

Sraffa and Wittgenstein: Physicalism and Constructivism

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ABSTRACT *After a brief review of facts and hypotheses concerning Piero Sraffa's intellectual exchanges with the philosopher Ludwig Wittgenstein and their content, a brief presentation of some of the basic ideas of *Productions of Commodities by Means of Commodities* is given, on the basis of which I argue, first, that Sraffa's 'objectivism' in economics is closely related to the 'physicalism' towards which Wittgenstein moved soon after his return to Cambridge and, secondly, that the mathematics of that book are in line with Wittgenstein's constructivist stance, as it is already found in his *Tractatus Logico-Philosophicus*.*

1.

Is it already mathematical alchemy, that mathematical propositions are regarded as statements about mathematical objects, – and mathematics as the exploration of these objects? (L. Wittgenstein, 1978, p. 274)

Piero Sraffa is the subject of much curiosity because of his friendship with two other major figures of the 20th century – an unlikely pair – Antonio Gramsci and Ludwig Wittgenstein. Amartya Sen recently speculated about a 'Gramsci connection', with Sraffa as the intermediary, in the evolution of Wittgenstein's thought (Sen, 2003, 2004; see also Albani, 1998; Davis, 1993, 2002). With the exception of a few remarks on Gramsci at the beginning of the next section, I shall be concerned with the connection between Sraffa and Wittgenstein. As no other than Paul Samuelson (1990, p. 264) wrote, 'one yearns to know more about Sraffa's precise influence on Wittgenstein'.

From 1927 onward, Sraffa lived in Cambridge, to which Wittgenstein returned in 1929. Sraffa joined him at Trinity in 1939, so they were colleagues until Wittgenstein's death in 1951.¹ They met early on, in 1929, through John

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¹For biographical information about Sraffa, see Kaldor (1985) and Potier (1991); for Wittgenstein, see Monk (1990).

Maynard Keynes. Cambridge must have been an unusually stimulating milieu. One anecdote has Frank Ramsey, Sraffa, Wittgenstein, and Keynes discussing the latter's *Treatise on Probability* over lunch (Newman, 1987, p. 42). Sraffa and Wittgenstein had opportunity to meet throughout most of the 1930s and 1940s. It appears that they met frequently, at times more than once a week, until May 1946, when Sraffa told Wittgenstein that he no longer wished to have conversations with him. Wittgenstein's biographer, Ray Monk, reported the incident in these terms:

In May 1946 Piero Sraffa decided he no longer wished to have conversations with Wittgenstein, saying that he could no longer give his time and attention to the matters Wittgenstein wished to discuss. This came as a great blow to Wittgenstein. He pleaded with Sraffa to continue their weekly conversations, even if it meant staying away from philosophical subjects. 'I'll talk about anything', he told him. 'Yes', Sraffa replied, 'but in your way'. (Monk, 1990, p. 487)

Sraffa must have been an exceptional conversationalist; Georg Henrik von Wright reported Wittgenstein as saying that 'discussions with Sraffa made him feel like a tree from which all branches had been cut. That this tree could become green again was due to its own vitality' (Malcolm, 1984, pp. 14–15). Discussions with Sraffa had a great impact on Wittgenstein. Regarding mistakes he thought he had made in his *Tractatus Logico-Philosophicus* (hereafter shortened to *Tractatus*) he wrote:

I was helped to realize these mistakes, to a degree which I myself am hardly able to estimate, by the criticism which my ideas encountered from Frank Ramsey, with whom I discussed them in the last two years of his life. Even more than to this always certain and forcible criticism I am indebted to that which a teacher at this University, Mr. P. Sraffa, for many years unceasingly practiced on my thoughts. I am indebted to this stimulus for the most consequential ideas of this book. (Wittgenstein, 1953, p. viii)

There is also an earlier, well-known passage, dating from 1932, where Wittgenstein recognized his debt to Sraffa, as the last of an impressive list:

I don't believe I have ever invented a line of thinking, I have always taken one over from someone else. I have simply straightaway seized on it with enthusiasm for my work of clarification. That is how Boltzmann, Hertz, Schopenhauer, Frege, Russell, Kraus, Loos, Weininger, Spengler, Sraffa have influenced me. (Wittgenstein, 1980, p. 19)

In these passages, Wittgenstein states that he owes 'the most consequential ideas of this book' to discussions with Sraffa and that he took a 'line of thinking' from him. Alas, the precise nature of this 'stimulus' remains subject to speculation. The reason for this is that there are hardly any traces of their conversations.²

²The Sraffa Papers are deposited at the Wren Library, Trinity College, Cambridge. For information about their content, see Kurz (1998). Among these papers, one only finds 'Notes on Wittgenstein's *Blue Book*, correspondence with G. H. von Wright and news cuttings of reviews of books by and on Wittgenstein' (catalogue No. I21). There is also a 'Fragment on Language' (D3/12/70), the content

There is only one recorded instance, an oft-quoted anecdote reported by Norman Malcolm:

Wittgenstein and P. Sraffa [. . .] argued together a great deal over the ideas of the *Tractatus*. One day (they were riding, I think, on a train) when Wittgenstein was insisting that a proposition and that which it describes must have the same 'logical form', the same 'logical multiplicity', Sraffa made a gesture, familiar to Neapolitans as meaning something like disgust or contempt, of brushing the underneath of his chin with an outward sweep of the finger-tips of one hand. And he asked: 'What is the logical form of *that*?' Sraffa's example produced in Wittgenstein the feeling that there was an absurdity in the insistence that a proposition and what it describes must have the same form. This broke the hold on him of the conception that a proposition must literally be a 'picture' of the reality it describes. (Malcolm, 1984, 57–58)

There are also a handful of references to conversations with Sraffa in Wittgenstein's manuscripts, such as this one:³

Are the propositions of mathematics anthropological propositions saying how we men infer and calculate? – Is a statute book a work of anthropology telling how the people of this nation deal with a thief etc.? – Could it be said: 'The judge looks up a book about anthropology and thereupon sentences the thief to a term of imprisonment'? Well, the judge does not USE the statute book as a manual of anthropology. (Discussion with Sraffa) (Wittgenstein, 1978, III, par. 65)⁴

On the basis of evidence such as this, commentators had to use ingenuity to come up with plausible suggestions. Wittgenstein commented to Rush Rhees that the most important thing he gained from his discussions with Sraffa was an 'anthropological' way of looking at things (Monk, 1990, p. 261). This is not self-explanatory but, if by 'anthropological' one understands, as Ray Monk did, looking at language not in isolation from circumstances in which it is used, as Wittgenstein did in his *Tractatus*, but within the context of the activities within

of which bears affinity with ideas from Wittgenstein. As for Wittgenstein, almost all references to Sraffa in his *Nachlaß* are cited here. The only published letter, from Sraffa to Wittgenstein, dated 14 March 1936 (Wittgenstein, 1995, pp. 290–292), interesting as it may be, has also no philosophical content. Further letters from Wittgenstein to Sraffa were recently acquired by Trinity College. I have not been able to see the letters but was informed about their content by Jonathan Smith, archivist at Trinity, and Carlo Panico. The letters throw much light on the personal relations between Wittgenstein and Sraffa but they have almost no philosophical content. An undated letter contains reference to a discussion on what being in a rage looks like, where Sraffa clearly opposed Wittgenstein from a physicalist standpoint, in terms of contraction of facial muscles. This is further evidence in support of the claims in Section 2 below.

³There are a few more remarks, for example Wittgenstein (1996, p. 26), which will not be examined here, for reasons stated below.

⁴For reasons unknown to me, the last three words were deleted for the printed version. According to Wolfgang Kienzler (1997, p. 53), the key word in this passage is 'use', which shows a 'pragmatic tendency' in the later Wittgenstein. For my part, I would point out that remarks such as this one open the door to the conception of mathematics as an 'anthropological phenomenon' that fascinated the later Wittgenstein (see, for example, Wittgenstein, 1978, VII, par. 33). However, the key point of this passage is that 'the judge does not USE the statute book as a manual of anthropology': the use of mathematical theorems is thus conceived by Wittgenstein as akin to jurisdiction.

which it is embedded, then, surely, Sraffa's influence on the later Wittgenstein is 'indeed of the most fundamental importance' (Monk, 1990, p. 261). There is a chronological difficulty already noted by Monk: Wittgenstein had already recognized the influence of Sraffa in 1932, while this 'anthropological' feature did not emerge until later. At all events, philosophers have diversely interpreted this 'anthropological' feature. For example, K. T. Fann (1969, pp. 49–50) claimed that Wittgenstein's 'method of speculative anthropology' – in Fann's words, the 'important method of imagining and constructing simple and complicated "language-games"' – is 'an adaptation of Sraffa's method'.⁵ Brian McGuinness (1982, p. 39) wrote instead of 'the idea of a way of thinking as reflecting the character of a culture'. These suggestive remarks were not, however, accompanied by detailed arguments.⁶

At any rate, it is not appropriate to enter here into the intricate exegesis of Wittgenstein's thought and the *anthropologische Betrachtungsweise*.⁷ In the remainder of this section, I shall briefly review an interesting attempt at fleshing out the influence of Sraffa by John Davis, while in the second and third sections, I shall present new suggestions. It should be noted at the outset that, in his preface to *Philosophical Investigations*, Wittgenstein implies that Sraffa (along with F. P. Ramsey)⁸ helped him to realize the mistakes he had committed in the *Tractatus*. Furthermore, Wittgenstein had already acknowledged (in the passage quoted above) the influence of Sraffa to be fundamental in 1932, that is, at a time when he had only begun to move away from the conceptions of his *Tractatus* and when the key ideas of the *Philosophical Investigations* were still in gestation. It is not surprising, therefore, that the anecdote concerning the Neapolitan gesture contains an important critique of an essential feature of the doctrines of the *Tractatus*. In light of these facts, I find it more judicious to try and look for points of contact between Sraffa and Wittgenstein at a stage when Wittgenstein was critically reflecting on his own *Tractatus*, instead of trying to make direct links with ideas that occurred later, such as the notion of 'language game', which first appeared in the *Blue Book* in 1934, although this is neither altogether inappropriate nor impossible.

Furthermore, since there are very few traces of the interactions between the two, one is forced to appeal, when framing suggestions, to background elements in their thinking that bear strong resemblance. I think that it is fair to say that Sraffa did not impart a new 'line of thinking' by discussing matters of economic theory with Wittgenstein. However, Sraffa must have reacted to Wittgenstein's ideas from his own standpoint and suggestions of a possible 'influence' must thus appeal to a shared background.

In *Sraffa and the Theory of Prices*, Alessandro Roncaglia was probably the first to come up with parallels between Sraffa's critique of the marginalist

⁵On related points, see also Rhees (1996, p. 50), Jacquette (1998, p. 205).

⁶See, however, McGuinness (1986) or Kienzler (1997, pp. 51–55). As for McGuinness, see also the views reported in Sen (2004, p. 40), in accordance with the remarks in footnote 4 above.

⁷I shall discuss it in Marion (forthcoming b).

⁸I have discussed the influence of Ramsey on Wittgenstein in numerous places; see Marion (1995a; 1998, ch. 4; forthcoming a).

theory in the 1920s and Wittgenstein's criticisms of the *Tractatus*, in order to frame valuable conjectures concerning the influence of Sraffa. Roncaglia focused on the following parallels between the marginalist theory and the *Tractatus*:

The marginal theory of value and distribution has been constructed, one might say, on the basis of a philosophical position (whether implicit or explicit, conscious or unconscious) similar to that of the early Wittgenstein, e.g., the atomistic basis of theory and reality (the 'economic agents' and the 'goods'), a correspondence between the facts of the world and the elements of the language (that is, the propositions of the theory as a rational description of reality), the claim to a complete description, according to general rules, of everything in the entire world which can be described (the 'general theories' that are so dear to the neoclassics). (Roncaglia, 1978, p. 122)

Roncaglia thus basically argued from analogy, to the effect that Sraffa's criticisms of the marginalist theory explain similar criticisms by Wittgenstein of similar aspects of the doctrines of his *Tractatus*.⁹

More recently, John Davis has argued in a similar fashion, drawing specific parallels based on Sraffa's 1926 paper 'The Laws of Returns under Competitive Conditions'. According to Davis, this paper contains a critique of marginalist theory that can be extended to a criticism of a feature of the *Tractatus* already noted by Roncaglia, its 'atomism'. In a nutshell, in neoclassical theory, in particular in the variant by Marshall and Pigou circulating in the 1920s, prices were said to be determined simultaneously and symmetrically in terms of demand and supply and these were in turn conceived as functions. Sraffa wanted instead to show that a commodity's value does not correspond to a 'fundamental symmetry existing between the forces of demand and those of supply' (Sraffa, 1926, p. 180) and he criticized in his paper the conception of diminishing and increasing returns underlying Marshall's neoclassical supply curve. He believed that in order to obtain a symmetrical treatment of demand and supply, a merging of very different notions of diminishing and increasing returns into a single 'law of nonproportional returns' is required (Sraffa, 1926, p. 182). Sraffa argued that claims concerning the independence of diminishing and increasing returns with respect to small variations cannot in fact be sustained, and that the determination of commodity values cannot be adequately achieved through partial equilibrium analysis:

If diminishing returns arising from a 'constant factor' are taken into consideration, it becomes necessary to extend the field of investigation so as to examine the conditions of simultaneous equilibrium in numerous industries: a well-known conception, whose complexity, however, prevents it from bearing fruit, at least in the present state of our knowledge, which does not permit of even much simpler schemata being applied to the study of real conditions. If we pass to external economies, we find ourselves confronted by the same

⁹I can only allude to Roncaglia's multi-faceted discussion (Roncaglia, 1978, ch. 7). One interesting aspect is his claim that Sraffa may have imparted a strong dislike of 'general' theories that can decide any statement framed in the language of the theory. For criticisms of Roncaglia's discussion of the influence of Sraffa on Wittgenstein, see Arena (1982).

obstacle, and there is also the impossibility of confining within statistical conditions the circumstances from which they originate. (Sraffa, 1926, p. 187)

Davis sees a parallel here between Sraffa's criticisms of the neoclassical supply curve and the criticism of Wittgenstein's picture theory, which he expressed through the famous Neapolitan gesture:

[...] in the discussion related by Malcolm, Sraffa had suggested that Wittgenstein's understanding of logical form was mistaken in its reliance upon the autonomous statement as a unit of meaning. A gesture, as a special sort of statement, was not meaningful apart from the social conventions dictating its usage. Similarly, then, in the 1926 article, conventions of usage are mirrored by forms of industry interaction under varying returns, such that just as a statement's meaning is dependent upon social context, so the summary measure of an industry, namely, the commodity value of that industry, cannot be established independently of that industry's interaction with other industries in presence of varying returns. (Davis, 1988, p. 33)

Davis rightly fleshes out, in this passage, the gist of the anecdote of the Neapolitan gesture: statements do not get their meaning solely and directly from sharing a logical form with facts, but also from being embedded within social practices. (This could be construed as the *anthropologische Betrachtungsweise* imparted by Sraffa.)

Surely, the essential affinity to which Davis draws our attention is merely a rejection by both Sraffa and the later Wittgenstein of a highly generic sort of atomism. Furthermore, Wittgenstein's moves away from the *Tractatus* can be related, in his manuscripts, to arguments having of course nothing to do with the marginalist theory. However, it seems right to infer that Sraffa, having precisely rejected the implicit atomism at the basis of the neoclassical theory, could not but be critical of the atomism underlying the doctrines of the *Tractatus*. Given the nature of their work, it is all but natural that Sraffa would argue with Wittgenstein on his own terms. After all, Wittgenstein aimed at producing jargon-free trivialities, with which everyone would agree (Wittgenstein, 1953, par. 128). It seems to me therefore correct to argue, as Davis does (using the strategy first laid out by Roncaglia), from motivations in Sraffa's background to his criticisms of Wittgenstein's *Tractatus*. This is essentially what I shall do in the next section.

2.

Silently quoting a famous saying by Keynes, Sraffa wrote in his 1926 paper on 'The Laws of Returns under Competitive Conditions' that, as a result of the marginalist revolution, economic theory had lost

much of its direct bearing upon practical politics, and particularly in regards to doctrines of social changes, which had formerly been conferred upon it by Ricardo and afterwards by Marx, and in opposition to them by the bourgeois thinkers. It has been transformed more and more into 'an apparatus of the mind, a technique of thinking' which does not furnish any 'settled conclusions immediately applicable to policy'. (Sraffa, 1926, pp. 180–181)

One can see here that Sraffa perceived the marginalist revolution as a shift in economic theory from problem-solving to a more 'theoretical' science (in the original sense of the Greek *theorein*), a shift that parallels an increase in the use of mathematics. In opposition to this shift, which he perceived as the essence of 'bourgeois economics', Sraffa sought to revive what he saw as the central project of classical political economy: 'as a simple way of approaching the problem of competitive value, the old and now obsolete theory which makes it dependent on the cost of production alone appears to hold its ground as the best available' (Sraffa, 1926, p. 187). One can see here the influence of Sraffa's socialist background on his thinking. As John Eatwell & Carlo Panico (1987, p. 445) write: 'His socialism demanded an economics that was concrete; that, however abstract, was appropriate to the interpretation of real economic institutions and phenomena'.¹⁰ Perhaps one should see here the influence of Gramsci.¹¹

This was indeed Sraffa's aim from the late 1920s onwards and he is to be credited for providing a new interpretation of the classical economic theory, from William Petty to David Ricardo, in his introduction to Ricardo's *On the Principles of Political Economy and Taxation* (Sraffa, 1951) and in his book on *Production of Commodities by Means of Commodities* (1960). Whether Sraffa's interpretation is accurate or not may be a matter of controversy, but for the purposes of this paper this is beside the point. I shall give a brief sketch, which will be, as usual with such presentations, somewhat lacking in precision.¹² This sketch will provide the elements needed for an understanding of the two points that I shall be making in relation to Wittgenstein. This is in accordance with the strategy presented in the preceding section.

In the 1920s, the received interpretation of the evolution of economic theory was Marshall's view that 'no real breach of continuity' was involved in the move from classical to marginal theory (Marshall, 1920, p. v); classical economists such as Ricardo were thus portrayed as early demand and supply theorists with

¹⁰In a conversation with Gilles Dostaler, Sraffa admitted that he could not have written his book had Marx not written *Das Kapital* and that he felt closer to Marx than to the 'camoufleurs of capitalist reality' (Dostaler, 1982, p. 103). Sraffa also said in that conversation that in his book he was describing the same phenomenon as Marx did, namely the class struggle between workers and capitalists; the latter describing it in terms of the appropriation by capitalists of the surplus value, while Sraffa thought that he described it algebraically, when the net product is divided between wages and profit, with the formula $r = R(1-w)$, where r is the profit rate, w is the share of wages in the net product, and R is the Standard ratio or maximum rate of profit (Sraffa, 1960, p. 22). The distribution of the physical surplus is precisely what is at stake in the class struggle, according to Sraffa (Dostaler, 1982, p. 103).

¹¹Indeed, it is perhaps apposite to see in the perception of the evolution of economic theory away from problem-solving and towards a fully 'theoretical' science as a 'bourgeois' manoeuvre, the influence of Gramsci. Indeed, one could easily describe this manoeuvre in Gramscian terms as 'hegemonic'. This is controversial: Nicholas Kaldor (1984, p. 149) has conjectured that Sraffa shifted his interest from money and banking to the classical theory of value as the result of Gramsci's influence, while Alessandro Roncaglia claims that there seems to be no such influence and that Sraffa's research and results should be judged independently from his political background (Roncaglia, 1983, p. 339; 2000, p. 11).

¹²I rely heavily in the following presentation on Kurz (2003), to which the reader is directed for details.

somewhat inelastic demand (Marshall, 1920, pp. 670–676). Following Sraffa's reconstruction, the classical economists were concerned with the laws governing the emerging capitalist economy, which was characterized by a division between workers and land and capital owners, by wage labour as the dominant form of appropriation of other people's work, by a division of labour within and between firms and by coordination through interdependent markets, where transactions are mediated through money. Sraffa's approach to the classical economists consisted in recovering their focus on the non-accidental and non-temporary factors governing the economic system. This implied a distinction between 'actual' and 'normal' value, the latter reflecting these invariant features of the system. According to classical economists, in conditions of free competition, the system would gravitate around a 'long-period' position characterized by uniform rates of profit and remuneration for each primary input. One had first to isolate the factors at work to determine income distribution and prices supporting that distribution in a given time and place and only then study the causes, such as capital accumulation or technical change, of changes over time.

There were two important features. First, production was conceived following Petty and the Physiocrats as a circular flow, i.e. commodities are seen as produced by commodities, as opposed to the 'one-way avenue that leads from "Factors of production" to "Consumption goods"' (Sraffa, 1960, p. 93). Secondly, the classical economists operated with a concept of 'physical real cost', based upon the underlying assumption that, in the words of Heinz D. Kurz (2003, p. 171), 'Man cannot create matter, man can only change its form and move it. Production involves destruction, and the real cost of a commodity consists in the commodities destroyed in the course of its production.' Therefore the cost of production is calculated 'objectively' in terms of the 'quantities of things used up', the surplus being the difference between the quantities produced and the latter. One can argue that the concept of 'physical real cost' has its modern origin in William Petty's (1690) *Political Arithmetick*, and that variants are to be found in François Quesnay, Adam Smith, David Ricardo, James Mill, Robert Torrens, and Karl Marx. Possibly under the influence of Gramsci, Sraffa focused in the late 1920s on the distinction between 'objective' and 'subjective' factors (Naldi, 2000, p. 92). He profoundly disliked the 'subjective, moral point of view' and wanted to do away with 'subjectivist' concepts, such as 'preferences' or 'utility', introduced by the marginalists, and asked for an 'objectivisation' (D3/12/7: 46, quoted in Kurz & Salvadori, 2004c), i.e. *reliance on observable, measurable quantities alone, to the exclusion of all 'subjectivist' concepts*. Thus no 'reductionism' is implied. Indeed, there is no claim that the 'subjective' concepts should be reduced to 'objective' ones (as in, for example, behaviourism in psychology), only that they should be avoided.

As Sraffa commented in an unpublished note, the 'objectivist' point of view is linked with the conception of production as circular:

This point of view implies replacing the notion that 'commodities are produced by factors of production' with the other that 'commodities are produced by commodities'; the latter amounted to 'replacing the idea that the process of production has a beginning and an end with that that it has a circular one – an idea

first introduced by the Tableau économique.' (D3/12/7, quoted in Kurz & Salvadori, 2004c)

However, in order to be able to determine the general rate of profits and the exchange ratios of different commodities in terms of the 'physical real cost', one would have to solve a set of simultaneous equations. The mathematical tools needed to do so were not available to the classical economists and they naturally set out to reduce the heterogeneity of commodities to a common measure. (Hence, for example, Marx's identification of labour as the common measure of value.) This is the key to Sraffa's controversial 'corn-profit' interpretation of Ricardo:

The advantage of Ricardo's method of approach is that, at the cost of considerable simplification, it makes possible an understanding of how the rate of profit is determined without the need of a method for reducing to a common standard a heterogeneous collection of commodities. (Sraffa, 1951, p. xxxii)

As Sraffa noted – this is the basis for *Production of Commodities* – all that was needed was the solution of a set of simultaneous equations; the problem of 'common measure' could be avoided.

To take the simple example of an economy with no surplus with three kinds of commodities, tools (*t*), raw materials (*m*) and the food of the labourer (*f*), the productions in three industries can be depicted with the following schema:

$$\begin{aligned} T_t + M_t + F_t &\rightarrow T \\ T_m + M_m + F_m &\rightarrow M \\ T_f + M_f + F_f &\rightarrow F \end{aligned}$$

Here, T_i , M_i and F_i are the inputs of the three commodities (as means of production and means of subsistence) in industry *i* and T , M and F are the total outputs in the three industries; the symbol + indicates that all inputs on the left-hand side are required to generate the outputs. From this schema, it is easy to figure out the values based on 'physical real cost'. If one denotes the value of one unit of commodity *i* by p_i , then one has the following set of equations:

$$\begin{aligned} T_t p_t + M_t p_m + F_t p_f &= T p_t \\ T_m p_t + M_m p_m + F_m p_f &= M p_m \\ T_f p_t + M_f p_m + F_f p_f &= F p_f \end{aligned}$$

Fixing a standard of value (whose price is equal to unity) allows one to solve the equations. This can be seen using a numerical example, adapted by Kurz (2003) from Sraffa:

$$\begin{aligned} 2 p_t + 15 p_m + 20 p_f &= 17 p_t \\ 5 p_t + 7 p_m + 4 p_f &= 28 p_m \\ 10 p_t + 6 p_m + 11 p_f &= 35 p_f \end{aligned}$$

For these equations, the values are: $p_t = 3 p_m$, $p_m = 2/3 p_f$ and $p_f = 1/2 p_t$. Here, any of the commodities *t*, *m* or *f* could serve as 'common measure'.

This sort of 'algebraic' thinking – about which more later – is at the heart of Sraffa's economic thinking: *Production of Commodities* comprises only more complex calculations based on such sets of equations, in order to extend the domain of their applicability, first to cases of production with a surplus and then to joint production. For example, for the determination of the general rate of profits r , the following set of equations will allow, when a standard of value is fixed, determination of the general rate of profits and prices:

$$\begin{aligned}(T_t p_t + M_t p_m + F_t p_f)(1 + r) &= T p_t \\(T_m p_t + M_m p_m + F_m p_f)(1 + r) &= M p_m \\(T_f p_t + M_f p_m + F_f p_f)(1 + r) &= F p_f\end{aligned}$$

There is no need to go into further details for my present purposes. There are no controversial issues – bearing on, for example, the case of joint production – that could have any significant effect on what follows. (The validity of the propositions derived by Sraffa or the soundness of his approach is not what is at stake in this paper.) I would like simply to make two observations about the mathematics of *Production of Commodities*.

First, Sraffa limits himself to linear algebra and this should be contrasted with the current reliance on analysis or topology. Since von Neumann, Arrow & Debreu, and McKenzie, the fixed-point theorem is the most common method in general equilibrium theory. This point will be discussed fully in the next section. Secondly, Sraffa is also careful to provide algorithms, for example, when proving the uniqueness of the Standard system (Sraffa, 1960, ch. V). When Sraffa discusses switching of techniques (Sraffa, 1960, ch. XII), he introduces an algorithm for the selection of an optimal set of techniques that has been dubbed recently by Christian Bidard (1998, p. 785) '*l'algorithme de Sraffa*'. I shall make brief comments on both algorithms in the next section. For the moment, I would like to point out that it seems to me *to be essential for Sraffa's whole enterprise that solutions to the sets of equations be at least computable; an 'existence' result that merely states that a given set of equations has a solution without giving us any means to compute it would not do*. Indeed, his mathematics are constructive throughout and it seems to me that Sraffa's viewpoint is that of economics as problem-solving.¹³

My remarks on the influence of Sraffa on Wittgenstein will concern *only* the two fundamental aspects that I have tried to make obvious in the above sketch, namely the 'objectivist' standpoint underlying the notion of 'physical

¹³One can trace the origin of this point of view in R. M. Goodwin's interpretation of *tatônnement* as an algorithm (Goodwin, 1951). But it could also serve to characterize the classical tradition, while it is somewhat lost in the neoclassical tradition, where the approach is of a more *formalist* nature. There is an obvious link between viewing economic theory as *problem-solving* and insisting on *constructive content* that has been expressed recently by Kumaraswamy Velupillai: 'standard mathematical economics is replete with existential theorems without the slightest concern over their constructive or algorithmic status. [...] It is also why economic theory has not been a pleasant playing field for those of us who would like to interpret the cardinal aim of the subject to be *problem-solving*' (Velupillai, 2000, p. 181).

real cost', and the 'algebraic' and 'algorithmic' thinking everywhere in appearance in *Production of Commodities*.

My first point is that one of Wittgenstein's first and most important moves away from the *Tractatus* consists of the adoption, around 1930, of a 'physicalist' stance, which is related to Sraffa's 'objectivism'. There is evidence for Wittgenstein's change of mind in passages such as these:

I used to believe that there was the everyday language that we all usually spoke and a primary language that expressed what we really knew, namely phenomena. I also spoke of a first system and a second system. [...] I do not adhere to that conception any more. I think that essentially we have only one language, and that is our everyday language. We need not invent a new language or construct a new symbolism, but our everyday language is *the* language, provided we rid it of the obscurities that lie hidden in it. (Wittgenstein, 1979, pp. 45–46)

I do not now have phenomenological language, or 'primary language' as I used to call it, in mind as a goal. I no longer hold it to be necessary. All that is possible and necessary is to separate what is essential from what is inessential in *our* language. [...] A recognition of what is essential and what is inessential in our language if it is to represent, a recognition of which parts of our language are wheels turning idly, amounts to the construction of a phenomenological language. (Wittgenstein, 1975, § 1)

In a nutshell, in the *Tractatus*, Wittgenstein discussed the possibility of an analysis of propositions of 'everyday language' in terms of their constituents. While this is not clearly stated, he conceived these propositions as using terms referring to physical, observable objects, while he believed that the process of analysis must terminate at a level of 'elementary propositions' that are a concatenation of 'simple names', which refer directly to 'simple objects'. Wittgenstein left many questions unanswered and the nature of these objects remains controversial. It can be argued that, while they are not analogous to Russell's 'sense-data', they have at least this in common with Russell's 'objects of acquaintance' – that they have to be given to me in immediate experience (see Marion, 2004, pp. 79–84). What is described in the above quotations as the abandonment of 'phenomenological language, or "primary language"' is the result of Wittgenstein's realization in 1929 that there is no such thing as a ground level constituted of 'elementary' propositions about 'simple objects' given to me in experience. This realization forced Wittgenstein to throw away the very idea of an 'analysis': 'We need not invent a new language or construct a new symbolism'.

To see how this change could be related to Sraffa's 'objectivism', one must first clarify the meaning of the term 'physicalism'. There was at that time a debate among the members of the Vienna Circle, in the early 1930s, about the adoption of a 'physicalist' language as the universal language of science, a thesis that involves the rejection of methodological dualism. The term was, however, ambiguously defined by the proponents of 'physicalism', Otto Neurath and Rudolf Carnap. Indeed, Neurath, who was also strongly influenced by Marx and responsible for the official economic views of the Vienna Circle, presented at times the universal language as simply the 'language of physics'

(Neurath, 1983, pp. 54–55).¹⁴ But a language that would contain only metrical concepts would not be suited for the job and Carnap weakened the physicalist thesis and developed a ‘thing-language’, which would contain also qualitative concepts provided that they ‘refer to observable properties of things and observable relations between things’ (Stegmüller, 1969, p. 293).

Opposed to this is the language based on the ‘stream of experience’ of a single, solitary subject, the language of ‘autopsychological objects’, to use an expression taken from Carnap’s *Logical Structure of the World* (Carnap, 1967, par. 58). This language is related to Ernst Mach’s ‘phenomenalism’, i.e. to his attempt at reconstructing science on the basis of atomistic, ‘simple sensations’ (Mach, 1959). While Carnap had remained neutral in *Logical Structure of the World* with respect to the choice of the basic language, in ‘Die physikalische Sprache als Universalssprache der Wissenschaft’ (Carnap, 1931), he decided in favour of the physicalist stance already adopted by Neurath. In short, Neurath and Carnap chose to adopt of physicalist language because they believed that the adoption of the ‘phenomenalist’ language would lead towards solipsism, i.e. the unpalatable thesis that only one mind exists.

There is no evidence that Sraffa was aware of these developments within the Vienna Circle. However, in 1928 he had already read and annotated carefully A. N. Whitehead’s *Science and the Modern World* (1925).¹⁵ In this book, Whitehead described modern science in terms reminiscent of the ‘physicalist’ language of the Viennese: ‘Science was becoming, and has remained, primarily quantitative. Search for measurable elements among your phenomena, and then search for relations between these measures of physical quantities’ (Whitehead, 1925, p. 66). Whitehead also described his own standpoint as an ‘objectivist philosophy adapted to the requirement of science and to the concrete experience of mankind’ (Whitehead, 1925, p. 129) and he further described his ‘objectivism’ in these terms:

This creed is that the actual elements perceived by our senses are *in themselves* the elements of a common world; and that this world is a complex of things, including our acts of cognition, but transcending them. According to this point of view the things experienced are to be distinguished from our knowledge of them. (Whitehead, 1925, pp. 128–129)

This last sentence is meant to contrast ‘objectivism’ with ‘subjectivism’, i.e. the view that ‘what is perceived is not a partial vision of a complex of things generally independent of that act of vision’ (Whitehead, 1925, p. 128). What Whitehead calls here ‘subjectivism’, one usually calls ‘idealism’. In Sraffa’s time, Marxists were critical of ‘idealism’, which they perceived as bourgeois philosophy; one should recall here Lenin’s criticisms of Ernst Mach’s ‘phenomenalism’ in *Materialism and Empirio-Criticism* (Lenin, 1970) or Gramsci’s critique

¹⁴To complicate matters, Neurath also spoke of the unified language as a purified version of everyday language, which he identified with the language of physics (Neurath, 1983, pp. 62, 91).

¹⁵I would like to thank Heinz Kurz for pointing this out to me.

of Croce in his *Quaderni del carcere*.¹⁶ It seems natural for the likes of Carnap, Neurath and Sraffa to adopt an 'objectivist' stance. On the other hand, the 'phenomenalist' language of the Viennese bears strong resemblance to the 'subjectivism' described by Whitehead and rejected by Sraffa. This 'subjectivism' is, however, the basis to Friedrich von Hayek's thinking about economics.¹⁷ The battle lines are here clearly demarcated.

One should now note that in the passages quoted above, Wittgenstein's alternatives are between having or not having a 'basic', 'primary' language that expresses 'what we really knew', and not, as was the case within the Vienna Circle, a choice between the 'thing language' or the language of the 'autopsychological'. It is also an alternative between 'everyday' language and a so-called 'phenomenological' language. But Wittgenstein's 'everyday' language sounds very much like Carnap's 'thing language': 'our ordinary language [speaks] of the events in our environment by talking of objects (things, bodies), ascribing properties to them, or relating them to each other, etc' (Wittgenstein, 1979, p. 254).

As for Wittgenstein's use of the term 'phenomenology', it is derived from that of physicists such as Ernst Mach or Ludwig Boltzmann, whose works were known to him. In physics, the idea of a phenomenology has nothing to do with a contrast between entities such as 'ideas', 'sense-impressions', or 'sense-data' and 'material objects', as is the case with the various philosophical forms of 'phenomenalism' such as Mach's or Russell's. It has to do with the possibility of a self-sufficient description of experience, e.g. a thermodynamics that does not assume molecules.¹⁸ The idea that there could be such a description for ordinary language was at the basis of the *Tractatus*, i.e. the idea of a 'primary' language – the distinction between 'primary' and 'secondary' language is taken from Hertz's (1899) *Principles of Mechanics*, another book read by Sraffa – which would express 'what we really knew'. But this 'phenomenological' language would be a pure description of immediate experience or the 'stream of experience', which is nothing more than Carnap's 'autopsychological' basis (Carnap, 1967, par. 64). This is the idea that Wittgenstein abandoned in 1929.

In the development of Wittgenstein's thought, the move from 'phenomenological' to 'physicalist' language is neither the sole nor the most important step away from the *Tractatus*. Its importance for later developments is a matter of controversy. Merrill & Jaakko Hintikka argued that it is related to the two major arguments of the later philosophy, on 'private language' and 'rule-following' (Hintikka & Hintikka, 1986, p. 138). Again, the validity of such claims is not at stake here. One simply cannot deny that Wittgenstein changed his mind about

¹⁶Lenin's critique of Mach was clearly off the mark. The motives for Gramsci's call for an 'anti-Croce' (Gramsci, 1971, p. 371) are complex and the attack on his idealism only part of it. For Gramsci's discussion of 'subjectivism' and 'objectivism', see Gramsci (1971, pp. 440–448). He did not simply reject 'subjectivism' but called for a synthesis (Gramsci, 1971, p. 402). In this, he is not followed by Sraffa.

¹⁷Hayek's first book, *The Sensory Order*, was an exercise in anti-'objectivism' (Hayek, 1952); see Nadeau (2001) for a discussion.

¹⁸See, for example, Boltzmann (1974, pp. 93f.). This is the 'phenomenological thermodynamics' mentioned by von Neumann (1945, p. 1).

the need for a phenomenological language. In the absence of any direct evidence linking Wittgenstein's change of mind to discussions with Sraffa it is not possible to draw any firm conclusions. However, if it were the case that Sraffa criticized the atomism of the *Tractatus* because his own thinking in economic matters was anti-atomistic, it would hardly have been possible for him not to have criticized in conversation the phenomenological stance of the *Tractatus*, since it amounts to a form of subjectivism which he so staunchly rejected in his own thinking about economic matters. The parallel between Sraffa and Wittgenstein is here too very striking.

3.

My second suggestion is that the mathematics used in *Production of Commodities* and Sraffa's 'algorithmic' thinking are in conformity with Wittgenstein's constructivist stance on the foundations of mathematics. It is even less appropriate here to speak of influence because one can claim that the *Tractatus* already embodies a constructivist philosophy of mathematics, so talk of a change of mind initiated by Sraffa's criticisms is out of question. Moreover, Sraffa had little mathematical training and knowledge; while writing *Production of Commodities*, he repeatedly needed help from the Cambridge mathematicians cited in the book's preface, Abram Besicovitch, Frank Ramsey and Alister Watson (Sraffa, 1960, vi–vii; on this, see Kurz & Salvadori, 2001, 2004b). For example, the (algorithmic) proof of the existence of a Standard commodity in note (D3/12/39–42) is literally from Besicovitch's hand.¹⁹ Furthermore, Sraffa never had any ostensible interest in issues concerning the foundations of mathematics. Therefore, *he could hardly be suspected of having had strong convictions in these matters.* (And strong convictions are what is needed to adopt a minority view such as constructivism in mathematics.) On the other hand, these are matters to which Wittgenstein devoted a great deal of space.

At any rate, even if Sraffa's attitude towards mathematical methods in economics is not derived from some prior philosophical belief about what is good mathematics or not, it remains that the mathematics used in *Production of Commodities* are correctly characterized as 'constructivist', while the true reasons why Sraffa eschewed non-constructive mathematics are surprisingly in line with Wittgenstein's own thoughts about mathematics. The claim that Sraffa implicitly adopted a constructivist stance is at any rate not new, having previously been argued for by numerous economists (see Bidard, 1998; Chakravarty, 1989; Dore, 1989; Punzo, 1986, 1989, 1991; Schmidt, 1985, 1988, 1990).²⁰ My discussion will differ from these because of the new connections that I establish with Wittgenstein's ideas. Before doing so, however, I shall give a very elementary and very brief presentation of the constructivist critique of 'existence' theorems in mathematics and discuss in more details the contrasts between Sraffa's

¹⁹I thank Neri Salvadori for pointing out this note for me.

²⁰This opinion is, of course, not universally shared. For sceptical comments, see Schefold (2004, p. 317). Heinz Kurz and Neri Salvadori have also expressed their scepticism to me in private correspondence.

mathematics and von Neumann's celebrated 1937 result on the existence of an economic equilibrium, as well as the differences between their respective models.

There can be no entirely satisfactory definition of constructivism in mathematics, since the term refers to half a dozen programmes or schools of thought that have little in common.²¹ However, these schools have at least one point in common, the rejection of pure 'existence' theorems. In simple terms, 'existence' theorems are statements of the form $\exists x A(x)$ that merely claim there exists an object x that possesses the property A , while the proof does not provide the means to exhibit an instance that has the property A . For constructivists, one must be able to exhibit, with a construction, such an instance. There are some philosophical grounds for such claims. Existence theorems can be justified by appeal to a Platonist philosophy about a realm of abstract entities existing independently of our cognitive capacities, while the constructivist view derives from a more Kantian approach: mathematical entities are constructed by us. At all events, there are some straightforward logical consequences. For example, this means that, if a statement $A(x)$ does not hold for every integer x , one is not allowed automatically to infer that there must exist an integer x for which this statement is false. In other words, constructivists prohibit use of this logical principle:

$$\neg \forall x A(x) \rightarrow \exists x \neg A(x).$$

This prohibition originates in the 19th century, in the writings and teaching of Leopold Kronecker, and it was developed in the 20th century by L. E. J. Brouwer into a full-blown critique of the Law of Excluded Middle:

$$A(x) \vee \neg A(x)$$

Brouwer's critique is at the origin of the school of intuitionism, which is a better-known variant of constructivism. Among other features, intuitionistic logic distinguishes itself by the rejection of the principle of double negation elimination:

$$\neg \neg A(x) \rightarrow A(x)$$

Existence proofs are often obtained by use of this principle in a *reductio ad absurdum*: assuming $\neg A$ implies a contradiction, so it must be the case that A . This principle of proofs is thus rejected by constructivists.

It is often claimed that adoption of intuitionistic logic would entail giving up mathematical results. Whether this viewpoint is the appropriate one for the foundations of mathematics is quite another question. However, one should note that it is often (but in principle not always) the case that 'existence' proofs only hide ignorance of an algorithm and that constructive proofs can be supplied. For example, Errett Bishop (1967) has given a constructive version of large parts of classical analysis in *Foundations of Constructive Analysis*. This fact will become important below.

L. E. J. Brouwer also happens to be one of the founders of modern topology but he did not adhere to his own logical strictures in his work on topology

²¹For basic information on the various schools of constructivism in mathematics, the reader should consult Troelstra & van Dalen (1988) or Bridges & Richman (1987).

(this does not mean that it has no constructive content) and his fixed-point theorem is a perfect example of an 'existence' proof. This example is worth mentioning since John von Neumann's celebrated result in 'A Model of General Economic Equilibrium' (1945) that *there exists* a set of prices for which supply equals demand in every market in any set of equations used to describe a competitive economy is also an 'existence' theorem. It asserts the existence of a certain object, the set of prices, without giving the means (algorithm) to obtain it. The argument is of the form: the non-existence of a solution involves a contradiction, therefore it is impossible that there is no solution.

As one can see from the original title of von Neumann's (1937) paper, 'Über ein Ökonomisches Gleichungssystem und eine Verallgemeinerung des Brouwerschen Fixpunktsatzes', the proof contains a lemma using Brouwer's fixed-point theorem. A generalization of the latter by Kakutani (1941) stands at the basis of much of today's general equilibrium theory, where the commodity space has the structure of a real vector space (with useful properties such as convexity). By contrast, the mathematics involved in *Production of Commodities* are limited to the perfectly constructive sphere of linear algebra.²² Sraffa's model can be given a matrix formulation and the key proof of the book, i.e. the existence of a unique Standard system, can be obtained using purely algebraic means, i.e. the Perron–Frobenius theorem on non-negative matrices (see Burmeister, 1968).²³ However, one should note that Sraffa avoided a matrix formulation. (The reasons for this will be discussed below.) Two preliminary remarks concerning this contrast are in order.

First, merely highlighting the difference between the constructive setting for Sraffa's model and the non-constructive nature of von Neumann's result may give rise to confusion. Since the famous exchange between Kaldor and Solow at the Corfu conference on 'Capital Theory' in 1959, the interpretation of von Neumann's model has been a matter of controversy. In short, von Neumann's growth model appears at a juncture. It is hailed by neoclassical economists as a crucial step between Cassell and Debreu, while others emphasize features of the model that justify looking at it, on a par with Sraffa's model, as part of the tradition of classical economics. Somewhat caught in the middle are some modifications or extensions of von Neumann's model that retain features from both traditions, such as Brody's (1970), Goodwin's (Goodwin & Punzo, 1987) or Pasinetti's (1981). It is of course not the purpose of this paper to adjudicate this debate, although the arguments towards a rapprochement of von Neumann's model with the classical tradition seem more compelling (Kurz & Salvadori, 1993, 2004a). In this context, emphasis on the constructive character of the mathematics underpinning Sraffa's model might provide grounds for the belief that, while Sraffa's model is clearly of 'classical' inspiration, von Neumann's belongs to the neoclassical tradition. What is at stake in this debate is not the mathematics but the economic features of the model (e.g. whether it is a 'long-period' model or not) and, on that

²²For a non-set-theoretical introduction to linear algebra that makes patent its constructive content, see Edwards (1995).

²³For an overview of the role of the Perron–Frobenius theorem, see Punzo & Velupillai (1984).

score, there are good grounds to link von Neumann's model with the classical tradition. The issue about mathematics cuts across this debate for the simple reason that the use of non-constructive mathematics is not a necessary and sufficient condition to belong to the neoclassical tradition. As Punzo (1989, p. 42) has pointed out, Cassel was aiming at a constructive proof of the existence of an equilibrium. Chakravarty (1989, p. 73) has also insisted that the non-constructive mathematics in von Neumann's paper are not sufficient in themselves for characterizing his model as neoclassical. However, von Neumann's paper has opened the door to a more 'formalist' mathematical treatment of economic questions, which is undoubtedly the mark of neoclassical mathematics. I shall come back to this important point.

Secondly, one should note that von Neumann's growth model can be seen as an interpretation of his saddle-point or 'minimax' theorem (Dore, 1989, pp. 87–92), which is also proved with help of the fixed-point theorem (von Neumann, 1928). As pointed out earlier, most 'existence' theorems are *bona fide* and constructive proofs can be obtained. A proof by Ville (1938) had already shown von Neumann's use of the fixed-point theorem to be redundant. It has been shown also that the latter's methods were sufficient to obtain von Neumann's equilibrium result (Georgescu-Roegen, 1951). But these results still appeal to topological tools, while George Dantzig, who is better known for the invention of the simplex algorithm, has given a constructive proof of the minimax theorem (Dantzig, 1956), while a constructive proof by linear programming methods has also been given for solutions in the generalized von Neumann model (Kemeny *et al.*, 1956). These results show that, from a constructivist standpoint, von Neumann's uses of the fixed-point theorem in economics are essentially harmless, since the results can also be obtained constructively.

This point serves to reinforce my comment, above, concerning the fact that a discussion of the mathematics underpinning Sraffa's and von Neumann's models has precious little bearing on the issue of the interpretation of the latter as pertaining or not to the classical tradition. There has been a recent tendency to picture Sraffa's *Production of Commodities* and Debreu's *Theory of Value* as alternative explanations against a common background of perfect competition in a market economy, while comparisons were made between Sraffa's and von Neumann's models (Steedman, 1976; Schefold, 1978, 1980). It is not clear that minimizing Sraffa's interest in conditions of imperfect competition provides an adequate picture of his overall intellectual project (see Arena, 1998). Be this as it may, it seems to me that the use, in the context of the problem of the selection of an optimal set of techniques, of non-constructive methods derived from von Neumann's famous 1937 paper²⁴ is less faithful to the spirit of Sraffa's *Production of Commodities* than, say, Christian Bidard's approach in 'An Algorithmic Theory of the Choice of Techniques' (Bidard, 1990). However, in this context the use of non-constructive methods is harmless, as Bidard (1998, pp. 784–787) himself recognized. The real difficulty comes with the fact that von Neumann's 1937

²⁴For example, in Schefold (1980) and Salvadori (1984).

result opened the door to a more 'formalist' approach to the use of mathematical methods in economics.²⁵

Christian Schmidt, using a semantical characterization *à la* Carnap of the relation between mathematical formalism and economic reality, has argued for the contrast between Sraffa's constructivism, for which it is impossible to disconnect the theory from its interpretation, and *inter alia* the formalist approach of neoclassical economists such as Debreu, for whom 'the theory, in the strict sense, is logically entirely disconnected from its interpretations' (Debreu, 1959, p. viii; see Schmidt, 1988, *passim*; 1990, pp. 105–107). (There are difficulties, however, with Schmidt's attempt at giving a more precise characterization of Sraffa's implicit constructivism, to which I shall return.) Indeed, Sraffa was clearly sensitive to the fact that the concepts he used and the relationships he discussed had to have a clear, straightforward economic interpretation; *this is, after all, what his 'objectivism' is all about*. His collaboration with the mathematicians Besicovitch, Ramsey and Watson shows that Sraffa was always reasoning first in economic terms and that he would seek help from mathematicians only at a later stage to complete the proofs. An entry in his diary from May 31, 1958 is rather telling: 'Besicovitch insists that I publish: the fact that I was able to foresee interesting mathematical results shows that there must be something in the theory' (quoted in Kurz & Salvadori, 2004b, p. 259).

It may be that Sraffa's avoidance of the matrix formulation is to be explained by his wish to avoid heavy reliance on mathematics in his exposition. It remains that the establishment of a one-to-one correspondence, i.e. an isomorphism between the mathematical entities and the real entities was fundamental to Sraffa and the idea that there might be more than one interpretation of the model or that the 'theory [...] is logically entirely disconnected from its (economic) interpretations' are simply foreign to him.²⁶ Moreover, uses of the fixed-point theorem and the topological standpoint within the neoclassical tradition raise problems with the one-to-one correspondence, since completed infinities are introduced that have no empirical counterpart. And the matrix formulation implies constant returns to scale (Dore, 1989, p. 84), an assumption that Sraffa did not wish to make (Sraffa, 1960, p. v).²⁷ As Mohammed Dore has pointed out, such difficulties are no doubt why Sraffa refrained from using these methods:

Sraffa does not express his results by using matrix methods. A methodological purist, and philosophically inclined towards constructivism, he could not possibly compromise his inquiry by embedding his theory in a more general mathematical structure which could raise the possibility of additional assumptions, a baggage that he did not need. For example, expressing the Sraffa system by

²⁵Lionello Punzo (1989, 1991) has investigated the motives for von Neumann's use of topological methods in his dealings with Karl Menger's *Mathematical Colloquium* in Vienna and in the 'formalist' philosophy of Hilbert, by whom he was much influenced in the late 1920s.

²⁶Joan Robinson (1965, p. 34) famously wrote about *Production of Commodities* as a 'prelude' to a critique of the marginalist theory that 'the function of pure logic is to liberate us from nonsense, not to tell us what we ought to believe'. This reading seems to me to be inaccurate for the very reason that Sraffa never conceived his model as disconnected from its interpretation.

²⁷Even though he did make some assumptions on returns; see Salvadori (1985).

matrix methods would imply that he was assuming constant returns to scale, an assumption he did not wish to make. [...] [E]mbedding entails the potential of a loss of focus, unless the one-to-one correspondence between mathematical objects and economic entities is carefully preserved, with no loss of economic intuition. (Dore, 1989, p. 87)

All this is fine, except perhaps the claim that Sraffa was 'philosophically inclined towards constructivism', a claim for which there is no direct evidence. This is where Wittgenstein's philosophy of mathematics, properly understood, may help to clarify matters.

It can easily be shown that Wittgenstein shared with other constructivists their rejection of 'existence' proofs (see for example, Wittgenstein, 1978, v, §§ 25 & 46, vii, § 41). There is only one known mathematical proof due to Wittgenstein and it is a constructive version of a well-known existence proof by Euler of the infinity of prime numbers (Wittgenstein, 1994, p. 325).²⁸ But Wittgenstein criticized some of the arguments of Brouwer and the intuitionists (thus causing quite a lot of confusion) and he reasoned from different premises.²⁹ The fundamental premise to Wittgenstein's philosophy of mathematics is that mathematics is essentially 'algorithmic', i.e. a sort of high-level abacus activity. According to him, mathematical statements are merely *Scheinsätze*: they are not pictures, they do not express a thought, like ordinary propositions (Wittgenstein, 1922, par. 6.2). Therefore, when mathematics is applied to a real-life situation to solve a problem, it cannot be conceived as a 'description', let alone as an 'explanation'. It merely is a calculation that allows one to make a transition from one ordinary proposition to another. Wittgenstein expressed this with great clarity in the *Tractatus*:

In life it is never a mathematical proposition which we need, but we use mathematical propositions *only* in order to infer propositions which do not belong to mathematics to others which equally do not belong to mathematics. (Wittgenstein, 1922, 6.211)

Thus, for Wittgenstein, 'Mathematics is always a machine, a calculus. The calculus does not describe anything' (Wittgenstein, 1979, p. 106).³⁰ The view is also expressed in a passage dating from 1931:

Mathematics consists entirely of calculations. In mathematics *everything* is algorithm and *nothing* is meaning; even when it doesn't look like that because we seem to be using *words* to talk *about* mathematical things. Even these words are used to construct an algorithm. (Wittgenstein, 1974, p. 468)

If mathematics is conceived as a sort of high-level abacus, then it is clear that there is no room for 'existence' proofs, where an algorithm is lacking but also where the

²⁸There is a detailed analysis of that proof and the light it sheds on Wittgenstein's views on mathematics in Mancosu & Marion (2003).

²⁹I have offered my interpretation of Wittgenstein on mathematics in Marion (1998).

³⁰This fundamental feature of Wittgenstein's philosophy of mathematics is in line with Kronecker's 'philosophy'; see Marion (1995b).

$\exists x A(x)$ would be a 'description'. Indeed, Wittgenstein abhorred above all the 'philosophical' view that a mathematical statement $\exists x A(x)$ could be a 'description' on a par with an ordinary statement that describes reality.

One consequence of Wittgenstein's refusal to conceive mathematical statements as 'descriptions' on a par with empirical statements is his refusal to recognize that there is anything like a *metamathematics*, i.e. mathematical statements about mathematical statements. This led him to a straightforward rejection of Hilbert's metamathematics:

The system of calculating with letters is a new calculus; but it does not relate to ordinary calculation with numbers as a metacalculus does to a calculus. *Calculation with letters is not a theory*. This is the essential point. In so far as the 'theory' of chess studies the impossibility of certain positions it resembles algebra in its relation to calculation with numbers. Similarly, Hilbert's metamathematics must turn out to be mathematics in disguise. (Wittgenstein, 1979, p. 136).

Again, all this is highly controversial, but the correctness of these views is not what is at stake here. I wish to attract attention to the fact that the 'algebraic' and 'algorithmic' thinking at the heart of *Production of Commodities* as presented in the previous section fits very well Wittgenstein's conceptions. The above discussion of the anti-formalist aspect of Sraffa's economic thinking is also in perfect conformity with Wittgenstein's criticisms of the formalist philosophy of Hilbert. This also fits the demands of Sraffa's socialist convictions, alluded to at the beginning of that section. Moreover, a very interesting parallel obtains between Wittgenstein's lifelong critique of the view that mathematics is essentially an activity of description of a world of abstract objects, as opposed to the provision of algorithms that solve practical problems, and Sraffa's belief, already quoted, that economic theory 'has been transformed more and more into "an apparatus of the mind, a technique of thinking" which does not furnish any "settled conclusions immediately applicable to policy"' (Sraffa, 1926, p. 181). For both Sraffa and Wittgenstein, mathematics provides us with algorithms that allow us 'to infer propositions which do not belong to mathematics to others which equally do not belong to mathematics', not with propositions which describe a (idealized economic) reality.³¹

It appears, however, that Sraffa had a less than perfect grasp of these issues, as the following story shows. In Section 37 of *Production of Commodities*, Sraffa presents, as a proof of the uniqueness of the Standard system, an algorithm which consists of the repetition of two steps – 'changing the proportions of the industries' and 'reducing in the same ratio the quantities produced by all industries, while leaving unchanged the quantities produced by all industries' (Sraffa, 1960, p. 26) – until a solution is found.³² As

³¹One should note here that Maurice Dobb (1973, p. 8) quoted with approval par. 6.211 of the *Tractatus* in *Theories of Value and Distribution since Adam Smith*. This shows awareness and appreciation in Sraffa's entourage of Wittgenstein's standpoint on mathematics.

³²For a formal version of the algorithm, see Kurz & Salvadori (2001, pp. 272–273).

Kurz & Salvadori have shown in their careful analysis of Sraffa's papers, Alister Watson,³³ who visited Sraffa to help, had qualms about the first step: there are infinitely many ways to perform it but steps in an algorithm need to be uniquely defined. As a mathematician, Watson raised the point but, as Kurz & Salvadori note, 'it is not clear whether Sraffa shared Watson's concern' (Kurz & Salvadori, 2001, p. 273).

I would like to conclude this section with a few critical remarks concerning Schmidt's interpretation of Sraffa's constructivist stance. Taking his lead, *inter alia*, from the fact that Sraffa's algebraic tools are constructive, Schmidt claimed that Sraffa's stance on the foundations of economics is 'the only known example of a treatment of theoretical economics directly in accordance with the principles of [Brouwer's] intuitionism' in the foundations of mathematics (Schmidt 1988, p. 17; see also Schmidt, 1985, pp. 57f; 1990, pp. 107f). This labelling, taken literally, is incorrect. Brouwer's intuitionism distinguishes itself from other constructivist schools by some idiosyncrasies.³⁴ The most obvious one is Brouwer's claim that mathematics is the product of the mental activity of a solipsist mathematician, whose results are only imperfectly communicated to others through language; in particular, the fundamental sequence of natural numbers is based on the mathematician's intuition of time. This arch-subjectivist viewpoint could hardly be said to be congenial to Sraffa or, for that matter, to Wittgenstein, who thought that this is 'all bosh' (Wittgenstein, 1976, p. 237).³⁵ It is true that, in opposition to the Arrow–Debreu model, which leaves no room for time (e.g. all transactions are assumed to take place at the present instant, the equilibrium consists of a simultaneous clearing of all markets, etc), Sraffa's, like von Neumann's, is a long-period model. It seems to me inappropriate, however, to draw analogies between Sraffa's key notion of 'self-replacing state' and intuitionist notions such as 'infinitely proceeding sequences' or 'law-like sequences' (Schmidt, 1990, pp. 107, 109). These are fundamentally different notions.

At any rate, it turns out that Schmidt's considered judgement is that Sraffa went beyond intuitionism: he conjectures that there is an intimate connection with the peculiar brand of constructivism presented by Wittgenstein in his *Remarks on the Foundations of Mathematics* (Wittgenstein, 1978). Unfortunately, Schmidt makes a few mistakes, at this stage, in trying to substantiate his claim

³³Little is known about A. G. D. Watson (1908–1982). He was a scholar and Fellow of King's and an Apostle. He is said to have introduced Wittgenstein to Turing. A mathematician, Watson published only one philosophy paper on 'Mathematics and its Foundations' (Watson, 1938), in which he expressed views akin to Wittgenstein's. According to Georg Henrik von Wright (private communication) Wittgenstein recommended reading that paper. During the war, Watson worked for the Admiralty, first on radio communication, then sonar. He finished his career working on oceanography. Watson was also known for his communist sympathies prior to the war; it was claimed after his death that he might have been a spy, linked to the famous Cambridge ring. It was only after the war that Watson travelled to Cambridge to help Sraffa.

³⁴See Brouwer (1975, Vol. 1) and Bridges & Richman (1987) for the differences between intuitionism and other constructivist schools.

³⁵On the rather complex philosophical relations between Brouwer and Wittgenstein, see Marion (2003).

(Schmidt, 1990, p. 112). The implication here is that Wittgenstein's stance on the foundations of mathematics was of a more radically constructivist nature than intuitionism, a sort called 'strict finitism'. Once more, this labelling seems inappropriate. Strict finitism can be defined in terms of a distinction between possibility *in principle* and possibility in practice or *feasibility*. To see what is meant here, it suffices to pick an arithmetical operation such as exponentiation and a number such as 2^{63553} . This number consists of two decimal numbers whose position shows what operation must be performed on them (multiply 2 by itself 63553 times) to obtain the number in the decimal system. It is usually assumed that although it is impossible *in practice* to carry out this task – the universe does not contain enough matter to carry out the computation – and thus to obtain the number in the decimal system which corresponds to 2^{63553} , the task can be achieved *in principle*. Strict finitism is defined by the rejection of everything which is outside of the domain of the 'feasible'.³⁶ This point of view may sound too radical, but nowadays the concept of 'feasibility' has gained renewed importance in theoretical computer science (where the complexity of algorithms is of fundamental importance) under the name of 'polynomial-time computability' – a stricter notion than mere 'effective' computability.³⁷ Schmidt assumes that Wittgenstein was a strict finitist but this interpretation is highly controversial (see Marion, 1998, ch. 8 for a refutation). Furthermore, as the above has already made clear, there is no trace of any preoccupation about the complexity of algorithms in Sraffa.³⁸ This shows, once more, the limits of Sraffa's thinking about these matters.

4.

Wittgenstein's thinking in the early 1930s is characterized by his abandonment of the search for a 'phenomenological' language in favour of an analysis of ordinary language, conceived broadly as 'physicalist' and by a working out of the constructivist stance on mathematics already implicit in the *Tractatus*. Wittgenstein's remarks about his conversations with Sraffa indicate that he gained from them an 'anthropological' way of looking at things. Apart from some brief remarks, I have stayed away from a discussion of the meaning of this *Betrachtungsweise*. I have tried instead to contribute to our understanding of the relation between Wittgenstein's thought and Sraffa's by concentrating on these two aspects, physicalism and constructivism, that have been for the most part overlooked in the secondary literature. In both cases, it seems to me that remarkable parallels obtain between the thoughts of these two Trinity men.

³⁶Obviously, the issues are more complex. For a more detailed presentation, see Gandy (1982).

³⁷Some economists, e.g. Velupillai (2000), are now taking computability and polynomial-time computability seriously.

³⁸In Sraffa's background, I could only find such preoccupation in Enrico Barone's (1935) 'The Ministry of Production in the Collectivist State'.

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