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Production of Commodities by Means of Commodities in a Mathematical Mode*

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Abstract

The claim in this paper is that Sraffa employed a rigorous logic of mathematical reasoning in his book, *Production of Commodities by Means of Commodities* (*PCC*), in such a mode that all existence proofs were constructive. This is the kind of mathematics that was prevalent at the *beginning* of the 19th century, which was dominated by the concrete, the constructive and the algorithmic. It is, therefore, completely consistent with the economics of the 19th century, which was the fulcrum around which the economics of *PCC* is centred.

Key Words: Existence Proofs, Constructive Mathematics, Algorithmic Mathematics, Mathematical Economics, Standard System.

JEL Classification Codes: B23, B24, B31, B41, B51

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*This paper is a belated dedication to the memory of one of the finest scholars - particularly of Sraffa and the Classical Economists - I have ever had the privilege of knowing: the late *Sukhamoy Chakravarty*.

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1 By Way of a Preamble[1]

> "Besicovitch insists that I publish [*Production of Commodities by Means of Commodities*[2], *[31]*]; the fact that I was able to *foresee interesting mathematical results* shows that there must be something in the theory."
>
> Sraffa's Diary Entry, 31 May, 1958[3]

Sraffa is nowhere, to the best of my knowledge, more specific about these '*interesting mathematical results*'; nor is it made clear what he means by '*there must be something in the theory*' - i.e., what in which theory. Perhaps somewhere buried in Sraffa's voluminous unpublished writings and correspondences (especially with mathematicians and mathematically competent economists) these things are clarified. It would be ironical if the phrase meant 'there must be something in the economic theory in *PCC*' implied by the 'interesting mathematical results', given the almost uniform opinion in the economics profession about *PCC* needing to be fleshed out mathematically to make explicit the economic originality of the book. Especially since the number of people who have made careers out of recasting the economics of PCC into trivial propositions implied by the mathematics of linear algebra and, occasionally, elementary topology, is legion.

Sraffa's contribution to economic theory is a well documented chapter in the history of economic thought. Sraffa may or may not have found it 'immoral to write more than one page per month' ([29],p.43), but every one of those pages are distilled essences of pure economic theoretical elixir – at least in this writer's opinion. I want to suggest that there were also, particularly in *PCC,* but not exclusively in that elegant work, mathematical and methodological elixirs still to be discovered.

Almost thirty years ago I had the opportunity to review Pasinetti's elegant *Lectures on the Theory of Production* ([32]), in which I remarked as follows (p.65):

> "There is a crucial distinction between the methodology followed by Pasinetti and that followed by Sraffa in proving the important propositions. ....
>
> .... The distinction seems to be that Sraffa, whenever he gives an explicit proof, invariably gives us a constructive proof, whereas all the proofs Pasinetti (and almost everyone else who has attempted

[1] This is my fourth explicit attempt (the previous three are: [32], [33], [35]), in the last quarter of a century, to tell a story about possible mathematical and methodological lessons to be inferred from a reading of Sraffa's economic writings. Each time I have had the pleasure and the privilege of advice, inspiration and encouragement from my patient friends, Guglielmo Chiodi and Stefano Zambelli. In more recent times I have also had the benefit of the wisdom of Tom Boylan to guide me through the weird and wonderful world of methodology. The idiosyncrasies, obscurities and infelicities that remain are, alas, entirely my own responsibility.

[2] Henceforth referred to as *PCC* in this paper.

[3] "Besicovitch insiste che io pubblichi[;] il fatto che ho potuto prevedere risultati matematici interessanti mostra che c'è qualcosa nella teoria" (cited and translated in [18],p.193).

> to formalise and generalise Sraffa) gives,follow the method of the formalist mathematicians."

I should now amend the above observation to delete the caveat 'almost' (with half a notable exception, about which I shall comment at the beginning of the next section). When I showed a copy of the review to Pasinetti he responded in a personal conversation[4], in particular referring to the above observation, by telling me the following 'story'. Some time in the early 70s a somewhat mathematical work, focusing on formalising *PCC*, was submitted for consideration as a Fellowship dissertation at King's College, Cambridge. The College asked Alister Watson to act as an external examiner, who wrote a negative report to the effect that the work had nothing to do with *PCC*! I do not know whether Watson meant that the submitted work had 'nothing to do with' the economics, the mathematics, or the mathematical economics of *PCC*.

From a purely mathematical point of view, *PCC lacks nothing.* This is the point of my paper. The concerns in PCC are the solvability of equations systems and, whenever, existence or uniqueness proofs are considered, they are either spelled out in completeness, albeit from a non-formal, non-classical, point of view or detailed hints are given, usually in the form of examples, to complete the necessary proofs in required generalities. Pure laziness, inertia and ignorance of alternative traditions in mathematical philosophy have caused untold mischief and created an industry of re-casting and distorting *PCC*, a work of aesthetic purity and mathematical elegance, into a trivial application, to a large extent, of linear algebra.

In this brief paper, then, I simply want to concentrate on *two issues* that have caused unusual and unfortunate misunderstandings in the reading and understanding of *PCC*, by so-called mathematical economists and economists with a mathematical bent, almost all of whom are hopelessly incompetent in mathematical philosophy and almost equally hopelessly ignorant about the existence (sic!) of alternatives to classical mathematics[5]. The two issues are *rigour* and *proof* in *PCC.* The elementary misunderstandings by these so-called mathematical economists have led to quite incredible assertions about the mathematical content and validity of the formal propositions in *PCC*, and their proofs, particularly the *existence proofs.* I should like to add that even some of the mathematically competent - albeit only in classical mathematics and its underpinning mathematical logic[6] - economists who are widely known to be sympathetic to

[4] The gist of that conversation was repeated by Pasinetti when he and I were invited for lunch at Jesus College by Geoff Harcourt in Spring, 2001. I understand that Alister Watson's report lies buried among inaccessible such documents in the 'vaults' of the King's College library. Some day, some assiduous graduate student, would no doubt write his or her doctoral dissertation on the basis of having excavated this report. My point, however, is that none should be surprised by Watson's perceptive evaluation. PCC is complete in its mathematics; it is just that it is not the kind of way mathematics is usually employed in economics texts.

[5] By 'classical mathematics' I am referring to the mathematics of real analysis underpinned by set theory plus the axiom of choice.

[6] Mathematical logic, as distinct from the logic of mathematics, is generally understood, these days, to consist of set theory, proof theory, model theory and recursion theory. I do

their visions of Sraffian Economics, have made incorrect assertions and unnecessary formalizations that have had the effect of diverting attention from the more basic economics in *PCC*.

I shall, however, concentrate almost exclusively on the issues of rigour and proof in PCC and shall refrain from making any comments or interpretations on the economics of PCC. The economic implications of the methods of proof used by Sraffa in PCC for the economics is quite a separate issue, with which I hope to struggle on another occasion.

The paper is, therefore, structured as follows. In the next section I discuss a representative sample of the incompetent and irrelevant - not to mention the 'irreverent' - assertions made about rigour, proof and the mathematics of Sraffa in PCC. However, mercifully, there have been distinguished economists, not quite known to be mathematical economists or even mathematically minded ones, who gleaned immediately that PCC was impeccable in its mathematical rigour and, to a lesser extent, also aware - however dimly - that the methods of proof employed by Sraffa were sound, even if 'unconventional'. The foremost examples of the former class are Richard Quandt ([25]), Edwin Burmeister ([3]) and Frank Hahn ([16]); a good sample of the latter set consists, at least, of Peter Newman, Harry Johnson, Melvin Reder and, above all, Sukhamoy Chakravarty. Somewhere in between are some of the mathematically competent so-called 'Neo-Ricardians'.

I go on,then, in §3 to suggest an alternative mathematical reading of PCC which exposes the errors of representation discussed in the previous section and, hopefully, makes clear the impeccable rigour with which the propositions of PCC have been demonstrated by Sraffa, particularly the existence proofs.

Finally, in §4, I try to derive broad methodological precepts, particularly from the point of view of a mathematical philosophy, for economic theory, from the exercises attempted in §2 and §3.

I would like to add a brief explanatory note, informed by reading two brilliant works of unusual nature, on a single topic. One by Richard Feynman and the other by Subhramnayan Chandrasekhar, both on re-reading and re-doing the *mathematics* of Newton's *Principia.* Richard Feynman, when asked to give a guest lecture to the Freshman class at Caltech, in March, 1964, decided to prove Kepler's law of ellipses 'using *no mathematics* more advanced than elementary geometry' ([13], p. 18; italics added):

> "Why did Feynman undertake to prove Kepler's law of ellipses using only plane geometry? The job is more easily done using more the powerful techniques of more advanced mathematics. Feynman was evidently intrigued by the fact that Isaac Newton, who had

not know of a single mathematical economist or an economist with 'official' competence in mathematics – whether sympathetic or hostile to the message in *PCC* - who has tried to read the mathematical reasoning in *PCC* in any other way than in terms of classical mathematics and set theory (plus the axiom of choice) - i.e., ZFC, as it is routinely referred to in mathematical circles. ZFC, in turn, refers to the Zermelo-Fraenkel axiom system of set theory plus the axiom of choice.

invented some of those more advanced techniques himself, nevertheless presented his own proof of Kepler's law in the *Principia* using only plane geometry. Feynman tried to follow Newton's proof, but he couldn't get past a certain point, because Newton made use of arcane properties of conic sections (a hot topic in Newton's time) that Feynman didn't know. So,.... Feynman cooked up a proof of his own.

.....

Isaac Newton invented the differential and integral calculus. There is little doubt that he used these powerful analytical tools to make his great discoveries. ..... [However] *the Principia is presented in the classical languages of Latin and Euclidean geometry.* The reason is obvious enough. *Newton had to speak to his contemporaries in a language they would understand.*"

ibid; p. 19 & 44; last two sets of italics, added.

Sraffa wished to speak to the majority of his economic contemporaries, many of whom were not versed in the advanced mathematics underpinning 'Perron-Frobenius theorems'. He presented his work in the 'classical languages of English and the Higher Arithmetic'. It is a pity that the mathematical economists had become immune to the aesthetic elegance of good prose and the deep beauty of the Higher Arithmetic.

Chandrasekhar[7], in his monumental re-reading of the *Principia* ([6]), without any recourse to secondary sources, undertook to redo Newton's proofs with modern mathematics, but acknowledged that 'the manner of his study of the *Principia* was to':

> "[R]ead the enunciations of the different propositions, construct proofs for them independently *ab initio*, and then carefully follow Newton's own demonstrations. In the presentation of the propositions, the proofs that I constructed (which cannot substantially differ from what any other serious student can construct) often precede Newton's proofs arranged in a linear sequence of equations and arguments, avoiding the need to unravel the necessarily convoluted style that Newton had to adopt in writing his geometrical relations and mathematical equations in connected prose. With the impediments of language and of syntax thus eliminated, *the physical insight and mathematical craftsmanship that invariably illuminate Newton's proofs* come sharply into focus."
>
> ibid, p. xxiii (Prologue); italics added.

[7] Lest the unlikely mathematical economist reader of this paper gets carried away and reads into Chandrasekhar's mathematical approach a justification for what he or she has done with *PCC*, let me also add the following wonderful caveat in [6], p.44 (italics added):

> "This simple notational device (suggested by Tristan Needham) allows us 'to draw on the intuitive power of infinitesimal geometry while continuing to pay lip service to *the tyrannical legacy of Cauchy and Weierstrass*'. "

'The economic insight and mathematical craftsmanship that invariably illuminate Sraffa's proofs', on the other hand, have been distorted and obscured by the attempts to re-mathematise *PCC* by economists without the deep knowledge and wide vision one needs to have of economics and mathematics. Chandrasekhar had these, for physics and mathematics, together with a deep understanding of the mode of reasoning of the classics, not only in physics.

2 Clarity and Confusion in Interpreting the Mathematical Underpinnings of *PCC*

> "Hardy was right after all: mathematicians are out to *debunk the fakery that lies concealed underneath every logically correct proof.* But they will not admit that their task is one of debunking; they will instead pretend that they are busily proving new theorems and stating new conjectures in compliance with the canons of present-day logic. ....
>
> *[M]athematical proofs come in different kinds*, that need to be classified. The notion of understanding, that is used in informal discussion but quashed in formal presentation, will have to be given a place in the sun; what is worse, *our logic will have to be accommodated to admit degrees of understanding.*
>
> Rota ([28], p.195; italics added)

This paper is dedicated to the memory of Sukhamoy Chakravarty for many reasons, but primarily because I had some of my most fruitful and enlightening discussions on Sraffa's contribution to economic theory with that erudite man, not long before his untimely death. When we began talking about Sraffa, mostly about the methods of proof used in PCC, Chakravarty's view on it was expressed in his early **Arthanithi** review:

> "We come now to the 'piece de resistance' of the book: the construction of the so-called standard system and the proof of its uniqueness. Here while the skill of the literary exposition is to be admired, nonetheless the roundaboutness of proofs (*if we can call such discussion proof*), is hardly a factor conducive to clear understanding. Restatement of Mr Sraffa's problem in terms of inter-industry analysis shows how the proof of the existence and uniqueness of such a "standard system" follows from the well-known theorem of Perron and Frobenius[8] in connection with non-negative square matrices."

[8] In view of what I think is the insidious role this theorem has played in distorting the mathematics of PCC, I would like to take this opportunity to correct an egregious mistake on the origins of the use of this theorem in economics. It was introduced to the mathematical economics literature by Richard Goodwin in the early 50s during a slightly acrimonious debate with John Chipman in the pages of the Economic Journal ([14]). I documented this story in [34], pp. 78-0, particularly footnote 12. In spite of Goodwin's own clear statements on the source for his own knowledge of this theorem (which was *Göran Ohlin*, who had been

Chakravarty ([4], p.8[9]; italics added)

In 1980 and 1981 I had long discussions with Sukhamoy Chakravarty on the nature of proofs in PCC. I pointed out to him, as I had done to my own *maestro*, Richard Goodwin, in the same context, and referring to the irrelevance and, indeed, the dangers of formalizing the propositions of the first part of *PCC* using 'Perron-Frobenius Theorems'. I did not get too far in convincing Richard Goodwin because he claimed incompetence in understanding the meaning of proofs; however, Chakravarty listened patiently to the case I was making and promised to re-think his interpretation of the method of proofs used in *PCC*. A quarter of a century later, in a review of yet another 'Sraffian' book, Chakravarty went at least half-way towards my interpretation of the nature of proofs in PCC:

> "Sraffa's austere prose of *Production of Commodities by means of Commodities* can prove more daunting to most students of economics than the use of matrix algebra. In recent years, an increasing number of textbooks have, therefore, made liberal use of the basic tools of linear algebra, including some results on non-negative square matrices to derive the analytical results which *Sraffa largely demonstrates constructively with the help of English prose*."
>
> Chakravarty ([5], p.122; second set of italics, added)

Apart from my own interpretation of the nature of proofs in *PCC* as *constructive*, this is the only other mention of such a possibility in the entire literature on that elegant and rigorous piece of 'austere prose' (with another, well-meaning, albeit technically incorrect exception in recent years, to be mentioned below), that I am aware of. For the rest, the profession simply recast the economics of *PCC* in the mathematics of linear algebra and proceeded to assure itself, as in the gratuitous words of a leading exponent of this genre, Frank Hahn:

> "Sraffa's book contains no formal propositions which I consider to be wrong ....."[10]

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a student in one of Goodwin's classes at Harvard in 1949), Desai and Ormerod state ([12], p.1433), incorrectly:

> "This ([14]) was also the article which first cited the Frobenius Theorem, first suggested to him, as Goodwin acknowledged, by a young student called Robert Solow."

[9] I am in the privileged position of having received a reprint of this rare review from Chakravarty himself. However, the pagination in the reprint is obviously not that of the published version.

[10] The completion of the sentence reads: 'although here and there it contains remarks which I think to be false'. (ibid, p.353). This is, in my opinion, a preposterous statement about a rigorous book, where there is not a single categorical statement - as remarks or in any other form whatsoever - is asserted without rock solid logical underpinnings. There are, of course, suggestions, with impeccable caveats - the prime example being the famous one to end the penultimate paragraph of p.33 in PCC:

The simplest of examples of how he and legions of others satisfied themselves that *PCC* 'contains no formal propositions [that they] consider to be wrong' can be given by taking one of Hahn's own renderings of a 'formal proposition', ostensibly from *PCC*. According to Hahn's reading of *PCC*, Sraffa in *PCC*, when *constructing* the standard system, is looking for a positive vector $\mathbf{x}^*$ and a (positive) scalar G^* such that the following vector-matrix equation is satisfied(op.cit, p.355)[11]:

$$x^* = G^* A x^* \tag{1}$$

where $n \times n$ matrix A consists of elements $a_{ij} > 0, i, j = 1, ..., n$

It is at this point that the usual 'distortion' and misreading of *PCC* enters the fray with a vengeance. Having formulated the problem of the construction of the standard system as one of finding particular eigenvalues and eigenvectors of a system of linear equations, Hahn goes on to claim, with a flourish that is almost dismissive (ibid):

> "We now have a purely mathematical problem for which there is a standard mathematical result. ... The [vector x^*] is a pure construct as of course is [1] used in its derivation."

He even helps the reader by referring to the (incorrect) appendix in his own book (written jointly with Arrow) for the 'standard mathematical result'. He does not, of course, tell us in the article or in the appendix of the book with Arrow, what assumptions were needed to prove the mathematical result he invokes. Nor does he add any caveat on the care with which *PCC* avoids any matrix formalizations. Above all, he does not warn the reader that (1) is not used in the derivation of the construction of x^* in *PCC*.

To be more precise, we are not informed, either in the above article by Hahn or in the book with Arrow to which he refers for 'the mathematical result', of the assumptions, frameworks and the methods of proof used in the derivation of those results. Perhaps they were derived by hand-waving, appeal to ESPs, or undecidable disjunctions! In fact, the Perron-Frobenius theorems are generally proved by an appeal to the Brouwer fixed point theorem where, at a crucial stage of its proof, appeal is made to the Bolzano-Weierstrass theorem, which is provably impossible to constructivise. Whether Sraffa was aware of this particular infelicity in deriving the 'mathematical result' which Hahn and others wave with a flourish whenever they mention the standard system and its construction is not the issue. The point really is that uncritical appeal to standard mathematical results means the mathematical and logical baggage underpinning it comes with it and could make a mockery of the economic rationale for the result and, most importantly, for the way its validity is demonstrated - i.e., proved.

> "The rate of profit, as a ratio, has a significance which is independent of any prices, and can well be '*given*' *before* the prices are fixed. It is accordingly *susceptible* of being determined from outside the system of production, in particular by the level of the money rates of interest." (italics added)

[11] Not all of the assumptions in Hahn's rendering are faithful to the economics of PCC; but let that pass.

Richard Quandt, in a technically inelegant and economically ungenerous review of *PCC* (op.cit), is slightly more explicit about appealing to the Brouwer fixed-point theorem - so beloved of the mathematical economists and the game theorists, but the curse of the constructivists and the intuitionists, with Brouwer himself leading the curse from the front[12]:

> "The existence of positive prices and the uniqueness of the standard system is proved. One feels that the existence proof would, under somewhat different assumptions, be amenable to a fixed point argument. In particular, if the price vector were required to be non-negative only, the Brouwer Fixed Point Theorem might be utilized."
>
> Quandt ([25], p.500)

One cannot help wondering why, if 'existence .. and uniqueness of the standard system is proved', there is any need to make 'different assumptions' just so as to make it possible to use 'a fixed point argument'? Was *PCC* an exercise in teaching or exhibiting the use of alternative 'mathematical results' and 'theorems'? For that purpose one can turn to the great and good mathematics texts themselves. Moreover, even 'if the price vector were required to be non-negative', it is entirely feasible to prove its existence by means of wholly constructive methods, without any invoking of the intrinsically non-constructive Brouwer Fixed Point Theorem.

Burmeister (op.cit) traverses the same worn out path, a little more explicitly than Hahn and Quandt - and a thousand others - so that it might be useful to have him state his case, too:

> "In Production of Commodities by Means of Commodities Mr Sraffa demonstrates that there exists a 'Standard System'.... . [A]pparently it is not widely recognized that the proposition can be easily established from well-known theorems in linear algebra. Here a straightforward proof is given; it circumvents much of Mr Sraffa's discussion in chapters III, IV and V, and hopefully will be enlightening to the mathematical economist."
>
> Burmeister (op.cit, p. 83)

Well! Obviously Professor Burmeister is a bit late to arrive at the feast! Not even a few weeks had elapsed after the official publication of *PCC* when

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[12] Obviously Professor Quandt does not realise that any appeal to the standard version of the Brouwer Fixed Point Theorem means also an appeal to the *Bolzano-Weierstrass Theorem*. This latter theorem, because of its intrinsic reliance on *undecidable disjunctions*, cannot be *constructified* by anything less than pure magic - a fact recognized by Brouwer quite soon after he had enunciated it and, therefore, rejected it. More than forty years after his first, classical, demonstration of the famous theorem that bears his name, Brouwer finally gave an intuitionistic proof of it. However, he did not forget to add an important remark to that 2-page paper ([2], p.1):

> "[T]he validity of the Bolzano-Weierstrass theorem [in intuitionism] would make the classical and the intuitionist form of fixed-point theorems equivalent."

Chakravarty's measured review article elegantly demonstrated the way 'well-known theorems of linear algebra' could be applied to re-read the book in the way that lazy mathematical economists could and would. Seven years later we have the same exercise repeated and published in an ostensibly prestigious Journal. But more importantly, what was the advantage in 'circumventing Mr Sraffa's discussion in chapters III, IV and V'? And how will it be 'enlightening to the mathematical economist' to establish the same propositions demonstrated by a faultless and innovative logic of mathematics by Sraffa 'from well-known theorems of linear algebra'? Surely, a competent mathematical economist would be curious to learn new methods of proof rather than simply rehash 'well-known theorems in linear algebra'? Or is Professor Burmeister suggesting that the economic propositions in *PCC* are so important and innovative that establishing them - of course without violating the assumptions in *PCC* - with the more familiar mathematics of the mathematical economist might serve a higher purpose? But that, too, will not make sense - because the economics of *PCC* is inextricably intertwined with the mathematical methods devised for proving the propositions on existence and uniqueness and 'circumventing' the three mentioned chapters would be like removing the good Prince of Denmark from that tragic drama played out in Elsinore. Finally, it is possible that Professor Burmeister himself did 'circumvent' the three chapters he mentions because, otherwise, he would not make the senseless statement with which he concludes his pointless paper (p. 87):

> "Unless it is assumed that the economy exhibits constant returns to scale with the matrix of input coefficients $[a_0/a]$ fixed, then the above analysis is meaningless if even a single quantity X_j changes."

If Professor Burmeister is referring to his own analysis when he states 'the above analysis', then he is eminently accurate; if not, he will have to go back and de-circumvent his reading of PCC to understand the nature of the purely auxiliary construction in it and why any assumption about returns to scale is completely irrelevant[13] for the constructions and proofs elegantly effected in 'chapters III, IV and V'.

[13] He may, alternatively, first read Reder's altogether more competent and sympathetic review of PCC as a refreshing introduction to the relevant circumvented chapters, before embarking upon a full-scale de-circumventing (Reder, [26], p.694):

> As noted at the outset, Sraffa explicitly denies that he is assuming constant returns to scale. At first blush this seems utterly inconsistent with the scalar expansions and contractions of processes required to construct the standard commodity. However, it is not necessary that these operations be carried out; it is necessary only that they can be defined so that for any given state of productive technique there will be one and only one standard commodity. If there should be increasing or decreasing returns to scale, this would mean only that the state of technique ... varies with the level of output. Whether it does so is irrelevant to Sraffa's argument, which is concerned only with explaining the consequences of technical change (i.e., of changes in input coefficients per unit of output), but not its 'causes'

Finally, let me end where I began this section, in the sense of considering a particular sympathetic interpretation of a method of proof given in *PCC* but, unfortunately gets derailed due to insufficient attention to the strictures of alternative mathematics, particularly the mathematics of algorithms, i.e., constructive and computable analysis. Kurz and Salvadori ([19]) discuss, in admirable detail, the discussion between Sraffa and Alister Watson regarding, in particular, the algorithm proposed by the former, in §37 of *PCC* to construct the standard commodity. They point out that Watson had some doubts about the feasibility of the algorithm but that Sraffa did not share the doubts. They then go on to suggest a formalization of an 'algorithm' (ibid, p. 206), claiming it to be the one suggested by Sraffa. However, neither Watson's doubt, as correctly perceived by Sraffa, nor the suggested Kurz-Salvadori 'algorithm' are quite pertinent from the point of view of constructing the standard system along the lines described in *PCC*. Sraffa outlines two steps to be alternatively implemented to construct the standard system. Watson is supposed to have had doubts about the feasibility of the first step, not its algorithmic formulation, at least if one reads and interprets the Watson statement literally, which is:

> "It isn't quite obvious that the first type of step can always be carried out."
>
> ibid, p.206

This is, apparently, the fourth of eleven queries stated in a list accompanying a letter from Watson to Sraffa dated 17 November, 1959. Sraffa does not seem to have had any doubts - quite correctly in my opinion - regarding the feasibility of carrying out the first of the two steps of his proposed procedure. The formalization suggested by Kurz and Salvadori expresses the first step with an *existential quantifier* (ibid, p. 206):

[i.0] There are $q_{i-1}^T \geqslant 0^T$ and $\lambda_{i-1} \geqslant 0$ such that $q_{i-1}^T[\lambda_{i-1}I - A] \geqslant 0^T$

This is a meaningless step as an algorithm for a computer - digital, analog or hybrid. Moreover, this is not the way the first step is stated in PCC. If this is also what Watson meant with the first step, which I doubt, then obviously it not only may not be possible 'always to be carried out' on a computer; it can *never* be carried out on a computer. Watson's query must, therefore, have to do with the fact that he had forgotten the notion of viability defined in *PCC* (footnote, p.5; cf. also Chiodi ([7], [8])[14].

More importantly, the claim by Kurz and Salvadori that their alleged algorithm generates a sequence that converges is incorrect in computable analysis, i.e., in the analysis that is relevant for a digital computer in which their algorithm is, ostensibly, to be implemented:

[14] It is very easy to implement the first step in the two-step alternating procedure specified by Sraffa, in §37 of *PCC*. I have indicated, using the example in Chapter IV (*pace* Burmeister!) of *PCC*, the nature of the algorithmic rule to be specified and implemented by a digital computer ([35], esp. pp. 7-8).

"Since the sequence $[\lambda_i]$ is decreasing and bounded from below ($\lambda_i > 0$), it converges to the requested solution."

ibid, p.206

Not only is it an unnecessary appeal to an irrelevant theorem; it is also invalid in computable analysis. Many years ago Ernst Specker proved the following important theorem in computable analysis ([30]):

> There is a strictly monotone increasing (decreasing) and bounded sequence β_n that does *not* converge to a limit.

This is the kind of danger inherent in being wedded to one kind of mathematics - that of classical, real analysis - while reading a rigorous text which has been written without any appeal to the logic of that kind of mathematics.

3 The Rigorous Mathematical Economics of *PCC*

> "The *methods* of mathematics are to be given by laying down the canons of definitions and of argument that govern the introduction of new concepts and the construction of proofs. This amounts to specifying the *logic of mathematics*, which we must take care to distinguish from *mathematical logic*: mathematical logic is a particular branch of mathematics, whereas the logic of mathematics governs all mathematical reasoning, including reasoning about the formal languages of mathematical logic and their interpretations. T*he logic of mathematics cannot be purely formal*, since the propositions to which it applies have fixed meanings *and the proofs it sanctions are meaningful arguments, not just formal assemblages of signs*."
>
> Mayberry ([22], p.12; last two sets of italics, added)

I doubt I shall be saying something very controversial if I state that the most competent - and, without doubt also the most sympathetic - of the 'first generation' of reviews of PCC was the elegant one by Peter Newman ([24]). Even Newman's sympathetic and competent review could not avoid referring to PCC as '*mathematically incomplete*' (ibid, p. 52), without, however, specifying in what sense, how or where the book was deficient in that respect. However, he did lay his cards, open faced, on the table (ibid, p. 59):

> "[T]he most useful function that this critique can serve is to *translate* [*PCC*] into the more widely used Walrasian dialect of mathematical economics, and to give proofs of his main results which are acceptable to the speakers of that dialect. Translated into this more common *argot*, his system may become less opaque, although perhaps - *as in good poetry* - *there are subtleties which defy translation*; ... "
>
> Last set of italics, added.

Not only are there 'subtleties that defy translation'; there will be distortions that deny the readers of a translation an appreciation of the full message of an original. Imagine prose or poetry composed in a language routinely using the subjunctive - as in modern Italian - and translating any work from that language to one that does not use such a grammatical case any more (like English). It will then be easy, for someone reasonably competent in both languages, to understand the kind of calisthenics required to translate from the Italian to English (not necessarily *vice versa*, which should *not* require equivalent calisthenics). Any theorem in constructive mathematics is valid in classical mathematics; but not *vice versa*. Any practitioner of constructive mathematics eschews the use of *tertium non datur*; not so in classical mathematics. This is why Fred Richman noted, almost with exasperation:

> It is a lot harder than one might think to recognize when a theorem depends on a nonconstructive argument. One reason is that proofs are rarely self-contained, but depend on other theorems whose proofs depend on still other theorems. These other theorems have often been internalized to such an extent that we are not aware whether or not nonconstructive arguments have been used, or must be used, in their proofs. Another reason is that the law of excluded middle [LEM] is so ingrained in our thinking that we do not distinguish between different formulations of a theorem that are trivially equivalent given LEM, although one formulation may have a constructive proof and the other not." ([27], p.125)

The mathematics of *PCC* is about formulating economic problems in the form of equations and finding methods to solve them. Where it is necessary to supplement the information about solutions with general statements of validity, i.e., theorems in the standard sense of the word, then, invariably, constructive proofs are given; or examples are worked out from which a diligent economist can extract a general pattern for an algorithm to make it a theorem. It is a classic text in methods of problem solving in the tradition of a Polya or a Simon, particularly *Human Problem Solving*. I shall illustrate this approach in *PCC* with just one example[15]; the one of 'reduction to dated quantities of labour', in single- and multiple-product systems. Sraffa, in *PCC*, devised the method of sub-systems for this purpose (Appendix A in *PCC*). Anyone seriously interested in using this method for reducing a system to dated quantities of labour is given enough - and just enough only - information on how to proceed to construct a sub-system for such a purpose. All that we are given is the following (*PCC*, p. 89; italics added[16]):

> "Consider a system of industries .. which is in a self-replacing state.

[15] Since I have dealt with the algorithmic interpretation of the proof of the existence of the standard system in [35]. I shall, however, have something to say about 'proof by contradiction', below.

[16] The reader would be well-advised to keep in mind the contents of Chiodi's important paper ([8]) on 'self-replacement'.

> The commodities forming the gross product ... can be unambiguously distinguished as those which go to replace the means of production and those which together form the net product of the system. Such a system can be subdivided into as many parts as there are commodities in its net product, in such a way that each part forms a smaller self-replacing system the net product of which consists of only one kind of commodity. These parts we shall call sub-systems.
>
> This involves subdividing each of the industries of the original system ... into parts of such size as will ensure self-replacement for each sub-system."

The assumptions are clearly stated - i.e., those that are underpinned by the statement 'a system of industries in a self-replacing state'. The nature of the problem is unambiguously stated, too. The procedure to be adopted is outlined in broad brush strokes - after all the book is not a manual for a Montessori School. The key to the procedure is an effective interpretation of the sentence: 'subdividing each of the industries of the original system into parts of such size as will ensure self-replacement for each sub-system'. *That is all - or almost!* The careful (and sympathetic) reader will then remember that there is, after all, a worked out example of a difficult special case from which to extract the exact algorithmic pattern: the example of §81 (pp. 68-9, *PCC*). The rest is up to the interested reader, long ago defined as 'that elusive character' by John Kelly. This is, after all, the procedure adopted in one of the classic texts in *Constructive Analysis* ([1]): broad hints for proving theorems constructively are given, on the basis of clearly stated assumptions, but it is also assumed that the logic of reasoning adopted will be that of the logic of mathematics (not necessarily that of mathematical logic).

There are, however, occasional appeals to '*proof by contradiction*', usually eschewed by the constructive mathematician, but not by the computable analyst. There are crucial differences between the constructive and the computable mathematicians, but I shall not enter into details of this arcane characterization. Suffice it to say that the computable mathematicians are not disturbed by using the device of 'proof by contradiction', especially to demonstrate universal propositions. In *PCC*, for example, this device is used in §42, ff., to demonstrate the proposition that 'the value of R to which correspond all-positive prices .. is the *lowest* of the k possible values of R.' The classical mathematical economist would, of course, have recourse to the Perron-Frobenius root and that is that. Here, in *PCC*, the proof of existence and uniqueness of the standard system have both been given in impeccable constructive mode. The auxiliary *proof by contradiction* of the determination of the appropriate vale of R is a consequence, in particular, of the uniqueness part of the earlier constructive proof[17].

[17] As clearly stated in *PCC* (§42; first set of italics added):

> "It can be seen, *as an immediate consequence of the above* [i.e., the uniqueness part of the proof of the existence of the standard system] that the value of R to which correspond all-positive prices .. is the l*owest* of the k possible values

The trouble with a proof by contradiction is that it is indirect and, somewhere in its recesses, there are appeals to a double-negation, which, in infinitary cases is rejected by strict constructivists. The conundrum is beautifully described as follows:

> "In indirect proofs [such as those employing 'proof by contradiction'], however, something strange happens to [the] 'reality' of the [constructed] objects. We begin the proof with a declaration that we are about to enter a false, impossible, world, and all our subsequent efforts are directed towards 'destroying' this world, proving it is indeed false and impossible. We are thus involved in an act of *mathematical destruction*, not construction. ...
>
> What have we really proved in the end? What about the beautiful construction we built while living for a while in this false world? Are we to discard them completely? And what about the mental reality we have temporarily created? I think this is one source of frustration .. .
>
> Actually, there is a way to alleviate the frustration. ... . It is based on the observation that in many indirect proofs, the main construction is independent of the negative assumption. You can therefore *separate out the construction from the negative assumption*, making it a positive act preceding the main (negative) argument."
>
> [21], pp. 323-4; italics in the original.

This is precisely the way to read the few indirect proofs in *PCC*. In the particular case of finding a rule for determining the relevant value of R the 'negative' part of the 'proof by contradiction' can be easily and felicitously separated from the subsequent positive, constructive, world created. Such a separation is absent in any blind invoking of the Perron-Frobenius apparatus, to which all and sundry resort in classic 'line of least resistance' fashion. *PCC* is not a text for the mathematically blind and mechanical; it is meant for the thoughtful mathematically minded economist who is adept at the logic of mathematical reasoning, even if not competent in mathematical logic and, especially, if not trained in classical (or any other kind of) mathematics.

4 Lessons for a Mathematical Philosophy of Economics

> "*Student*: The car has a speed of 50 miles an hour. What does that mean?
>
> *Teacher*: Given any $\epsilon > 0$, there exists a δ such that if $|t_2 - t_1| < \delta$, then $\left|\frac{s_2 - s_1}{t_2 - t_1} - 50\right| < \epsilon$.

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of R."

Student: How in the world did anybody ever think of such an answer?
Grabiner ([15], p.185)

I have refrained from entering into the various debates on the economics of *PCC*. However, I should like to point out one neglected aspect of the richness of *PCC*. It is entirely feasible to interpret the construction of the standard system as an attempt to device an ìdeal- index number. In fact, it is remarkable that the famous index number constructed by Doris Ikle ([20]) does exactly what Sraffa set out to achieve with the standard system. An explanation of this observation will require a complete paper in itself and I must leave it at that, hopefully for this author to return to the theme at a later stage or, even better, if someone else takes up the hint and works out the analogies and ramifications.

Imagine, now, a world of economists, none of whom were trained in any kind of mathematics, but all of whom are perfectly wise in the sense of possessing reasoning faculties. If to this world some enlightened being introduced *PCC*, how would it be read and interpreted? In this imaginary world, let us add to the indulgence and assume also that these perfect reasoning entities, if you like *ideal computing machines*, are also equipped with the mathematics of the digital computer - and no other mathematics. How would they, then, read and interpret the proofs, conjectures and problems enunciated in *PCC*?

Such is the counter-factual or, perhaps, the *gedankenexperiment* I have tried to carry out in the preceding pages (and in my many readings of *PCC*). I came to the conclusion, albeit gradually, that the propositions and reasonings in *PCC* were impeccably rigorous and the existence proofs were invariably constructive, even when occasionally side-tracked by the indirect proofs that were appended to the main propositions.

This is in complete contrast to any and every other mathematical economics text in existence today - naturally, to the best of my knowledge. I have long been of the opinion that the immortal Dickens, in that wonderful story of *Great Expectations* accurately encapsulated the activities of the mathematical economist when he observed:

> "They took up several wrong people, and they ran their heads very hard against wrong ideas and persisted in trying to fit the circumstances to the ideas, instead of trying to extract ideas from circumstances. "

For over a century and a half the mathematical economists, first as mathematically competent economists, have been 'trying to fit' mathematical results and concepts to economic concepts, instead of trying to extract, using the logic of mathematical reasoning, economic ideas 'from circumstances'. This is no better illustrated than in the attempts made by the doyen of mathematical economics, Gerard Debreu, in a series of recent papers ([9], [10], [11] to make the case that the development of economic theory is simply achieved by applying developing mathematical ideas. It is inconceivable for such people, and they are the majority of mathematical economists, that an economic theory that is intensely mathematical can be developed without appealing to a single mathematical result but employing an eminently reasonable logic of mathematical reasoning. The problem, of course, is that the mathematical economists and the mathematically competent economists have no idea of the difference between mathematical logic and the logic of mathematical reasoning.

As for rigour, no one has ever questioned the impeccable rigour of *PCC*. Melvin Reder, in particular, and Harry Johnson, too, in their early, appreciative, but perplexed reviews, were handsome in paying full tribute to the impeccable rigour displayed in *PCC*. When referring to the existence and uniqueness proof of the standard system, the former stated ([26],p. 691; italics added):

> "The logical structure of this part of the argument is exceptionally tight, even for this volume, and further condensation would make for obscurity. Suffice it to say, *I find the argument valid in its essentials.*"

Clearly, Reder has understood the 'logic of mathematical reasoning' employed in *PCC* and appreciates it. If the argument is valid in all its essentials and the logical structure is exceptionally tight, why do we require any other mathematical formalism to understand it quantitatively? Have economists forgotten the art of reading English (or Italian, French, German, Spanish, Japanese - the obvious world languages into which PCC has been translated, I presume) prose, supplemented with elementary arithmetic, formulations in terms of simultaneous equations and a challenge to find methods to solve them in senses that are economically meaningful?

Johnson, too, was unreserved in his acknowledgement of the rigour of *PCC* and refers to it as:

> "[This] extremely elegant and rigorous analysis."
>
> Johnson ([17], p.3).

Can there be a *rigorous* logical structures, employing valid arguments in its essentials, that cannot be mathematized conventionally? We know, from the tortuous history of the infinitesimal, the Dirac delta function, the Feynman diagrams, and several other famous examples, that the answer is in the affirmative. These famous concepts have had to wait for conventional mathematics to be broadened to encapsulate such rich conceptual structures. Conversely, even eminent mathematicians, the notable and tragic example of von Neumann is foremost in the case of dismissing the Dirac delta function and the de Broglie-Bohm approach to Quantum Mechanics, have been entrapped in their narrow mathematics to such an extent that they devised alternative theories to avoid what they thought were mathematically unrigorous concepts.

The problem is that conventional mathematicians associate the notion of rigour with one kind of mathematics or with one kind of mathematical logic. That there is no accepted formal notion of rigour is something that is alien for these practitioners of orthodox mathematics and narrow mathematical logic. *A fortiori*, for the notion of proof.

By example and explanation I have tried to show that PCC is an intensely mathematical text, tight in its logical reasoning, rigorous in its mathematical demonstrations and unorthodox in the nature of the mathematical formulation of its economic problems. All who have read PCC also know that most of the explicit references are to texts from the *19th century*. It is, therefore, appropriate I end with an allusion to the kind of mathematics in PCC with its link to that noble century:

> "As the nineteenth century began, virtually all mathematical research was of the *concrete*, *constructive*, *algorithmic* character. By the end of the nineteenth century much abstract, *non-constructive*, *non-algorithmic* mathematics was under development. What happened, how did it happen, and why?"
>
> Metakides and Nerode ([23], p. 319)

Mercifully, as the 21st century dawns, as a consequence of the ubiquity of the digital computer, we are reverting to the mathematics of the beginning of the 19th century. *PCC*, in my opinion, was written in the spirit of the mathematics of the beginning of the 19th century. It has been read and misinterpreted by a 20th century audience unfamiliar with early 19th century mathematics that was of a 'concrete, constructive and algorithmic character'. Needless to say, *PCC* was also written from the perspective of the economics of the 19th century, with which the 20th century has also been alienated. The hope is that the mathematics of the 21st century may inspire the young economists who will be competent in it to go back also to the magnificent dynamics of the economics of the early 19th century. *PCC* will be the bridge in both senses.

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