prices when he remarks that ‘Such classical terms as “necessary price”, “natural price” or “price of production” would [serve my purpose], but value and price have been preferred as being shorter and in the present context (which contains no reference to market prices) no more ambiguous.’ What Sraffa rejects is only the determination of those prices in terms of the neoclassical forces of demand and supply. (Levrero, 2012, p. 533).

The reader should, however, note that nowhere in the quotation does Sraffa say that his prices are ‘*identical* to long-period prices’. Nor has Sraffa anywhere endorsed the ‘long-period price’ interpretation of those terms. As we shall see, he actually has a very different interpretation. It is also highly curious that Levrero felt the need to expunge three words from Sraffa’s original quotation, which said ‘meet the case’ and replace it with three words of his own ‘serve my purpose’. What does removing of Sraffa’s own words and putting Levrero’s words in his mouth do? Well, ‘meet the case’ obviously refers to ‘the case’, i.e., the context in which the statement appears but the expression ‘serve my purpose’, on the other hand, refers to a ‘purpose to which those words could be put to serve’. The design is to remove the statement from its immediate context and place it in a general context of the ‘purpose of the book’. Very clever, indeed! Now that the murder is out, let us get down to the post-mortem of the body.

This quotation from Sraffa appears in the second chapter of his book. In the first chapter Sraffa establishes the fact that the production equations contain sufficient information to uniquely determine prices in a subsistence economy—no information from human psychology such as effectual demand etcetera are needed. In the second chapter Sraffa allows those equations to produce surplus output and the question is: can prices be still determined without bringing in additional information from the side of demand? At this stage Sraffa argues that on the condition that the rate of profits ‘must be uniform’, prices are yet again uniquely determined by the information contained in the equations of production. This brings him to the conceptual distinction between basic and non-basic goods. In this context Sraffa writes:

It is desirable at this stage to explain why *the ratios which satisfy the conditions of production* have been called ‘values’ or ‘prices’ rather than, as might be thought more appropriate, ‘costs of production’.

The latter description would be adequate so far as *non*-basic products are concerned, since, as follows from what we have seen in the preceding section, their exchange ratio is merely a reflection of what must be paid for means of production, labour and profits in order to produce them—there is no mutual dependence.

But for a basic product there is another aspect to be considered. Its exchange-ratio depends as much on *use* that is made of it in the production of other basic commodities as on the extent to which those commodities enter into its production. (One might be tempted, but it would be misleading, to say that ‘it depends as much on the Demand side as on the Supply side’). (pp. 8-9, first emphasis added).

Here Sraffa claims that the exchange ratios derived from the ‘surplus’ equation system by adding a *uniform* rate of profits as an unknown ‘satisfy the *condition* of production’. And the fact that the price of a basic commodity enters as cost in the production of all other commodities whereas other basic commodities’ prices enter as cost in its price renders the idea of determining ‘price’ of a commodity by its ‘cost of production’ meaningless, since there is no one-way avenue of determining cost prior to and independent of prices. Then in a parenthetical remark he adds that ‘[o]ne might be tempted, but it would be misleading, to say that “it depends as much on the Demand side as on the Supply side”’. The point to note is that Sraffa says that the price of a *basic* commodity ‘depends as much on *use* that is made of it in the production of other basic commodities as on the extent to which those commodities enter into its production’. Here it is clearly stated that the price of a basic commodity *depends* on the internal structure of the system of production, i.e., how much of it is *used* by the system and how much of other commodities it *uses* in its production—the ‘profit’ elements of the left hand side of the equations are completely left out of the description. Hence the demand emanating from the use of the surplus output is completely left out of the consideration. This is what ‘the condition of production’ represents and it is claimed that the prices determined on the basis of a *uniform* rate of profits are the prices that satisfy the ‘condition of production’. In other words, these prices can be derived from the objective data of the method of production alone without bringing any additional information, as was the case with the ‘subsistence system’. The parenthetical remark is a clue to warn the reader that how the profits are disposed of has no impact on prices and that is why it would be misleading to think of his solution of prices as an equilibrium of demand and supply.

It is immediately after this para that Sraffa goes on to add the para under consideration:

A less one-sided description than cost of production seems therefore required. Such classical terms as ‘necessary price’, ‘natural price’ or ‘price of production’ would meet the case, but value and price have been preferred as being shorter and in the present context (which contains no reference to market prices) no more ambiguous. (p. 9).

Now it is clearly inconceivable that after giving the clue that his prices were derived directly from only objective data of inputs and outputs and that they should not be interpreted as an equilibrium solution of demands and supplies, Sraffa would go on to suggest that his prices crucially rest on the additional

information contained in the data on effectual demands. It appears that Sraffa interprets such classical terms as ‘necessary price’, ‘natural price’ or ‘price of production’ differently than ‘long-period equilibrium prices’. A clue to this could be found in an early brief note written sometime in Winter 1928 soon after he had written down his subsistence economy and surplus equations, which he refers to in the ‘Preface’ as ‘the opening propositions of this paper’:

When A. Smith etc. said ‘natural’ he did not in the least mean the ‘normal’ or the ‘average’ nor the ‘long run’ value. He meant that physical, truly natural relations between commodities, that is determined by the equations, and that is not disturbed by the process of securing a greater share in the product. (Sraffa ND, D3/12/11: 83).

Clearly, he maintains that ‘natural prices’ of Classical economics were determined by physical data of production alone and were not affected by the competitive process—the exact point he is trying to make for his prices, as I have explained above. The interested readers are encouraged to consult Sinha (2016) in which I have documented a detailed story based on archival evidence of how Sraffa struggled over the years to ‘justify’ the condition of uniform rate of profits in his equations without bringing any outside information from the side of demand.

Aspromourgos (2012), however, further argues:

But quite apart from the status of the uniform rate of profit in Sraffa’s equations, the uniformity of the prices of distinct commodities, and of the wages of homogeneous labour, which are features as well of Sraffa’s equations, also requires justification. The justification, at core, is the same as that for the uniform profit rate: the operation of competition causing the elimination of abnormal returns, for given magnitudes of the parameters governing ‘fundamental’ (Quesnay), or ‘natural’ (Smith, Ricardo), or ‘production’ (Marx), or ‘normal’ (Garegnani), or ‘equilibrium’ prices. Sraffa’s recourse to these uniformities as much implies recourse to ‘subjective’ motivations ‘to maximise’ (308n), as does the uniform net rate of profit. (Aspromourgos 2012, p. 493).

Kurz (2012) also makes this point (see Sinha 2013 for a specific rebuttal of Kurz 2012). There is clearly a confusion here between the notion of ‘arbitrage’ and ‘competitive mechanism’ that brings unequal rates of profits to uniformity. If Kurz and Aspromourgos were right then one should expect a spectrum of ‘market prices’ for one commodity to exist while the rate of profits are not uniform—but no Classical economist assumes that. Market for each commodity clears at *one* ‘market price’, they simply happen to be either above or below the ‘natural price’ thus giving rise to a spectrum of rates of profits in different industries. A market by definition assumes one price for homogeneous goods. An existence of a possibility of arbitrage by definition means existence of more than one market such that

someone can take advantage of buying in 'low price market' and selling in a 'high price market'. That is why in most of the general price theories under Monopoly, Duopoly or Oligopoly, which are by definition non-competitive markets, the rule of one price for one homogenous good is assumed. In one market, where by definition there is no space, time and information constraint for a buyer, no reason exists for the buyer to buy from a high price seller when a lower price is available.

Now, let us take the case of 'uniform wage' for one unit of labour. Sraffa writes:

We suppose labour to be uniform in quality or, what amounts to the same thing, we assume any differences in quality to have been previously reduced to equivalent differences in quantity so that each unit of labour receives the same wage. (Sraffa 1960, p. 10).

So what Sraffa does is to take empirical wage differentials as data and uses those differentials as a multiplication factor (taking one given wage rate as unit) to reduce quality into quantity. After which he normalizes the total homogenous labour units to 1 and assigns every industry its aliquot proportion of total labour used in the system, which is equal to their proportion of wages in total wage bill in the system. It is a simple mathematical exercise conducted on given data and there is no implicit 'competitive mechanism' working behind it.

Point (ii): 'Commodity Residue' has no Theoretical Significance

Angelo Reati's largely sympathetic review (2012) criticized me on a couple of technical points. He was kind enough to share the draft with me before the publication. I felt that he had made a couple of logical errors (at least in understanding my points) and with the kind consent of the editor, I pointed them out in the same issue (Sinha 2012b). To which Reati (2014) reacted rather harshly with doubling down on those points.ⁱⁱ I present his second point first:

Another point of disagreement arose from Sinha's contention that "it is no longer possible to hold that *only labour is productive*. . . . What is productive is *the system of production as a whole*" (Sinha 2010: 187; emphasis in the original). In my review I presented, as a counterargument, a simple parable to show that "Labour alone can make all the capital goods [while] Capital goods alone can make nothing" (Pasinetti 1981: 199-200). Then I added that Sraffa's logical exercise of reduction of prices to dated quantities of labor does not undermine this conclusion because the "commodity residue" that in any case exists could eventually be reduced to the products of nature that could be exploited directly by human activity.

In response Sinha (2012: 401) writes: "What Reati forgets is that when [the] commodity residue becomes negligible then the labor content going along with the residual commodity also becomes negligible, and if and when [the] commodity residue becomes zero then the labor

content *must* also become zero along with it.” This astonishing assertion must lead us to wonder whether Sinha has understood Sraffa’s discussion of the reduction of prices to dated quantities of labor. If Sinha had considered the reduction equation found near the top of page 35 of Sraffa (1960), he would have seen that the labor content is not determined solely by the term referring to the most remote period of time but by an entire series of quantities of labor, weighted by the rate of profit. Thus, contrary to what is argued by Sinha, labor remains the sole true factor of production. (Reati 2014, p. 404).

The logical error I had pointed out in my response is contained in Reati’s statement: ‘Then I added that Sraffa’s logical exercise of reduction of prices to dated quantities of labor does not undermine this conclusion because the “commodity residue” that in any case exists could eventually be reduced to the products of nature that could be exploited directly by human activity’. This expresses a profound misunderstanding of Sraffa’s theoretical project. First of all, the statement that ‘commodity residue’ could be ‘eventually reduced to products of nature’ is a logical impossibility—as Sraffa had immediately realized once he had written his physical equations. He had realized that once a produced ‘commodity’ enters as input in the production equation of any industry, i.e., a produced input that is acquired by an industry through exchange from some other industry, then the reduction process will never reach a stage where all commodities will disappear. In other words, the reduction chain becomes infinite—the path to the primordial state that Reati and Pasinetti are so fond of is blocked forever! One immediate consequence of this revelation was that the attempts by Jevons, Böhm-Bawerk and Wicksell to measure capital by ‘roundabout way of production’ or ‘period of production’ were flawed—since there always will exist some ‘commodity residue’, there exists no absolute measure of the ‘period of production’ and therefore one will have to be satisfied with reducing the commodity residue to an extent that it could be ignored. However, at what stage in the reduction chain the ‘commodity residue’ becomes negligible depends on the wage rate—if wage is high then commodity residue will become negligible at an earlier stage than when wage is low and when wage is zero then ‘commodity residue’ will never become negligible. This gave Sraffa an insight that a measure of capital independent of wages or the rate of profits is not possible: ‘The length of the period of production is not a purely physical (objective) fact, which can be measured by a clock; and which is independent from the way in which, after it is completed, the product is going to be divided between workers and capitalists’ (Sraffa ND, D3/12/7: 90, dated 8.7.28, quoted in Sinha 2016, p. 113). It also gave Sraffa the insight that the relationship between wages and the rate of profits turns out to be radically different—if the ‘commodity residue’ could be reduced to zero then all the capital could be reduced to a long chain of advanced wages and in this case if wages are reduced to zero then the rate of profits must rise to infinity. However, Sraffa reckoned that since there must be a positive ‘commodity residue’, when

wages are reduced to zero then the rate of profits must reach a finite maximum and not infinity. This is the foundation of the notion of ‘maximum rate of profits’ in Sraffa’s system. Then came the problem of proving that the ‘maximum rate of profits’ is a physical property of the production system and therefore independent of wages and the rate of profits. This led Sraffa to search for the Standard system and the Standard commodity associated with any given empirical system to prove that the ‘maximum rate of profits’ is a physical property of the production system. On the basis of which he could derive his main equation $r = R(1 - w)$, where R , the maximum rate of profits (or the net output-capital ratio), is constant with respect to changes in r (the rate of profits) and w (the wage rate). Only if R could be proven to be constant with respect to changes in r or w that it could be argued that if w is given from outside the equation system then r is also determined independently of prices, i.e., both the distributional variables are determined independently of prices as was the case with Marx. Prices can then be determined by plugging in the value of r in the equations, which turns it into a system of linear equations—just like the equations for the subsistence economic system. The idea of ‘commodity residue’ was so central to Sraffa’s theoretical project that he titled his book emphasizing this very point—*Production of Commodities by Means of Commodities* highlights the circular nature of the problem. Once commodities enter the production equations then there is no way of getting rid of them—there is no primordial or ‘originary’ anchor to which productive activity could be reduced to, there is no such thing as, what Reati calls, ‘sole true factor of production’.ⁱⁱⁱ All this is now carefully documented in Sinha 2016. Here I present just one excerpt from Sraffa’s unpublished notes that conveys the message:

The error of Jevons-BB (omitting Commodity residue term) has much more far reaching consequences than the trifling ones that are made to appear above.

For in the above, the relation of w and r in the Reduction equation is still as determined in the original equation. But since the original equation included (implicitly) the Residue Term, the latter has not been effectively eliminated.

But J-BB start from a finite series similar to the Reduction series, and know nothing of an original equation. Therefore they have no “given” relation between \underline{w} and \underline{r} : they must deduce this from their finite series of pure labour terms, But the relation thus deduced must be very different for the one obtained from the original equation – in particular: a) there can be no maximum for \underline{r} , and b) \underline{r} throughout its movement (as w falls) cannot behave as if it approached a maximum.

Therefore the effects of omitting Residue term are not shown only for values of r very near the maximum (as is implied in the previous pages) but throughout. (Sraffa ND, D3/12/26: 13; dated 29.11.42, quoted in Sinha 2016, p. 116).

Point (iii): Uniform Rate of Profits is not a Structural Property of the System

The second point Reati makes is:

In his book Sinha maintains that for Sraffa the uniform rate of profit is not the result of a gravitation process of the different industries' rates of profit resulting from competition among capitals but, rather, that it is "a logical necessity of any . . . system of production that determines prices internally, irrespective of the equilibrium of demand and supply" (Sinha 2010: 289). He substantiates this argument in a confusing chapter "On the Uniform Rate of Profits" where, instead of referring to the actual rate of profits – *i.e.* the rate of profits that results when the wage rate is fixed at some positive level – he bases his reasoning on the *maximum* rate of profit, that is the rate of profit that could be theoretically charged on costs when the wage rate is zero (workers live on air!). Then Sinha develops a lengthy argument to show that this maximum rate of profits should be the same in the real and the standard system (R for the real system and R^* for the standard system). From this he deduces that the maximum rates of profits (which *are not* the actual rate of profits) of the different industries must be uniform, otherwise the equality $R = R^*$ would not hold. Thus, one cannot invoke the gravitation process for the uniformity of the actual rates of profits. Thus, Sinha contends, the gravitation process is not what underpins Sraffa's uniform rate of profits assumption: the sectoral rates must be equal as a matter of logical necessity. As we shall see in a moment, Sinha's reasoning is hopelessly confused.

To support his argument that Sraffa did not rely upon any gravitation mechanism, Sinha (2010: 293) misrepresents a passage from Sraffa's manuscripts, dated 1955, in which Sraffa notes that "With changes in w [the wage rate] The impulse towards price change is an internal one to each industry . . . not from those conditions *compared* with those of other industries." Sinha does not discuss the context in which Sraffa made this remark, but its meaning is quite obvious, and not at all what Sinha takes it to be: Sraffa is saying that if w changes, all prices will change simply because production costs will change *within each sector*. Sraffa's observation has got nothing to do with gravitation, and therefore has no bearing on Sinha's argument. (Reati 2014, pp. 402-3).

If Reati's argument were to be true then a rise in wages should change all the prices even when the direct to indirect labour ratios (or the organic composition of capital) of all the industries were equal too, since 'production costs *within each sector*' would still change. But it is well known that in this case wages would move from zero to its maximum value without affecting the prices—it only

affects the rate of profits and since its effect on profits is proportional to their capital investments it has no effect on the prices.

Reati, however, further goes on to argue:

This confusion between the maximum rate of profit and the actual rate of profits is reiterated in Sinha (2012) when he states that “R is the weighted average of the industrial rates of profits,” which would imply that, to ensure that $R = R^*$, the industrial rates of profits must be equal. ...

Comparing the actual and the maximum rates of profits we see that they are different in nature. While the actual rate of profits reflects a market mechanism, the maximum rate of profits is the value which guarantees the *compatibility* of the different industries at the level of the economic system. More precisely, R is univocally associated with a positive vector of prices (see below). This also means that, taking into consideration the complex network of industrial relations, no industry could *theoretically* charge a price on which the rate of profits is higher than R.

Concerning the equality of the maximum rate of profit of the real system (R) with the maximum rate of profits of the standard system (R^*), in my review I noted that this is simply due to the fact that both depend on the maximum eigenvalue of the input matrix, which is the same for both systems. It is perhaps worth recalling the details here (see Pasinetti 1977: 76-78). If we assume the wage rate is zero, the price system may be written as:

$$\mathbf{p} = \mathbf{pA}(1 + R) \quad (1)$$

where \mathbf{p} is the vector of prices and \mathbf{A} the irreducible non-negative input matrix (I assume for the sake of simplicity that all capital is circulating capital). Equation (1) can be written

$$\mathbf{p}[\mathbf{I} - (1 + R)\mathbf{A}] = \mathbf{o} \quad (2)$$

Writing

$$1/(1 + R) = \lambda \quad (3)$$

equation (2) becomes

$$\mathbf{p}(\lambda\mathbf{I} - \mathbf{A}) = \mathbf{o} \quad (4)$$

This homogeneous system has solutions different from zero if the determinant of the expression within the parentheses is zero, and the roots of the characteristic equation

$$\det(\lambda\mathbf{I} - \mathbf{A}) = 0$$

are the eigenvalues of matrix \mathbf{A} .

From the Perron-Frobenius theorems we know that only one eigenvalue – the maximum one λ_m – is certainly associated with all prices being positive. Thus, making use of equation (3), we can see that the highest profit rate compatible with the viability of the system is:

$$R = (1/\lambda_m) - 1$$

The relation between R and the actual rates of profits is clear: R represents the upper limit for the rates of profits of the individual industries (r_i). Thus, Sinha (2012) is quite wrong, and thoroughly confused, when he asserts that R is a weighted average of the sectoral maximum profit rates. Also, R cannot be identified with the average of the surpluses of the various industries. (Reati 2014, pp. 403-4).

So let us take a simple example^{iv}:

$$x \rightarrow 2y$$

$$\frac{1}{2}y \rightarrow x$$

We can write the price equations for this economy as:

System 1

$$p_x(1 + r_1) = 2p_y \quad (i)$$

$$\frac{1}{2}p_y(1 + r_2) = p_x \quad (ii),$$

where p 's and r 's are industrial prices and rates of profits. We take x as the *numéraire*. Therefore, $p_x = 1$. If we assume $r_1 = r_2 = R$, then we derive $R = 1$ and $p_y = 1$. Now suppose the so-called 'market price' of y happens to be, $p_y = 3/2$. This gives us $r_1 = 2$ and $r_2 = 1/3$. Since $r_1 = 2$ is greater than $R = 1$ and p_x , p_y and r_2 are positive, we have proved that, from Reati's own perspective, his statement, 'no industry could *theoretically* charge a price on which the rate of profits is higher than R ', is false.

The question of positive or zero wages is irrelevant to the issue. If one lives in the 'market-prices-natural prices' world, which is Reati's world, then in a disequilibrium situation 'market-prices' (or $(n - 1)$ rates of profits) could be arbitrarily taken from outside and n remaining unknowns can be solved, as we have done in our example. This would generate an average ' R ' and some r 's must be higher than ' R ' by definition, if all r 's are not equal. My argument, however, denies this *crucial assumption* that under disequilibrium condition one can take sufficient numbers of 'market prices' or rates of profits arbitrarily to solve the system of basic equations as I now explain below.

Let us multiply the first equation of system 1 with $\frac{1}{2}$, we get our two equations as:

System 2

$$\frac{1}{2}p_x(1 + r_1) = p_y \quad (i)$$

$$\frac{1}{2}p_y(1 + r_2) = p_x \quad (ii)$$

$$(\frac{1}{2}p_x + \frac{1}{2}p_y)(1 + R^*) = p_x + p_y$$

This is our Standard system. The aggregate of our equations (i) and (ii) of System 2 gives us the average rate of profits of the system $R^* = 1$, independently of our knowledge of p_x and p_y . Sraffa (1960) has proved that there is one and only one Standard system that is associated with any given system such

as system-1. Sraffa's argument turns out to be that *the properties of the whole of the system determine the properties of its parts*—just as in quantum theory in physics.

So let us go back to our original equations (i) and (ii) of system 1 with the additional aggregate equation:

System 3

$$p_x(1 + r_1) = 2p_y \quad (i)$$

$$\frac{1}{2} p_y(1 + r_2) = p_x \quad (ii)$$

$$(p_x + \frac{1}{2} p_y) (1 + R) = p_x + 2p_y$$

Here R is the average rate of profits of the whole system, which is an unknown. If $r_1 = r_2$ then $r_1 = r_2 = R$. If r_1 is not equal to r_2 then they must be either above or below R and if one is above then the other must be below. Let us hypothesize that r_1 is not equal to r_2 and they deviate from R by d_1 and d_2 . In other words, $r_1 = R + d_1$ and $r_2 = R + d_2$. Without loss of generality, we assume $d_1 < 0$ and $d_2 > 0$. Now we can write our equation system as:

System 4

$$p_x(1 + R + d_1) = 2p_y \quad (i)$$

$$\frac{1}{2} p_y(1 + R + d_2) = p_x \quad (ii)$$

$$(p_x + \frac{1}{2} p_y) (1 + R) = p_x + 2p_y$$

By definition $(p_x d_1 + \frac{1}{2} p_y d_2) = 0$. Now we multiply equation (i) by $\frac{1}{2}$ and transform the system to:

System 5

$$\frac{1}{2} p_x(1 + R + d_1) = p_y \quad (i)$$

$$\frac{1}{2} p_y(1 + R + d_2) = p_x \quad (ii)$$

$$(\frac{1}{2} p_x + \frac{1}{2} p_y) (1 + R') = p_x + p_y$$

Here R' is the unknown average rate of profits and could be different from R as we have rescaled equation (i). Given that we have assumed $d_1 < 0$ and shrunk the size of the 1-industry by half while leaving the 2-industry undisturbed, we should expect the average rate of profits of the system to rise, i.e., we should expect $R' > R$. However, by inspection we can see that $R' = 1$, independently of p_x and p_y , as it is the Standard system. Thus $R' = R^*$ and we have already established that $R = R^* = 1$ (in our example). Therefore, $R'=R$. Now, this could happen only if $(\frac{1}{2} p_x d_1 + \frac{1}{2} p_y d_2) = 0$ —condition 1. But we have already (from System 4) established that, by definition, $(p_x d_1 + \frac{1}{2} p_y d_2) = 0$ —condition 2. Since both p_x and p_y are positive, conditions 1 and 2 would simultaneously hold if and only if $d_1 = d_2 = 0$, QED. The crucial point to note in this argument is that condition 1 could be derived only because System 5 happens to be a Standard system and so the value of R' can be derived independently of the knowledge of prices. It was Sraffa's discovery of the Standard system and his proof of its uniqueness that provide the crucial argument in the proof of the proposition that the 'rate of profits *must* be uniform'.

Now, a positive wage does not change the terms of the problem. Take System 2 and add 1 unit of labour to the whole system with wages equal to $(\frac{1}{4}x + \frac{1}{4}y)$ per unit of labour, the half of the Standard net output. This will change R^* to $\frac{1}{2}$, independently of what prices prevail. Rescale this system back to System 1 with total labour used in the whole system equal to 1 unit. Apply the wages equal to $(\frac{1}{4}p_x + \frac{1}{4}p_y)$ and change the *numéraire* equation from $p_x = 1$ to $(\frac{1}{2}p_x + \frac{1}{2}p_y) = 1$. Apply $r_1 = r_2 = R$. We find $R = \frac{1}{2}$ in this case as well. This is why the Standard commodity as the *numéraire* and wages measured in terms of the Standard commodity become crucial in linking the properties of the Standard system, which shows the physical nature of these properties, to the empirical system. From here on, the argument goes one-one as above.

So what have we done here that is different from Reati's understanding? Following the traditional Sraffian mathematical tradition (hence his reference to Pasinetti), Reati takes the average of n -independent equations as the 'statistical average'. Once you assume that empirical prices are 'market prices', i.e., determined by demand and supply conditions in the market and independently of Sraffa's system of equations, then the statistical average rate of profits need not be constrained by Sraffa's equations. That is what I did when I arbitrarily chose $p_y = 3/2$ in system 1 to show that r_1 could theoretically go above R and the statistical average rate of profits 'R' ($= 9/7$) would be different from R , if either the rates of profits or prices are taken arbitrarily. This is why it is important to claim that *all* prices must be determined by the system of equations, and Sraffa showed that there is sufficient information in his equation system of basic goods to do so. Sraffa from early on works with a notion of 'algebraic average'. For him, it is by definition that an average can be distributed equally over the population. That is why, by assuming equal rate of profits in his equations he was simply deriving the average, i.e., the 'algebraic average' rate of profits of his equation system. For example, if we put our 'statistical average' rate of profits ' $R = 9/7 = r_1 = r_2$ ' in our System 1 then the equation system becomes contradictory. During the Winter 1927-28 while writing his 2nd equation, that is the equation of a system producing surplus, Sraffa put it this way: 'But since we have (in the no-surplus system) a spare equation, we can use it to determine, simultaneously with value, *the ratio that total surplus bears to total initial stocks of the community.*' (Sraffa ND, D3/12/6: 17, emphasis added, quoted in Sinha 2016, p. 57). Then he goes on to put an equal rate of profits for all industries to solve for it. Clearly, the solution for an equal rate of profits for him was designed to solve for the ratio of the aggregate equation, which is the average rate of profits of the system. And in 1942-43, when he was struggling to work out the Standard system he explicitly wrote: 'Yet the aggregate is *not a statistical* result but an algebraic one' (D3/12/36: 79, emphasis in original, quoted in Sinha 2016, p. 134). Above we have alluded to quantum physics in relation to the properties of the whole determining properties of the parts, we could also invoke another analogy from physics to bring our point to sharp relief: in Newtonian world space and time are separate but Einstein showed that that was incorrect, one cannot separate space

and time—the universe is a fabric of space-time. Similarly for Sraffa industrial equations cannot be separated from the aggregate equation of the system—they together constitute the whole.

Part 2: *A Revolution in Economic Theory: The Economics of Piero Sraffa*

Recently I have come across two rather negative reviews from Sraffian quarters of my recent book, *A Revolution in Economic Theory: The Economics of Piero Sraffa* (Sinha 2016). Below I present my response to them.

Professor Alessandro Roncaglia in his review (Roncaglia 2017) mostly talks about his own work rather than my book. As far as my book is concerned, the review devotes disproportionately large space to one footnote, which refers to his two earlier works. In that footnote, I state:

Among the Sraffians, Roncaglia (1978, p. 16) did appreciate that ‘there is no reason to believe that Sraffa’s prices of production should equate quantity demanded and quantity supplied’. This position was based on his claim that Sraffa’s system was like ‘a photograph of the market place’. ... Roncaglia, however, did not manage to show how then Sraffa could take the rate of profits to be uniform and thus succumbed to holding the contradictory position that Sraffa’s system was ‘a photograph of the market place’ and at the same time *assumed* to be at the center of gravitation; as he writes: ‘In Sraffa’s analysis, as in that of the classical economists and Marx, the analytic condition upon which determination of the prices of production (the ‘natural’ prices of the classical economists) rests consists quite simply in an equal rate of profits in the various sectors. This assumption corresponds to the idea pondered by Smith and Marx among others, that the unity of the capitalist system is guaranteed by the free flow of capital from one sector to another in pursuit of the most advantageous utilization.’ (Roncaglia 2000, pp. 34-35).

Professor Roncaglia complains that this misinterprets his position, but instead of showing how my interpretation of his quotation from Roncaglia (2000) was wrong he simply asserts that: ‘In a long footnote (p. 215) Sinha incredibly attributes to me Garegnani’s views ...On the opposition between Garegnani’s interpretation and mine there is much in the literature, some of which is even quoted in Sinha’s book’ (Roncaglia, 2017, p. 143). But the quotation in my book was to make the point that Roncaglia, inadvertently, falls back on Garegnani’s interpretation. The point is how is ‘the free flow of capital from one sector to another in pursuit of the most advantageous utilization’ any different from Garegnani’s gravitation mechanism that brings the rate of profits in the system to equality?

A similar argument is made in relation to my claim that classical centre of gravitation requires the constant returns assumption:

Another, more important, point concerns the interpretation of Classical theory. According to Sinha (p. 15), ‘Smith must have implicitly assumed constant returns to scale for all his industries’ in the context of his analysis of the gravitation process. This corresponds to the traditional neoclassical reading of the history of economic thought, superimposing on Classical authors the later supply-and-demand equilibrium notion. However (Roncaglia, 2005, pp. 139–145), gravitation is for Smith nothing but a metaphor used to evoke the role of competition as a force making for the stabilization of the market; ‘demand’ and ‘supply’ did not indicate stable and well-identified functional relations connecting price and quantity of a commodity; indeed, Smith contrasted his analysis of natural prices to previous analyses which emphasized the role of scarcity and demand in price determination; for Smith the market price was not to be seen as a theoretical variable, a role reserved to natural prices (it was Marshall who, in order to combine classical and marginalist analysis, established the habit of considering both natural and market prices as theoretical variables).’ (Roncaglia 2017, p. 142)

But after all said and done, the question remains: where does the ‘force of competition’ come from? What generates the ‘force’? Let me remind Professor Roncaglia that Sraffa from very early on, i.e., 1927–31, was clear that he was ‘not assuming any forces’ (Sraffa ND, D3/12/7: 65).

However, yet again the same argument reappears:

Throughout his book, Sinha tries to demonstrate that Sraffa’s assumption of a uniform rate of profits is a logical necessity, a point to which I shall return below. In this way, he leaves aside the most natural interpretation of the assumption, that it is part of the adoption of the Classical (‘circular flow’) framework in his analysis, hence the Classical notion of the ‘competition of capitals’ based on the free entry of new capital into any line of production. (Roncaglia 2017, p. 143).

Professor Roncaglia has obviously not paid sufficient attention to the third chapter, titled ‘A New Beginning’, of my book; as it documents in details how Sraffa was unable to accept what Roncaglia calls ‘natural interpretation’ for the equal rate of profits postulate. It is simply because Roncaglia’s ‘natural interpretation’ entails the notion of ‘inducement’, i.e., a psychological variable, that Sraffa could not allow in his system.

In the end, his final and the main criticism turns out to be:

In any case, as already indicated, Sinha’s argument (see for instance p. 203) relies on the assumption that the wage is measured in terms of the standard commodity. (For a critique of

this assumption, or better of its unacceptability as a general case, see [Roncaglia, 1978](#), pp. 67–83.) (Roncaglia 2017, p. 144).

Actually I have devoted one full section of chapter 7 titled ‘What is *given* w or r ?’. In this section I have argued that in the real world wages in terms of the Standard commodity cannot be taken as given because ‘money’ must be a means of deferred payment and the Standard commodity almost always must change from period to period. Thus Sraffa had to move from taking wages as given from outside to the rate of profits as given from outside from page 33 onwards of his book. The point of taking wages as given from outside (in terms of the Standard commodity) was to establish a theoretical proposition; i.e., to show that the three structural variables of the system, viz., the productivity of the system R , and the two distributional variables w and the average r of the system are related in a relationship given by $r = R(1 - w)$, where R is constant with respect to changes in w and r . This relationship is independent of prices, which ensures that industrial r ’s in the real system must be uniform. Once this theoretical proposition is established, one could move to taking r as given from outside and use the relationship $r = R(1 - w)$ as the *numeraire* equation to solve for prices and wages. The resulting wages, whatever they turn out to be, must be *ipso facto* in terms of the Standard commodity for that system. The importance of the Standard commodity in all this is that if we use any arbitrary commodity or even the value of the net output of the system as the numeraire then with changes in wages or the rate of profits prices will change in such a way that the value of R (i.e., the ratio of net output to means of production) will keep changing as well. Thus it cannot be established that once either w or r is given then the other distributional variable r or w gets determined independently of prices as well. This is because the arbitrary measuring scale itself gets affected by the changes in wages and the rate of profits. The Standard commodity provides us with a scientific scale by which we can see the real relationship that exists between these variables and why they are independent of prices.

Now I turn to David Andrews’s review in *History of Economic Ideas*. Leaving aside the fact that I completely disagree with Andrews’ (2017) reading of my book, here I would only like to point out that his main criticism turns out to be rather bizarre! He argues that since I think that the Classical theory of centre of gravitation assumes constant returns and is weak, I want to distance Sraffa’s theory from it. This leads him to argue:

Rather than allow Sraffa’s analysis to be tied to a problematic theory, Sinha attempts to distance him from the classical theory of natural prices. However, it is not only Garegnani who links Sraffa to the classical theory—Sraffa makes the same link, as Sinha acknowledges: “the classical notion of ‘natural price’ relates to, *at least in Sraffa’s interpretation*, the solution of his equation system” (181; emphasis added). This creates an extraordinary situation in which Sinha’s interpretation of Sraffa involves arguing against Sraffa himself. After emphasizing his

remarkable intellect, Sinha implies that Sraffa has profoundly misunderstood his own work in connection with a topic on which Sraffa's authority is widely recognized. (Andrews 2017, p. 219)

This I find absolutely unbelievable! Actually, the phrase quoted from my book appears on page 191 and not 181 as cited by Andrews. The full sentence reads: 'As we have seen, the Classical notion of 'natural price' relates to, at least in Sraffa's interpretation, the solution of his equation system'. By removing the all-important reference to 'As we have seen', i.e., the preceding discussion, Andrews gives the impression to the reader that I go along with the idea that Sraffa's interpretation of Classical 'natural price' and Garegnani's are of course the same and I'm claiming that Sraffa did not understand his own work! There is no attempt on the part of Andrews to understand what 'we have seen' in the previous pages stands for. Actually, I go on to cite the quotation from Sraffa's unpublished notes on Adam Smith's 'natural prices',

When A. Smith etc. said 'natural' he did not in the least mean the 'normal' or the 'average' nor the 'long run' value. He meant that physical, truly natural relations between commodities, that is determined by the equations, and that is not disturbed by the process of securing a greater share in the product. (Sraffa ND, D3/12/11: 83),

to reemphasize Sraffa's interpretation of Classical 'natural' prices as purely physical one. I also point out Sraffa's parenthetical note: '(which contains no reference to market prices)' as a reference to the absence of the idea of gravitation in his theory, as 'gravitation' must refer to 'market prices'. None of this mattered to Andrews, however! Now the fact of the matter is that I argue that Classical notion of the 'centre of gravitation' assumes constant returns to scale and no substitution possibilities in production equations. Given these two assumptions, the Classical solution to the centre of gravitation will be identical to Sraffa's prices, which, I repeat, do not require those assumptions.

References

- Andrews, David. 2017. 'Review of *A Revolution in Economic Theory: The Economics of Piero Sraffa*', *History of Economic Ideas*, XXV(2): 219-21.
- Arrow, K.J. and G. Debreu. 1954. 'Existence of an Equilibrium for a Competitive Economy', *Econometrica*, 22(3), pp. 265-290.
- Aspromourgos, Tony. 2012. Review of 'Ajit Sinha, *Theories of Value from Adam Smith to Piero Sraffa*', *European Journal of the History of Economic Thought*, 19(3): 490-95.
- Fratini, S.M. 2012. Review of 'Ajit Sinha, *Theories of Value from Adam Smith to Piero Sraffa*', *Contributions to Political Economy*, 31: 102-5.
- Garegnani, P. 1976. 'On a change in the Notion of Equilibrium in Recent Work on Value and Distribution: A Comment on Samuelson', in Murray Brown, Kazuo Sato and Paul Zarembka (eds.), *Essays in Modern Capital Theory*, Amsterdam: North Holland, pp. 25-45.
- Garegnani, P. 2007. 'Professor Samuelson on Sraffa and the Classical economics', *European Journal of the History of Economic Thought*, vol. 14(2), pp. 181-242.

- Garegnani, P. 2012. 'On the present state of the capital controversy', *Cambridge Journal of Economics*, 36(6), pp. 1417-32.
- Hicks, J.R. 1939. *Value and Capital*, Oxford: Clarendon Press.
- Kurz, H.D. 2012. 'Don't treat too ill my Piero! Interpreting Sraffa's papers', *Cambridge Journal of Economics*, 36(6), 1535-1569.
- Levrero, E.S. 2012. 'Review of *Theories of Value from Adam Smith to Piero Sraffa*', *Review of Political Economy*, 24(3): 527-35.
- Reati, Angelo. 2012. Review of *Theories of value from Adam Smith to Piero Sraffa*. *Review of Radical Political Economics* 44 (3) (Summer): 395-400.
- Reati, Angelo. 2014. 'A Note on Some Misunderstanding of Sraffa's System', *Review of Radical Political Economics*, 46(3), pp. 402-5.
- Roncaglia, A. 1978. *Sraffa and the Theory of Prices*, Chichester: John Wiley & Sons.
- Roncaglia, A. 2000. *Piero Sraffa His Life, thought and cultural heritage*, London: Routledge.
- Roncaglia, A. 2017. 'A. Sinha: A Revolution in Economic Theory: The Economics of Piero Sraffa', *Contribution to Political Economy*, 36(1), pp. 140-44.
- Sinha, Ajit. 2010. *Theories of Value from Adam Smith to Piero Sraffa*, London: Routledge.
- Sinha, Ajit. 2012a. 'Listen to Sraffa's Silences: A New Interpretation of *Production of Commodities*', *Cambridge Journal of Economics*, 36(6), 2012, pp. 1323-1339.
- Sinha, Ajit. 2012b. 'A Response to Angelo Reati', *Review of Radical Political Economics*, 44(3), pp. 401-402.
- Sinha, Ajit. 2013. 'The New Interpretation of Sraffa's Prices: A Response to Heinz Kurz', *Cambridge Journal of Economics*, 37(6), pp. 1449-1453.
- Sinha, Ajit. 2015. 'A Reflection on the Samuelson-Garegnani Debate', *Economic Thought*, 4(2), pp. 48-67.
- Sinha, Ajit. 2016. *A Revolution in Economic Theory: The Economics of Piero Sraffa*, Cham: Palgrave Macmillan.
- Sinha, Ajit. 2018. 'Afterword' in *Theories of Value from Adam Smith to Piero Sraffa*, 2nd edition, London: Routledge.
- Sraffa, P. [1925] 1998. 'On the Relation between Costs and Quantity Produced', in L. Pasinetti (ed.), *Italian Economic Papers*, translated from Italian, Volume III: Il Mulino and Oxford: Oxford University Press.
- Sraffa, P. (1960). *Production of Commodities by Means of Commodities*, Cambridge, Cambridge University Press.
- Sraffa, P., ND. *Piero Sraffa Papers*. Wren Library, Trinity College, Cambridge.

ⁱ I leave aside the question of the 'correctness' of Garegnani's interpretation of classical gravitation mechanism and the question of to what extent Sraffa would have ever accepted it.

ⁱⁱ Sadly Angelo died in a bicycle accident soon after writing those comments—he was a good friend. I mourn his death and regret that I did not get a chance to change his mind.

ⁱⁱⁱ In his book, Sraffa gives credit to Marx for bringing this theoretical point to fore: '... but more generally owing to his [Marx's] emphatic rejection of the claim of Adam Smith and of others after him that the price of every commodity 'either immediately or ultimately' resolves itself entirely (that is to say, without leaving any commodity residue) into wages, profits and rent—a claim which necessarily presupposed the existence of 'ultimate' commodities produced by pure labour without means of production except land, and which therefore was incompatible with a fixed limit to the rise in the rate of profits.' (Sraffa 1960, Appendix D, p. 94).

^{iv} I am indebted to Professor Bertram Schefold for coming up with this simple example in an e-mail exchange.