

## Sraffa's Protoeconomics

A Comment on Ajit Sinha:  
Interpreting the nature of Sraffa's equations:  
A Critique of Garegnani's Interpretation

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Sinha's paper opens with an interesting question. What is new in the way of how Sraffa leads back to classical economics? According to Sinha, Sraffa disliked that Smith used subjectivist hypotheses like self-love. There are notes in the archive where Sraffa calls Smith the first of the vulgar economists. Smith's adding-up theory of value may be one of the reasons why Sraffa chose this denomination. Another could be that Sraffa strove to eliminate subjectivist foundations of economic thought from his analysis, and Sinha suggests that Smith's use of self-love constituted such a subjectivism. We know from Smith's *Theory of Moral Sentiments* that his psychology was more sophisticated than that, although it is true that, in the *Wealth of Nations*, not much remains of the more complex notions except this self-love. No economist would deny the importance of profit maximization as an explanation of capitalist behaviour. Smith's notion of self-love is broader than that, hence the ease with which neoclassical economists identify with Adam Smith in the belief that, where there is self-love, there is also the maximization of utility. Sraffa, according to Sinha, wished to eliminate such subjectivist notions. To be more precise: Sraffa did not deny the reality of profit maximization but he sought to found his theory on another basis. It is true that profit maximization is not invoked explicitly in *Production of Commodities by Means of Commodities* but there is the uniform rate of profits. Sraffa says, where he introduces this rate, that it "must" be uniform, without explaining the "must". (Sraffa 1960, 6) Garegnani does not hesitate to invoke competition, driven by the maximization of profits, to explain this "must", according to Sinha (2019). Ajit Sinha believes that here the pupil misunderstood his master. Sinha cannot seriously deny that Sraffa accepted the reality of profit maximization, but he is of the opinion, that Sraffa did not make such an assumption. Hence he claims that Sraffa's theory of prices can be founded without it. How can we interpret such a claim?

We ask more generally, how a new academic discipline can be founded or on which grounds a new theory can be erected. One difficulty of founding economic theory as a scientific discipline derives from the fact that we are part of the economy, our behaviour is shaped by our economic experiences and our language is full of economic terms. Their significance should be explained and not be taken for granted.

The conceptual framework of the new theory must be founded on something outside the subject under consideration, otherwise, one moves in a circle, because *explanans* and *explanandum* get mixed up.<sup>1</sup> The concepts thus introduced must refer to a universe of which we have an intuitive understanding. We have, in a Kantian tradition, to reflect on the conditions of cognition. Kant thought that the instruments for our intuitive apprehension of the empirical world were given to us, as far as space was concerned, by our vision of Euclidian space, and the conceptual apparatus for its description was given prior to experience in the form of mathematics. The foundations of physics therefore could not be anything. The movement of masses in mechanics could only occur according to laws compatible with Euclidian space, with our understanding of time and our consequent analysis of movement. Kant therefore undertook to derive the fundamentals of physics, for him the Newtonian axioms, from his philosophy in his *Metaphysische Anfangsgründe der Naturwissenschaft* (Kant 1787), and the result seemed convincing until it was understood that there were also other geometries beyond the Euclidian one, that the speed of light and communication had a specific limit and that elementary particles were quantised.<sup>2</sup> Kant had hoped thus to get to fundamental statements that would be true *a priori* and yet be more than tautologies (that would be “synthetic” and not only “analytical” truths). In this he failed, but the postulate that the instrument of the description should be given prior to the description has remained as a methodological principle for deductive reasoning.

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<sup>1</sup> This circle is on the contrary potentially virtuous, not vicious, according to the hermeneutical approach as an alternative to the Kantian position: The study of e.g. philosophical texts begins with a reading based on the ordinary understanding of the concepts used. These take on a deeper meaning for the reader as the concepts can be used to clarify their meanings mutually by repeated reading, by studying connexions between related texts, by contextual analysis and by communication with others engaged in same endeavour. This is an inductive approach to science, whereas Sinha takes *Production of Commodities by Means of Commodities* to be a piece of deductive reasoning.

<sup>2</sup> In what was in my recollection the most fascinating seminar during my time as a student, the physicist and philosopher Carl Friedrich von Weizsäcker took us through the *Metaphysische Anfangsgründe* and showed from paragraph to paragraph how Kant deduced the elements of Newtonian mechanics from his theory of cognition and failed to arrive at Einsteinian physics primarily because he took assumptions such as that of an infinite velocity in the transmission of information or Euclidian properties of space as obvious truths. A short introduction to this exercise is in von Weizsäcker (1971, 189-192).

But the postulate cannot be fulfilled completely. What could be the fundamental concepts outside economics which one might use to found economic theory? How shall we introduce concepts such as good, commodity, labour without a prior understanding of economic matters, basing ourselves exclusively on a non-economic discourse? The neoclassicals, at least in the Austrian variant (Mises 1940), reply that all action is economic and everything economic has to do with human action. The essential propositions of economics then follow from the understanding of action, by gradually filling up this deductive conception with intuitive content. The economic agents, in possession of their faculties and of goods, can produce for themselves or exchange. They exert their freedom to act in the act of exchange. While Weber wanted to understand and order different rationalities, there are not different rationalities but different degrees of understanding of what is going on in the Austrian approach. A rain dance is rational action, insofar as one tries to influence superhuman forces to make it rain, but one is in error regarding the causes, which make the weather change. Rationality then implies utility maximisation etc.

Marx, with his dialectical materialism, chose a different procedure. He took a number of elementary economic concepts such as labour and exchange and defined plausible relations from which he could deduce a theoretical framework, such that more complex notions could be derived. He assumed that there was labour, distinguished between abstract and concrete forms of labour, postulated that goods exchanged according to their content of abstract labour – a proposition, which seemed roughly plausible, but of which Marx knew that it was only a provisional approximation to the true state of things in a competitive capitalist world. He got from a theory of value to a theory of exchange, money and production, from there to exploitation and accumulation. The drive of capital was to chase after surplus value, but it first had to be explained that profit was a form of surplus value, and before there could be talk of a maximization of the rate of profit, the rate of profit had to be defined. Competition was not any form of human rivalry, but it showed in the equalization of the rates of profit and the tendency for a general rate of profit to form. The uniform rate of profit was therefore not simply assumed as a result of competition, but, on the contrary, competition had first to be explained in terms of the process of the equalization of rates of profit, and these resulted from the drive to amass surplus value by exploiting labour.

As soon as this has been understood, one can see (cf. Schefold 2019), looking at the beginning of the Marxian construction, that it is really the opposite of what Austrian neoclassicism assumes: The value ratios do not result from the free intercourse of independent agents who may decide to swap or not to swap, but the exchange takes place between interdependent producers according to value relationships that are given to them. The material circumstances determine what they can do; Marx sticks to his materialist determinism even where he deals with entrepreneurial action. The capitalist is only the “character mask”; the process of surplus value creation is impersonal and anonymous.

Sinha is right that Sraffa has his own way of founding economics, at any rate that section of it which is concerned with value, price and distribution. I used to call this Sraffa’s protoeconomics. He begins with his famous consideration of an “extremely simple society”. Not the logic of action, not predisposed labour times, but the structure of the reproduction of commodities determines the price ratios. The author leads the reader from assumptions to conclusions. The homogeneity of the commodities is clearly simply assumed and it is simply assumed that the structure of production is such that self-replacement is possible.

Various metaphors have been used to visualize how these magnitudes become known. Sinha prefers to speak of an observer. This is reminiscent of Einstein’s thought experiments and Sraffa, in taking up this particular metaphor, probably was influenced by contemporary discussions of modern physics, as Kurz and Salvadori (2005) argue. Thought experiments involve strong assumptions and abstractions. Sinha is in danger of taking the metaphors too literally. Sraffa begins his protoeconomics with

$$\mathbf{A}\mathbf{p} = \mathbf{p} \text{ and } \mathbf{e}\mathbf{A} = \mathbf{e}$$

where  $\mathbf{e}$  is the summation vector, so that the second formula is the otherwise self-explanatory formalization of “self-replacement”. If this is assumed and  $\mathbf{A}$  is an indecomposable semi-positive input output matrix, the second equation shows that  $\mathbf{e}$  is a Frobenius eigenvector of  $\mathbf{A}$ , with an eigenvalue of 1, so that the right-hand side

It is Frobenius eigenvector  $\mathbf{p} > \mathbf{0}$  must also exist: The existence of prices is guaranteed. a foundation of prices, independent of a labour theory of value or of utility theory.

There follows the model of production with a surplus, where, instead of labour inputs, wage goods consumed by the labourers, whatever their social status, slaves, serfs or wage labourers, are treated on the same footing as the inputs to production.

The formula summarizing this model is

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

Here we have the uniform rate of profit  $R$ , of which Sraffa said that it “must” be uniform, without telling us why. Sinha sees two possibilities to explain the “must”.

There is his own attempt to provide a structural reason and Garegnani’s reference to competition, which Sinha criticizes extensively. Sinha’s procedure is curious. The structural reason, which he tries to give, is nowhere to be found in Sraffa’s published writings. The opposition between Garegnani and Sraffa, which he tries to reconstruct, is *a priori* not plausible. Garegnani was without question Sraffa’s most important pupil, his friend and support over many years to the end. Garegnani may have had his own agenda, but there was trust between them and Garegnani was appointed Sraffa’s literary executor. Finally, it does not occur to Sinha that a structuralist explanation of the “must”, if one really exists, and Garegnani’s interpretation do not necessarily exclude each other; they might be complementary on two different levels of abstraction.

Garegnani’s interpretation is indispensable, if one wants to get from protoeconomics to economics. For instance, the importance of competitive processes shows in classical theory in the treatment of market prices. If one moves from Sraffa’s protoeconomic prices to the classical theory, whether one speaks of the Ricardian theory of rent or of the Marxian theory of accumulation, whether one deals with Malthus and effective demand or with John Stuart Mill and the stationary state: One will always use natural prices to conceptualize different states of the economy in evolution, and one will know that market prices deviated from them, in ways that depend on competitive processes, because of harvest cycles, crisis and political disturbances. And what is the meaning of the natural prices, if the market prices do not somehow gravitate to them?

Sinha nowhere explains how he wants to get from protoeconomics to economics and loose his innocence, as it were. The diversity of his observations forces me also to make diverse remarks that can not be ordered without some arbitrariness.

We can start from market prices, since Sinha begins with Garegnani's interpretation of the formation of market prices in Adam Smith. He speaks of demand and supply schedules in this context, as if Smith had a visualization of supply and demand as in neoclassical theory. Here Sinha seems to me to be uninformed about the state of discussion of supply and demand in the 18<sup>th</sup> century. In particular, reference should be made to James Steuart. In the 18<sup>th</sup> century, and still in Marx, demand and supply were seen as "forces". This visualization of the market process is fundamentally different from that expressed in terms of schedules. Such schedules are absent in Smith; they first appear in Rau and in Cournot. But this is another story which it would take longer to explain (Schefold 1997).

An example that we find in the earlier literature concerns the arrival of a ship in a colony (e.g. Steuart 1967 [1767, 1805] pp. 270-273). If people in the town can expect that other ships will follow soon, the merchant will sell his wares at more or less habitual prices, that is, prices that reflect normal costs, because the buyers will otherwise prefer to wait. If the arrival of other ships is more uncertain, moral suasion will play a role. The examples given by Adam Smith comprise the conditions in a beleaguered city, where prices can rise very high because of extreme scarcity. The gravitation of market prices to natural prices therefore is a matter of anecdotal circumstances. There is no theory of gravitation in Adam Smith, but he has an idea what it is and where it can lead, and we of course, can construct modern models of gravitation.

In doing so, we have many possibilities. The natural price may be realized as an average over many years. This applies in particular to agricultural commodities. Or market prices can be close to natural prices, because charging mark-ups on direct costs such that normal profits are obtained in the long run is a good strategy for producers of industrial commodities. The adaptation of prices can be formalized as a convergent process, if the quantities are rigidly given; an example is the following self explanatory sequence which, if it starts from any prices, will converge to natural prices in terms of the wage rate:

$$\mathbf{p}_{t+1} = (1 + r)\mathbf{A}\mathbf{p}_t + \mathbf{l}$$

There are more sophisticated gravitation models with cross-dual dynamics (the result, whether gravitation obtains, depends on assumptions). One can even regard intertemporal general equilibrium theory as the gravitation process of market prices to prices characterized by a uniform rate of profit, if conditions for the supply of unproduced factors on the one hand and demand conditions on the other are stationary, as I have discussed elsewhere. Sinha is right that Garegnani has made different attempts to formalize gravitation processes, which were not all equally convincing. My conclusion is that gravitation is an axiom of classical theory, not a result, and many and diverse models have been invented to justify this axiom.

His misconception of what gravitation is about leads Sinha to a number of unhelpful statements.

It is true that Smith was not interested in the repercussions of gravitation in one market on gravitation in other markets. But that does not mean that Smith was generally unaware of the interdependence of markets; he was simply arguing, as one later would say, *ceteris paribus*. He was right to be cautious. If one talks about the interdependence of markets, one should not too easily postulate the law of one price. The same commodity can have different prices in different sectors in the real world. A detailed model of the gravitation process then becomes very complex; yet, the tendency remains theoretically and empirically plausible.

Why does Sraffa in *Production of Commodities by Means of Commodities* not need assumptions about returns to scale? One reason is: because the analysis of gravitation is not part of protoeconomics, although it may help to justify the assumptions. Sraffa hints however at some applications where constant returns to scale are the most convenient hypothesis for getting an intuition of what he discusses. An example are his rising supply schedules in the case of intensive rent.

I do not agree that the contrast between natural prices and market prices could not be introduced in the system without a surplus, as Sinha seems to believe, but there is no room to introduce such a special gravitation model here.

I hope we can agree that Sraffa's system is so interesting not the least because of its open character with regard to distribution, the composition of output and accumulation (Schefold 1987, Part Three). His theory is strong and rigorous, as far as the logic of the formation of natural prices is concerned, if distribution is given, but the influences on distribution may be varied and quite different, depending on whether one is dealing with an economy experiencing strong growth or one which is stagnant. The structure of the economy (the protoeconomics) do not suffice to explain how the economy will behave regarding changes of distribution, demand and employment. The question then is how the step from protoeconomics to economics is to be made. Sinha seems to be tempted to simplify too much, when he uses the image of the observer in this context. He believes that mere observation could reveal the protoeconomic structure of an actual economy. Can one not see what factories there are, how many workers there operate, how many consumption goods are being delivered?

But the observer of the real economy will not see homogeneous commodities, for competition regarding the quality of commodities will accompany competition regarding prices. The observer does not see one technique in use in any industry, but usually techniques that rival with each other, and the dominant technique can not easily be identified. Actual production is not based on single product industries, but on a certain degree of joint production in most cases, and often it will be difficult to ascribe outputs and even inputs to specific processes. The observer, in trying to catch the underlying system, will not be able to avoid subjective interpretations, attempts to understand the intentions of producers and the desires of consumers. The bold simplicity of Sraffa's assumptions cannot be defended by the ingenuous postulate of the observer. I think it is one of the best and most original insights in *Production of Commodities by Means of Commodities* that Sraffa characterizes long period positions by an equality of the number of commodities produced and the number of processes used. There are not many economists out there who have understood what a long period position is, and the number of those who have understood this condition is even smaller. It is quite naive to

believe that this condition could be verified in reality in a simple and straightforward manner. And yet I believe that phenomena, by which this law imposes itself, can be observed. I tried to show it using examples of energy production and consumption (Schefold 1985). It is a domain where the observer seems to be confronted with objective data: Household *a* uses gas, household *b* uses electricity for heating, country *A* uses nuclear power for electricity production, country *B* uses wind. But which system will dominate? What is “socially necessary” here depends on a political process, which needs to be *understood*.

We now turn to Sinha’s assertion that the uniformity of the rate of profit can be described as a structural property of the system. In a quite formal sense this is trivial and a matter of definition. But if unequal rates of profit are admitted as a possibility, the question becomes whether a meaningful structural property can be found that imposes itself as a plausible assumption, that would imply uniformity *and* that would be in some sense more fundamental than the recourse to competition.

As we already indicated, Sraffa (1960, 6) introduces “Production with a Surplus” in the second chapter of his book (4<sup>th</sup> paragraph) and says:” The distribution of the surplus must be determined through the same mechanism and at the same time as are the prices of commodities”. And he continues: ”We add the rate of profits (which must be uniform for all industries) as an unknown”. What is the “mechanism” and what does “must” mean? According to the standard interpretation, rejected by Sinha and associated by him with Garegnani, the “mechanism” is competition.

It is clear, however, that there can be homogeneity of commodities, of prices and a structure of an economy producing a surplus such that unequal rates of profit result. Consider the following model of physiocracy, based on a suggestion by Jean Cartelier (1976): There is a physical surplus in both of two sectors. The artisans produce one unit of steel by means of  $\frac{1}{2}$  of steel and  $\frac{1}{4}$  of corn. Farmers produce one unit of corn by means of  $\frac{1}{4}$  of steel and  $\frac{1}{2}$  of corn, so that we get the following table:

	steel	corn
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artisans	1/2	1/4
farmers	1/4	1/2

Diagram 1: A model of physiocracy

If we interpret this table as a Sraffa system, it is clear that we have standard proportions 1:1 and a maximum rate of profit of  $1/3$ . Relative prices are also given 1:1. For the maximum rate of profit, we may write  $R = 1/3$  and  $1 + R = 4/3$ . If  $p$  is the price of steel in terms of corn, we get

$$(1 + R)(1/2p + 1/4) = p$$

$$(1 + R)(1/4p + 1/2) = 1$$

But there is no full scale capitalism, if we interpret the table as a model of physiocracy. The wages of the workers are part of the means of production. The surplus is appropriated not by capitalists, but by the monarchy in the form of a tax (*impôt unique*). We denote the price of steel in terms of corn by  $u$  and the tax rate by  $q$ ; the tax is to be levied in agriculture alone by virtue of the physiocratic doctrine: agriculture is based on nature, nature is productive, the productive sector should be taxed. Hence we have the price system:

$$1/2u + 1/4 = u$$

$$(1 + q)(1/4u + 1/2) = 1$$

Obviously, one obtains  $u = 1/2$  and  $1 + q = 8/5$ , hence  $q = 3/5$ , so that  $q - R = 4/15$ .

This means that, with the tax rate  $q = 3/5$ , the entire surplus is levied in agriculture.  $q - R = 4/15$  means that the tax rate exceeds the rate of profit (if it is uniform) by  $4/15$ . If physiocracy rules, the onus of surplus extraction is shifted to the farmers.

Historically, or, rather, in the history of ideas, one goes from the physiocratic system with the surplus levied in agriculture to the capitalist system with the uniform rate of profit, therefore from the model which we have introduced as the second to the first. Really, *institutions change*, if the surplus is now, other things being equal, appropriated by the capitalists. Marx would have said that they share the booty equally.

Instead of making competition and institutions responsible for the equalization of the rates of profit, Sinha has in his paper a model with several sectors and profit differentials. He wants to argue that profit differentials must disappear thanks to a transformation into a standard system, but here we have, for the model of physiocracy, a standard system from the start, and the differentials persist. That the differentials become zero in Sinha is nothing but an assumption for which he provides a formal reasoning, but he has no economic argument to substantiate his formal procedure; he does not even state his assumption in clear and explicit form. I do not see why his formalism should be better than the simple assumption that the rate of profit is uniform; rather, I think, it is confusing.

I do not deny that there is a deeper motivation. Sraffa, according to various notes in the archive quoted by Sinha, wanted to show that the rate of profit was not a price phenomenon. In a sense, this is obvious from the start, since the rate of profit is expressed in percentage terms, and the percentage can be announced before prices are made explicit. As it is a percentage that is given, it can be given independently of a numéraire. To visualise it, one may refer to the corn model and to the standard commodity as a physical analogue, as was once suggested by Eatwell (1975). The question then is how to express the wage, for the real wage depends on the standard of prices, and so Sraffa left theories of distribution behind which are based on a somehow given real wage. Instead, he advanced the hypothesis that it is the rate of profit, which is “determined from outside the system of production”. He suggested that it might be determined in particular “by the level of the money rates of interest” (Sraffa 1960, p. 33). This has remained controversial. Distribution *does* depend on prices, if the theory of distribution must start from a real wage. Hence there is an ambiguity in the statement that distribution is not a price phenomenon, and I do not feel at ease when the attempt is made to remove this ambiguity by referring to the rate of profit instead. Is distribution primarily determined by the subsistence needs of workers, by effective demand, by the rates of interest or by marginal productivity? This is partly a theoretical, partly an empirical and historical question. To have laid the stress mainly on the rate of profit is for me not Sraffa’s main achievement. By referring to the rate of profit, he posed a problem, but he did not solve it.

Sraffa's protoeconomics remain fascinating. In the end, the question is where we get when the step to economics is made.

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