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SEVEN KINDS OF COMPUTABLE AND CONSTRUCTIVE INFELICITIES IN ECONOMICS

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Seven Kinds of Computable and Constructive Infelicities in Economics♣

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*"And all the men and women merely players. They have their exits and their entrances, and one man in his time plays many parts, His acts being **seven ages**."*

As You Like It, Act. II, Sc. VII (bold emphasis, added)

♣ Remembrances of *Seven Types of Ambiguity* (Empson, 1930), *Seven Kinds of Convexity* (Ponstein, 1967), the *Seven Pillars of Wisdom* (Lawrence, 1922), *Seven Schools of Macroeconomics* (Phelps, 1990), *The Magical Number Seven* ... (Miller, 1956) and the various *Seven wonders of the world*, have played not a small role in the title of this paper. I have, however, with some reluctance, not invoked the *Seven Deadly Sins*, nor the *seven plenteous years* of the **Genesis** (41-46, King James' version), in my metaphorical allusions.

Abstract

At least seven kinds of misconceptions about constructive and computable mathematics prevail in economics. In this paper the infelicitous claims of computable or constructive frameworks for the excess demand functions of microeconomics, constructive general equilibrium theory and computable general equilibrium modelling, agent based economics, search theory, game theory, index number theory and Neo Ricardian economics and the fundamental theorems of welfare economics are considered. The claims are shown to be misleading from the point of view of formal computability theory or constructive mathematics (especially – but not only – when based on Brouwerian Intuitionistic Logic).

JEL Codes: C63, C65, C70, D58, D60,

Keywords: Constructive general equilibrium theory, agent based economics, index number theory, fundamental theorems of welfare economics, excess demand functions, search theory, game theory, computability, constructive analysis.

§ 1. Introduction

It is encouraging to discern a concern for the computational underpinnings of theoretical frameworks in economic theory, especially in its applied aspects. This encouraging trend is also manifested in concern for the constructive – in its strict mathematical sense – nature of proofs, sometimes underpinned by intuitionistic logic.

Alas, the emphases and concerns are intermingled with mathematical inaccuracies and, worse, complete and thoroughly muddled claims and statements on computability theory and constructive mathematics (especially, but not exclusively, based on its Brouwerian Intuitionistic Foundations) of a variety of economic theories – even in textbooks aimed at graduate students -and applications. Seven of these extravagant, yet false, computable and constructive claims form the main subject matter of this paper.

Thus, in the next seven sections, they are considered in terms of representative computable or constructive claims, but not necessarily in the chronological order in which they appeared in the relevant economic literature. In the next section, § 2, computable and constructive properties of the excess demand function is considered. In § 3, similar claims by so-called constructive general equilibrium theory and computable general equilibrium *modelling*, in the sense of the Stone-Johansen modelling of multisectoral growth theory, are discussed. In §4, some of the more extravagant computable and constructive assertions by agent based economics (ABE) is discussed and, hopefully, diffused. Claims of constructive proofs in applied search theory are the subject matter of § 5. Similar issues, this time in Index Number Theory and aspects of what, for want of a more appropriate name, I may refer to as Neo Ricardian Economics, form the topic of § 6.. A particular, but untenable, claim on a constructive proof of the min-max theorem is the subject matter of § 7. The more core theoretical issues of computability theory and constructive mathematics pertaining to the fundamental theorems of welfare economics are the topics discussed in § 8. The concluding § 9, apart from discussing a few additional claims of a similar sort, also from the point of view of computability theory or constructive mathematics, suggests alternative perspectives for an economic theory underpinned by computability theory or constructive mathematics.

A mild explanatory note is in order regarding the difference between what I call CGE *Theory* and CGE *Modelling* This distinction is made, mostly, for lack of an alternative way of delineating the difference between a CGE *Theory* underpinned by conventional general equilibrium theory à la Arrow-Debreu and CGE *Modelling* based on the Johansen-Stone tradition of *Computable Models of Growth* (cf., Johansen (1960), Stone-Brown (1962)).

Finally, an even milder cautionary note may not be out of place here. It is crucial that any reader of this essay keep in mind the essential difference between computability theory (or recursion theory), based on standard mathematical logic, and constructive mathematics - of any variety, but particularly that which is underpinned by intuitionistic logic.

§ 2. Computable General Equilibrium theory & The Excess Demand Function

The path from an Arrow-Debreu model of Walrasian economic equilibrium to Computable General Equilibrium (CGE) models is via the *Uzawa's Equivalence Theorem*. The claim that such models are computable (or constructive) rests on mathematical foundations of an algorithmic nature: i.e., on recursion theory (or some variety of constructive mathematics).

By now it is almost well-known that those important results in general economic equilibrium theory or game theory, relying on one or another (sometimes many, simultaneously – implicitly) fix-point theorem, are uncomputable and (recursively) undecidable, whilst also being non-constructive. However, the status of the *Excess Demand Function*, from computable and constructive points of view, remain generally unexamined. Thus, on 21st July, 1986, Arrow (1986) wrote as follows to Alain Lewis (Arrow Papers, Box 23; italics added):

> "[T]he claim the *excess demands are not computable* is a much profounder question for economics that the claim that equilibria are not computable. The former challenges economic theory itself; if we assume that human beings have calculating capacities not exceeding those of Turing machines, then the *non-computability of optimal demands is a serious challenge to the theory* that individuals choose demands optimally."

That ‘the excess demands are not computable’ can be proved, using result on the *Unsolvability of the Halting Problem for Turing Machines*[1].

To prove that the excess demand functions are not computable, it is best to start from one half of the celebrated Uzawa Equivalence Theorem (Uzawa, 1962), which is, by the way, the basis for Scarf’s pioneering work on computable general equilibrium models. This half of the theorem shows that the Walrasian Equilibrium Existence Theorem (WEET) implies the Brouwer fix point theorem (which, in its nonintuitionistic versions, is both nonconstructive and uncomputable) and the finesse in the proof is to show the feasibility of devising a continuous excess demand function, ***X***(***p***), satisfying Walras' Law (and homogeneity), from an arbitrary continuous function, say ***f***(.): ***S***→***S***, such that the equilibrium price vector implied by ***X***(***p***) is also the fix point for ***f***(.), from which it is ‘constructed’. The key step in proceeding from a given, arbitrary,
f (.): ***S***→***S*** to an excess demand function ***X***(***p***) is the definition of an appropriate scalar:

$$\mu(p) = \frac{\sum_{i=1}^{n} p_i f_i[\frac{p}{\lambda(p)}]}{\sum_{i=1}^{n} p_i^2} = \frac{p.f(p)}{|p|^2} \tag{1}$$

Where:

$$\lambda(p) = \sum_{i=1}^{n} p_i \tag{2}$$

From (1) and (2), the following excess demand function, $X(p)$, is defined:

$$x_i(p) = f_i(\frac{p}{\lambda(p)}) - p_i\mu(p) \tag{3}$$

i.e.,

$$X(p) = f(p) - \mu(p)p \tag{4}$$

It is simple to show that (3) [or (4)] satisfy:

(i). ***X***(***p***) is *continuous* for all prices, $\boldsymbol{p} \in \boldsymbol{S}$

[1] I believe the proof is achievable by other ways, as well – but none of the other proofs this author is capable of implementing can avoid an appeal to free swinging, formal, set theoretic or orthodox mathematical logic tools (including the *tertium non datur* and, thereby, *reductio ad absurdum*). All of this is eschewed by a constructivist mathematician, particularly those who follow Brouwer or Bishop.

(ii). $\boldsymbol{X}(\boldsymbol{p})$ is *homogeneous of degree* 0;

(iii). $\boldsymbol{p} \bullet \boldsymbol{X}(\boldsymbol{p}) = 0, \forall \boldsymbol{p} \in S$, i.e., *Walras' Law* holds:

$$\sum \boldsymbol{p}_i \boldsymbol{x}_i(\boldsymbol{p}) = \boldsymbol{0}, \forall p \in S \ \& \ \forall i = 1\ldots..n \qquad (5)$$

Hence, $\exists \boldsymbol{p}^*$ s.t., $\boldsymbol{X}(\boldsymbol{p}^*) \leq 0$ (with equality unless $\boldsymbol{p}^*=0$). Elementary logic and economics then imply that $\boldsymbol{f}(\boldsymbol{p}^*) = \boldsymbol{p}^*$. I claim that the procedure that leads to the definition of (3) [or, equivalently, (4)] to determine $\boldsymbol{p}^*$ is provably *undecidable*. In other words, the crucial scalar in (1) cannot be defined recursion theoretically to *effectivize* a sequence of projections that would ensure convergence to the equilibrium price vector.

Theorem 3:

$\boldsymbol{X}(\boldsymbol{p}^*)$, as defined in (3) [or (4)] above is *recursively undecidable*; i.e., *cannot be determined algorithmically*.

Proof:

Suppose, contrariwise, there is an algorithm which, given an arbitrary $\boldsymbol{f}(.)$: $\boldsymbol{S} \rightarrow \boldsymbol{S}$, determines $\boldsymbol{X}(\boldsymbol{p}^*)$. This means, therefore, in view of (i)-(iii) above, that the given algorithm determines the equilibrium $\boldsymbol{p}^*$ implied by WEET. In other words, given the arbitrary initial conditions $\boldsymbol{p} \in \boldsymbol{S}$ and $\boldsymbol{f}(.)$: $\boldsymbol{S} \rightarrow \boldsymbol{S}$, the assumption of the existence of an algorithm to determine $\boldsymbol{X}(\boldsymbol{p}^*)$ implies that its *halting configurations* are *decidable*. But this violates the ***undecidability of the Halting Problem for Turing Machines***. Hence, the assumption that there exists an algorithm to determine - i.e., to construct – $\boldsymbol{X}(\boldsymbol{p}^*)$ is untenable.

Almost sixty years ago, Patinkin (1956, p11; 1965, p. 7; italics added), made a deceptively simple claim on the feasibility of 'constructing' excess demand functions:

> "Indeed, we can consider the individual – with his given indifference map and initial endowment $\boldsymbol{P}$ – to be a 'utility computor'[2] into whom we 'feed' a sequence of market prices and from whom we obtain a corresponding sequence of 'solutions' in the form of specified optimum positions. In this way we can *conceptually generate* the individual's excess-demand curve for, say, $\boldsymbol{X}$; this shows the excess amounts of $\boldsymbol{X}$ he demands at the various prices."

Patinkin's thought-experiment is no different from an engineer claiming to build a perpetual-motion machine, violating the second law of thermodynamics, which is, by the way, a 'law', not

[2] Note: 'comput*or*' – not 'comput*er*'! But, of course, Turing's classic (Turing, 1936) was about the human computer, which is what Patinkin intends with his 'computor'.

a theorem, just as much as the *Undecidability of the Halting Problem* is underpinned by a 'thesis' – the Church Turing Thesis.

§ 3. *Constructive* General Equilibrium *Theory* & Computable General Equilibrium *Modelling*

That CGE in the Scarf Tradition is not underpinned by either computable or constructive mathematics has been shown above, in § 2 and in Velupillai (2009). But it is less well documented that the claims of computability of the Johansen-Stone tradition is equally untenable. Therefore some comments may be in order.

Somewhat surprisingly, the adherents and aficionados of Leif Johansen's classic work on **A Multi-Sectoral Study of Economic Growt**h (Johansen, 1960 [1974]) claim that this was `the *first* CGE model' (Dixon & Parmenter, 2009, p. 6). Their rationale for this claim is the following (p. 6; last two italics, added):

> "[The Johansen model] was general in that it contained .. cost minimizing industries and utility-maximizing household sectors....His model employed market equilibrium assumptions in the determination of prices. Finally, it was *computable* (and applied). It produced a numerical, multi-sectoral description of growth in Norway using Norwegian input-output data and estimates of household price and income elasticities derived using Frisch's ... additive utility method."

This is an untenable claim. The authors are confusing numerical analysis and so-called scientific computing and, in particular, experimental simulations, with computable claims. The Stone-Brown study, although titled as a **A Computable Model of Growth**, is in the same class as the Johansen study, and the tradition that was broached by it. In particular, neither was underpinned by formal general equilibrium theory in the Arrow-Debreu-Walras senses.

On the other hand, the study by Mantel (1968) is explicitly based on formal general equilibrium theory and lies in what I would like to refer to as the Scarf-tradition, following Scarf (1973). However, Mantel is more explicit in his constructive claims, although there is the caveat 'Toward' in his results on a 'Constructive Proof of the Existence of Equilibrium in a Competitive Economy'.

Alas not a single proof in Mantel (ibid) is constructive in any of the senses of constructive analysis, in particular, once again, not in the Brouwer or Bishop traditions. Mantel's invoking – almost indiscriminately – the Bolzano-Weierstrass Theorem, moreover, makes the results non-constructifiable in the Brouwerian sense[3].

I shall only take up three examples of nonconstructive elements in Mantel's analysis and results.

Firstly, in the '*Proof of the Fundamental Theorem*', Mantel (*op.cit*, pp. 184-6), a subtle invoking of the nonconstructive Farkas' lemma is invoked.

Secondly, in '*An Alternative Proof of the Fundamental Theorem*', Mantel (ibid, p. 194, ff), uses the method of proof by contradiction and an appeal to the tertium non datur in the case of infinite alternatives.

Thirdly, in the proof of Theorem 3 (ibid, pp. 187-8), nonconstructive characterisations of compact sets are freely used.

All of the existence proofs in Mantel's classic paper are nonconstructive. It will be hard to find a way to justify the explicit claim that the approach is 'Toward' anything like constructive proofs of the Existence of Equilibrium in a Competitive Economy. Furthermore, none of the computations that are claimed feasible by Mantel are algorithmic either in the sense of computability theory, or in terms of any variety of constructive mathematics.

§ 4. Agent Based Economics (ABE)

Agent Based Economics ABE), together with what can be called Computable General Equilibrium *modelling* based on the Johansen-Stone tradition of *Computable Models of Growth* (cf., Johansen (1960; 1974), Stone-Brown (1962)), belong to the general area of Computational Economics. Whereas all such computational economics exercises and approaches – at least to the extent they

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[33] Mantel does not specify – and neither do any of the others discussed in this paper – what variety of constructive mathematics he means when he uses the term.

are known to this author – seem to be based on traditional numerical analysis and 'scientific computation', i.e., (see Braverman and Cook, 2006), the claims of at least one branch of ABE make explicit computability and constructivity claims. These claims are untenable. The discussion here refers to some of the paradigmatic claims, by a few – but leading – exponents of this approach, in particular, Testfatsion (2006) and Epstein (2005).

Epstein is, perhaps, the most explicitly egregious example of incorrect claims on the computable and constructive foundations of what he refers to as *Generative Social Science* (*op.cit*, chapter 1), which is simply another name for one strand – perhaps the most popular one – of ABE[4]. There is, unfortunately, a thorough confusion of the differences between computable and constructive mathematics, and their respective mathematical logic foundations, quite apart from pure mathematical and metamathematical mistakes on the definitions and characterisations of both these branches.

The following claims are made in Epstein (op.cit), all of which are examples of either a lack of understanding the difference between computability theory and constructive mathematics (of any variety – but especially that which is based either on Intuitionistic Logic or on Bishop-style constructive mathematics (Bishop, 1967, 1985), or of an incomplete and incorrect command of computability theory or constructive mathematics.

First of all, there is the absolutely elementary fallacy in even the statement of the Church-Thesis. Epstein's (*op.cit*., p. 11; italics added[5]) less than careful version is stated as follows:

> '[E]very computation – including *every agent-based computation* – can be executed by a suitable register machine.'

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[4] It is most unprofessional to refer to definitions, theorems and results in computable and constructive mathematics by simply stating the name of an author, with the date of the title of the book from which an attentive reader is supposed to obtain a substantiation of the claims made in Epstein (*op.cit*). It is especially irritating, then, when one does work through the referred texts – ex., Hodel (1995) or Kleene (1967) – to find that the definitions, theorems and results are incorrectly stated in Epstein (*ibid*). These observations apply, *pari passu*, to Testfatsion (2006).

[5] The statement on p.27 (ibid) is equally fallacious, and for the same reasons.

There are, then, references to 'Hodel 1995; Jeffrey 1991' (but NOT with exact page references. In fact, however, as can be expected, the statements in either of these two classic and competent references are more careful. Just as an example, the definition in Hodel (*op.cit*., p. 44) is stated as Church's Thesis – *not* as the Church-Turing Thesis (italics added):

> '***Church's Thesis:*** Every *computable* function is *recursive*.'

The statement in Jeffrey (ibid, p. 105, in §7.4, with the heading 'Church-Turing Thesis'; italics added), is:

> 'Register machines can be programmed to compute any functions that are *computable* at all.'

The subtlety involved in these alternative, but equivalent, statements of Church's Thesis (which is how it should be referred to, not as the Church-Turing Thesis, see Soare, 2014) depends very much on the word *computable* and its distinction from *computation*. It is not the case, as Epstein asserts, that 'every computation .. can be executed by a suitable register machine'; it is that every computable function can be so programmed by a register machine for 'execution' in the sense of computability.

Every function defined in constructive mathematics executes a computation – in Brouwerian constructivism they are also continuous; but such functions are not necessarily recursive in the sense of computability theory – nor do they appeal to Church's – the Church-Turing – Thesis.

Epstein claims (ibid, p. 11; italics added), that:

> 'Not all deductive arguments have the *constructive character of agent-based modelling*. Nonconstructive existence proofs are obvious examples'

However, the 'theorem' asserted on the same page[6] is proved non-constructively (by Kleene, 1967, §42, especially pp. 244-7 – but, once again, Epstein does not give page references), indeed by the very 'proof by contradiction' that is 'condemned' by Epstein (ibid, pp. 11-12) for underpinning its validation by an appeal to the *tertium non datur*.

[6] The theorem is, unfortunately – but unsurprisingly - stated imprecisely.

Unfortunately, the recursive unsolvability of the halting problem is also proved non-constructively and by the more general – than proof by contradiction - method of reductio ad absurdum; the characterization of the notion of absurdity used in this proof is not constructive. As a matter of fact, those three pioneers of the beautiful negative result on Hilbert's Tenth Problem made an observation that is relevant in this particular context (Davis, Matiyasevic and Robinson, 1976, p. 340; italics added);

> 'In a sense, the negative answer to Hilbert's tenth problem[7] justifies the past treatment of individual Diophantine equations by ad hoc methods (instead of developing a general theory which is impossible). In a similar sense, .. our proving the finiteness of the number of solutions of an exponential Diophantine equation *by reduction ad absurdum and other noneffective methods* [is justified]; these methods may well be the best available to us because universal methods for giving all the solutions do not exist.'

The sufficiency, effectively computable and necessity conditions claimed by Epstein, for agent based economics (ibid, p. 8), requires that the data processed be such that they are generated by recursively enumerable sets that are recursive. Moreover, with the methods of agent based economics, as described and defined by Epstein, the implication if that the recursion theoretic graph theorem must hold. The formal statement of the recursion theoretic graph theorem is:

Theorem:

The Recursion Theoretic Graph Theorem: Let ϕ and ξ be, respectively, a partial and a total function. Then:

1. ϕ is partial recursive *iff* its graph is a recursively enumerable set;
2. ξ is recursive *iff* its graph is a recursive set;

Then, using (Brattka, 2001):

Brattka's Thesis I: A function $\zeta : \mathbb{R}^n \rightarrow \mathbb{R}$ is computable iff it can be evaluated on a physical computer with arbitrary given precision;
and,

Brattka's Thesis II: A closed subset $\Phi \subseteq \mathbb{R}$ is recursive iff it can be displayed by a physical computer for an arbitrary given resolution;

it is not difficult to prove – but nonconstructively – the following proposition:

Proposition:

[7] Epstein's reference to this famous problem in on p.26 (*ibid*), but he does not realise that Prasad's results are vitiated by Harrop's finessed definition of recursivity of finite sets (cf., Harrop, 1961 and Tsuji, et. al., 1998).

No function employed in any of the agent based models in Epstein (*op.cit*), satisfies either of Brattka's Theses.

Remark:
Under the above conditions generative, in the sense defined by Epstein (ibid, p. 11), implies, and is implied by, deductive.

Thus, the constructivist claims by both Epstein (2005) and Testfatsion (2006, especially pp. 835-6) are untenable; unfortunately, the series of confusions perpetuated by these authors, on computability theory, constructive mathematics and the relationship between the two, are not of much help in the justification of the noble practice of learning by simulation – whether by computably or constructively underpinned methods, or not. Finally, the reliance on first-order (mathematical) logic, freely and indiscriminately invoked by Epstein, is not compatible with Brouwerian – or even Bishop's – constructive analysis.

§ 5. Search Theory[8]

Müller and Rüschendorf (2001, p. 683, Theorem 4.1; henceforth referred to as *M & R*), state and 'prove' the following '*extension of Strassen's theorem*'[9]:

> For two probability measures P and Q on the real line the following conditions are equivalent:
>
> 1. $\boldsymbol{P} \leq_{cx} \boldsymbol{Q}$[10];
> 2. there are random variables $\boldsymbol{X}$, $\boldsymbol{Y}$ with distributions $\boldsymbol{P}$, $\boldsymbol{Q}$ such that $\boldsymbol{E}\,[\boldsymbol{Y} \mid \boldsymbol{X}] = \boldsymbol{X}$ and the conditional law $[\boldsymbol{Y} \mid \boldsymbol{X} = \boldsymbol{x}]$ is stochastically increasing in $\boldsymbol{x}$.

M & R then claim the following for their 'proof' of the above theorem:

i. That their's is 'a simple *algorithmic proof*' (*ibid*, p. 681);

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[8] I am greatly indebted to my friend and mentor, John McCall, for introducing me to the important work of Alfred Müller and Ludger Rüschendorf, in the context of *sequential search theory* (see McCall and McCall, p. xxi, p.391).

[9] The reason for the inverted commas within which the term *prove* appears will become evident in the sequel. There are eleven theorems in Strassen (1965), but *M & R* do not specify which one is *THE* 'Strassen theorem' that is 'extended'. I surmise it is either theorem 1 or theorem 8; however, the contents of this section does not depend in the exact identification of *The* 'Strassen theorem', although I believe M & R actually mean theorem 8 in Strassen (*op.cit*)..

[10] The suffix ***cx*** denotes a *dual convex order* (p. 681).

ii. At the same time it is claimed (op.cit, p. 681) that it is a '*constructive proof*';
iii. Moreover, it is also claimed (p. 681, italics added), that their 'proof … can *easily*[11] be transformed into an *efficient algorithm*.'

To the above claims on the 'proof' the authors add the following two remarks (both on p. 681, *ibid*):

a. That 'the classic paper of Blackwell (1953) contains *a constructive proof*';
b. That 'Strassen (1965) gives a *nonconstructive functional analytic proof* of the representation result in a general context';

It is *easy* (sic!) to show that the 'proof' of theorem 4.1 in M & R is *Nonconstructive*[12]. Hence, the first claim for the 'proof' is false. As for the third claim, the authors do not define 'efficiency' of an algorithm – but I presume they refer to some notion of *computational complexity*. This notion applies only to algorithmic – i.e., computabily theoretic – derivations and *not* to constructive proofs. The difference is important: the former is predicated upon some version of the Chruch-Turing *Thesis*; the latter explicitly disawows any such invoking (cf. Bishop, 1967, p. 6; 1985, p. 20).

As for the remarks on Blackwell and Strassen, respectively, the opposite is the case; i.e., 'the classic paper of Blackwell (1953)' does *not* 'contain a constructive proof' – whereas the proofs of theorems 1 and 8 in Strassen, are based on properties of the classic 'functional analytic' tool of the Hahn-Banach Theorem. This theorem has eminently sensible constructive and algorithmic (i.e., computable) versions[13], which requires, above all, an assumption of a kind of separability of the spaces over which some definitions are based – which are, perhaps inadvertently, made by Strassen.

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[11] The article contains the sobriquets *easy*, *easily* and *simple* at least fifteen times (according to my last count) within the limited span of thirteen pages – but *none* of the demonstrations or derivations can be referred to as such. Moreover, none of them are *constructive* or *algorithmic* (in the sense of *computabilit*y theory) in any mathematically rigorous sense. The authors seem not to be clear about the difference between constructive and computable theories.

[12] In the 'proof' of their theorem, M & R claim 'that [Y | X = x] is stochastically increasing in x follows from results in Stoyan (1983). This is not a very helpful claim, especially in the context of constructive or algorithmic proofs, because the 'results in Stoyan (1983)' are quintessentially nonconstructive.

[13] See Bishop, *op.cit*, p. 263) and Metakides & Nerode (1985, p. 87)..

In passing the notion of a '*prophet*', invoked by M & R for the value of a particular random variable (cf. first equation on p. 673) in an optimal control problem, can be shown to be that of a computation with an *Oracle*.
Finally, the notion of a *mean preserving spread* defined and used in both Machina & Pratt (1997) and Rothschild & Stiglitz (1970) are neither constructive, nor algorithmic (again, in the strict sense of computability theory).

§ 6. Index Number Theory & Claims of Constructive Proofs in Sraffa

I have bracketed these two seemingly diverse theories for a very special reason. The particular index number construction by Ikle (1972), that is the topic of this section, is similar to the construction of a Standard Ratio as in Sraffa (1960). The proof of the construction of the standard commodity is the topic that has come under some discussion, from the point of view of constructive proofs, by some investigators of Sraffa's proof of the existence (and uniqueness) of the standard system.

Constructing (sic!) index numbers is an important aspect of policy analysis. Whether the particular constructed index is underpinned by theory or not, it has to be *computed*, by the use of some *algorithm* – formally, based on computability theory or any of one or more varieties of constructive mathematics. The third alternative is by means of numerical analysis and so-called scientific computing, one that has not been resorted to in serious index number analysis.

In Ikle's elegant development of index numbers for aggregate prices and quantities (Ikle, 1972, especially p. 195), proof of a method of determining its unique value is claimed to be given by an iterative – i.e., algorithmic – process. However, the proof is noneffective and nonconstructive (see Ikle, ibid, Appendix III[14], pp. 206-210).

[14] Appendix III is subtitled *Iteration-Convergence Problem* and is authored by Mrs. B. S. Stone and not by Ikle.

Firstly, because undecidable disjunction are invoked in the proof, via an appeal to the ubiquitous Bolzano-Weierstrass Theorem[15] (*ibid*, p.208), its iterative convergence to a unique value is recursively indeterminate.

Secondly, a systematic appeal to proof by contradiction – and, hence, also the *tertium non datur* – in addition, of course, to the Bolzano-Weierstrass Theorem makes the results nonconstructive. In fact, all five lemmas in the appendix resort to nonconstructive proofs by contradiction and an (implicit) invoking of the *tertium non datur*.

In passing, it may be mentioned that the main alternative to the Ikle Index – in this writer's opinion – is the Geary-Khamis Index, whose existence is proved by invoking the nonconstructive and uncomputable version of the Brouwer fix-point theorem.

As for claims on a constructive proof of, for example, the Perron-Frobenius Theorem, using a Sraffa-type algorithm, made, for example, by Lippi (2008), it is easy to show that this is untenable both from a constructive and computable point of view. In the so-called proof by Lippi (*ibid*, esp., p. 252), appeal is made to proof by contradiction, *tertium non datur* (in the case of infinite alternatives) and choice of uncomputable real numbers and function. This latter can be guaranteed only on the assumption of the unrestricted axiom of choice. The related results in Kurz-Salvadori (2001) and Salvador (2008) are equally delinquent from the point of view of a constructive proof; moreover, in the former, Kurz and Salvadori further assume the validity of the Bolzano-Weierstrass Theorem.

§ 7. Game Theory

Although there are many computable and constructive claims by game theorists and others (Osborne and Rubinstein, 1994, p.6, Giocoli, 2003, especially §5, Mirowski, 2002, for example, p. 410 ff.), all of which, without exception, are incorrect, I shall only discuss Dantzig's classic

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[15] As stated in the above proof, p.208, 'Hence the R_k [the value of the aggregate quantity index at the *k*th iteration] form a bounded sequence and by a well-known result have at least one limit point'. The 'well-known result' is the Bolzano-Weierstrass Theorem.

paper of 1956[16], and its claim on providing a constructive proof of the min-max theorem. Unfortunately, the literature on this topic is severely harmed by a lack of knowledge of Euwe's contribution to a constructive formulation of the min-max theorem (Euwe, 1929) and the Brouwer-von Neumann exchange on constructive approaches to game theory. This is admirably and concisely pointed out by van Dalen (2005, p. 636, italics added):

> 'In 1929 there was another publication in the intuitionistic tradition*: an intuitionistic analysis of the game of chess* by Max Euwe [(1929)]. It was a paper in which the game was viewed as a *spread* (i.e. a tree with the various positions as nodes)[17]. Euwe carried out *precise constructive estimates* of various classes of games, and considered the influence of the rules for draws. When he wrote his paper he was not aware of the earlier literature of Zermelo and Dénès König[18]. *Von Neumann called his attention to these papers*, and in a letter to Brouwer Von Neumann sketched a classical approach to the mathematics of chess, pointing out that it would *easily be constructivized*.'

Just for the record, von Neumann *never constructivized* the 'classical approach to the mathematics of chess – nor is it true that 'it would easily be constructivized'. In fact, to the best of this author's knowledge, von Neumann never 'constructivized' any proof of any of his game theoretic or growth theoretic theorems.

The claims, in Giocoli (*op*. *cit*., p. 34, footnote 32) and Mirowski (ibid), on the constructivity of Dantzig's proofs of the min-max theorem, are incorrect. Both of these authors make the untenable assertion that the so-called 'direct method' of proof is either constructive, or a path towards constructive proofs, in contrast with the 'indirect method' of proof not being so. Giocoli

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[16] It was, however, already released as a Rand Discussion Paper in 1954 (Dantzig, 1954).

[17] This is the Brouwerian constructive mathematical equivalent of the standard formulation of extensive form games.

[18] Dummett's detailed observation on the intuitively constructive and effective status of *König's Lemma* may be useful to keep in mind (Dummett, 2000, p. 49; italics added):

> "*Intuitionistically*, the .. proof of König's Lemma is invalid … . Moreover, we cannot remedy the situation by modifying the proof of König's Lemma, since reflection on the difficulty involved shows that there is no reason to suppose König's Lemma to be *constructively* true. Intuitionistically understood, the assertion that *there exists* an infinite path amounts to the claim that we can *effectively* define such a path; but the mere fact that there is no finite upper bound on the lengths of paths does not supply us with any way of doing this, since we have no *effective* means of deciding, for each given node, whether or not it is the case that there is a finite upper bound on the lengths of paths going through it."

goes on to make the explicit claim that this (incorrect) interpretation – for which I do not blame him, but the sources he has used for inferring thus, particularly Punzo (1981) and Mirowski (op.cit.), are both seriously deficient in understanding the meaning, nature and scope of constructive mathematics – allows him to impute a particular constructive approach in von Neumann-Morgenstern (1944; 1947, 1953).

On the other hand, without a clear mastery of constructive mathematics there is always the danger of false assertions and untenable claims, together with a confusion with a computable – or recursion theoretic - development of proofs and algorithms[19]. This latter issue is what makes Dantzig's claims of having provided a constructive proof of the min-max theorem incorrect. A final remark here may well be apposite, especially in view of the specific examples chosen and discussed by Dorfman (1952) and Dantzig (op.cit.): the simplex method is intractable – in the sense of computational complexity theory (Schriver, 1986, §11.4, p. 139, ff.).

Before continuing, it may be useful, just for convenience, to state, formally, a statement of the <u>Min-Max Theorem</u> (Dantzig, ibid, p.26)):

> $\exists$ a choice for player I of $w_i = \hat{w}_i$ *and* a choice for player II of $y_j = \hat{y}_j$ such that the corresponding $w_o = \hat{w}_o$ is the maximum value for $\hat{w}_o$ and the corresponding $y_o = \hat{y}_o$ is the minimum value for y_o and, $\hat{w}_o = \hat{y}_o$.

The following three reasons – at least – make the proof of this theorem, in Dantzig (ibid), nonconstructive:

- The (implicit) assumption of the (nonconstructive) *axiom of choice*;
- The *constructive undecidability* of the inequalities in (7) of Dantzig (ibid, p. 27);
- Relation (11), and hence (12), is constructively undecidable.

Several of the non-strict inequalities in the proof of the Min-Max Theorem are also constructively undecidable.

[19] As Errett Bishop (1967, p. 6; italics added) observed, with his usual perspicacity:

> 'In fairness to Brouwer it should be said that he did not associate himself with these efforts to formalize reality; it is the fault of the logicians that many mathematicians who think they know something of the constructive point of view have in mind a dinky formal system or, *just as bad*, *confuse constructivism with recursive function theory*.'

Now, the most striking fact of Dantzig's classic is that the word 'constructive' appears *only* in the title of the paper[20]. Apart from this curious fact, the methods of proof, used in this fine paper, are distinctly nonconstructive – especially exemplified by the proof by contradiction of the *construction of the optimal basis* (see, p. 29, ff, especially p. 30).
However, the methods of proof are entirely consistent with the canons of computability theory and, hence, the determined algorithms can be considered to be predicated upon the strictures of the Church-Turing Thesis. In particular, the computably determined algorithm for computing the optimal basis is, as the author explicitly acknowledges, a variation of the simplex method (p. 29, footnote 3), there will be cases when it will be intractable, in the sense of computational complexity theory (Schriver, ibid).

Therefore, it is in order to make a categorical statement here: *Max Euwe's 1929 contribution (ibid), remains, to this day, the first and only constructive proof of the min-max theorem*[21].

§ 8. The Fundamental Theorems of Welfare Economics

It may be appropriate simply to state the uncomputable and non-constructive underpinnings of the two fundamental theorems of welfare economics. This is because these two theorems are stated, and are the subject matter of detailed discussion, in all of the advanced textbooks on mathematical economics and advanced microeconomic theory (for example, Starr, 2007, Stoke, et.al., 1989). But when stated, they are done so in terms of their importance and relevance in policy contexts – without any indication of their computable or constructive status.

The only exception to this 'rule' is when some appeal to made to what I have come to call the 'Neighs theorem', with the assumption that the proof of this theorem defines an algorithm in the sense of computability theory, to make the first fundamental theorem computationally implementable. This is not correct (cf. Velupillai, 2015).

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[20] Although the word *nonconstructive* does appear once (*ibid,* p. 26).
[21] I am, of course, well aware of Stigler's maxim on 'naming' the originator of a theory and Borges' precept on predecessors (Stigler, 1966, p.77, Borges, 1999, p.73)

As for the second fundamental theorem of welfare economics, this is intrinsically nonconstructive in view of the appeal made to the nonconstructive Hahn-Banach Theorem.

The First Fundamental Theorem of Welfare Economics asserts that a competitive equilibrium is Pareto optimal. The theorem is proved using an uncomputable equilibrium price vector to compute an equilibrium allocation. Therefore, the contradiction step – which is, therefore, nonconstructive -- in the proof requires a comparison (which is recursively undecidable) between an uncomputable allocation and an arbitrary allocation, for which no computable allocation can be devised. Moreover, the theorem assumes the *intermediate value theorem in its non-constructive form*. Finally, even if the equilibrium price vector is computable, the contradiction step in the proof invokes the *tertium non datur* and is, therefore, unacceptable constructively (because it requires algorithmically undecidable disjunctions to be employed in the decision procedure, cf. Starr, *ibid*, chapter 19).

The Second Fundamental Welfare Theorem establishes the proposition that any Pareto optimum can, for suitably chosen prices, be supported as a competitive equilibrium. The role of the nonconstructive Hahn-Banca theorem in the proof of this proposition is in establishing the above mentioned suitable price system. The Hahn-Banca theorem does have a constructive version, but only on subspaces of separable normed spaces. The standard, `classical' version, valid on no separable normed spaces depends on Zorn's Lemma which is, of course, equivalent to the axiom of choice, and is therefore, non-constructive[22]. Schechter's perceptive comment on the constructive Hahn-Banca theorem is the precept I wish economists with a numerical, computational or experimental bent should keep in mind (ibid, p. 135; italics added):

> ‘[O]ne of the fundamental theorems of classical functional analysis is the Hahn-Banach Theorem; ... some versions assert the existence of a certain type of linear functional on a normed space *X*. The theorem is inherently nonconstructive, but a constructive proof can be given for a variant involving normed spaces *X* that are separable - i.e., normed spaces

[22] This is not a strictly accurate statement, although this is the way many advanced books on functional analysis tend to present the Hahn-Banach theorem. For a reasonably accessible discussion of the precise dependency of the Hahn-Banach theorem on the kind of axiom of choice (i.e., whether countable axiom of choice or the axiom of dependent choice), see Narici & Beckenstein (1997). For an even better and fuller discussion of the Hahn-Banach theorem, both from ‘classical’ and constructive points of view, Schechter's encyclopedic treatise is unbeatable (Schechter, 1997).

> that have a countable dense subset. *Little is lost in restricting one's attention to separable spaces*[23], for in applied math most or all normed spaces of interest are separable. The constructive version of the Hahn-Banach Theorem is *more complicated*, but it has the advantage that it *actually finds the linear functional in question*.'

§ 9. Reflections on the Past, Hopes for the Future

The main thrust of this essay can be summarized by Errett Bishop's masterly summary of what I have called the confusion between computability theory and constructive mathematics and a regrettable lack of understanding of the logical and other foundational differences between the two (Bishop, 1967, p. 6; italics added):

> 'In fairness to Brouwer it should be said that he did not associate himself with these efforts to formalize reality; it is the fault of the logicians that many mathematicians[24] who think they know something of the constructive point of view have in mind a dinky formal system or, *just as bad*, *confuse constructivism with recursive function theory*.'

The only caveat I would like to add to this observation is that that the word 'mathematician' be substituted by 'mathematical and other kinds of economists' – at least for the explicit purpose of reading this essay.

Why does this particular 'confusion' persist – now for over at least sixty years? One reason seems to be the way mathematics and mathematical logic are taught at graduate schools and practiced by so called mathematical economists, of any vintage. There would have been an excuse for ignorance of computability theory and constructive mathematics, with their attendant analytical branches as computable and constructive analysis (see, for example Aberth, 2000, Weihrauch, 2000 and Bishop & Bridges, 1985, respectively), in the works of our noble neoclassical mathematical economics predecessors – primarily Walras and Marshall – simply because these subjects did not come into being, in their modern formulations, at the time they framed their theories in the language of the dominant mathematics of their time. Yet, they tried valiantly to find a way at least to discuss solvability with a context of what may today be called

[23] However, it must be remembered that Ishihara (1989), has shown the constructive validity of the Hahn-Banach theorem also for *uniformly convex spaces*.

[24] The only caveat I would like to add to this observation is that that the word 'mathematician' be substituted by 'mathematical and other kinds of economists' – at least for the explicit purpose of reading this essay.

algorithmic formalisation, but which were alternative formulations of a variety of noneffective iterations, in non-recursive, nonconstructive, frameworks.

Modern teaching and graduate texts aimed at advanced undergraduates and normal graduate students in economics, enrolled at any decent University – or even special courses for those employed at leading international institutions, Central Banks Treasury departments and the like - in an age of algorithmic thinking and implementation, seem to be oblivious to the *Zeitgeist*. Indeed, the exemplar of practice seems to be best illustrated by the example of the contents of a thoroughly non-algorithmic advanced textbook – viz., that by Efe Ok, on **Real Analysis with Economic Applications** (Ok, 2007), but even the excellent graduate microeconomic textbook by Starr (2007) represents this genre, one that is in the formalistic tradition of Debreu (1959), but without any incongruency can be considered an extension of von Neumann-Morgenstern (op.cit). Starr uses the *excess demand function*, the *Uzawa Equivalence Theorem* and the *Brouwer Fixed-Point Theorem* quite without any reservation – algorithmic, or not. Indeed, he seems to be unreserved in extolling their virtues in any attempt at formalizing a general equilibrium economy, especially in the tradition of Arrow-Debreu (Starr, ibid, p. 193; italics added):

> 'What are we to make of the Uzawa Equivalence Theorem? It says the Brouwer Fixed-Point Theorem is not merely one way to prove the existence of equilibrium. *In a fundamental sense, it is the only way*. Any alternative proof of existence will include, inter alia, an implicit proof of the Brouwer Theorem. Hence, *this mathematical method is essential*; one cannot pursue this branch of economics without the Brouwer Theorem. If Walras …provided an incomplete proof of existence of equilibrium, it was in part because the necessary mathematics was not yet available.'

The proof of the Brouwer-Fixed Point Theorem – whether via the Sperner Lemma or not – is unconstructifiable (cf. Brouwer, 1952). Does this mean we, as mathematical economists, are 'condemned' to nonconstructive general equilibrium theory? Why does Starr repeat the 'old adage' that 'Walras … provided an incomplete proof of existence of equilibrium' (see, Hicks, 1983, p. 183)?

I leave the reader to ponder over these untenable assertions and continue with a reflection on Ok's even more absurd stance on constructive mathematics, underpinned by intuitionistic logic. Ok (2007, p. 279; italics added), asserts that,

> "It is worth noting that in later stages of his career, he became the most forceful proponent of the so-called intuitionist philosophy of mathematics, which not only forbids the use of the Axiom of Choice but also rejects *the axiom* that a proposition is either true or false (thereby disallowing the method of proof by contradiction). The consequences of taking this position are dire. For instance, an intuitionist would *not accept the existence of an irrational number*! In fact, *in his later years, Brouwer did not view the Brouwer Fixed Point Theorem as a theorem*. (He had proved this result in *1912*, when he was functioning as a 'standard' mathematician)'

What can a poor graduate student in economics do, in the face of such unreserved claims in two fairly standard textbooks (i.e., Starr and Ok)? In fact, Ok's suggestion is:

> 'If you want to learn about *intuitionism* in mathematics, I suggest reading – in your spare time please – reading the articles by Heyting and Brouwer in Benacerraf and Putnam (1983).'

Even though (Brouwerian or any other kind of) constructive mathematics is not mentioned by either Starr or Ok, and the latter seems to refer only to Intuitionism, it is clear that they both mean that constructive mathematics (or even computability theory) is irrelevant for the mathematics they advocate.

I have to mention that every one of the claims in the first of the above quotes from Ok is incorrect. Yet, every one of the assertions in the above quote is false, and also severely misleading. Brouwer did not 'become the most forceful proponent of the so-called intuitionist philosophy of mathematics in *later stages* of his career'; he was an intuitionist long before he formulated and proved what came, later, to be called the Brouwer Fix-Point theorem (cf. Brouwer, 1907, 1908A & 1908B). Just for the record, even the fixed-point theorem came earlier than 1912. It is nonsensical to claim that Brouwer did not consider the 'Fixed Point Theorem as a theorem'; he did not consider it *a valid theorem in intuitionistic constructive mathematics*, and he had a very cogent reason for it, which was stated with admirable and crystal clarity when he finally formulated and proved it, forty years later, within intuitionistic constructive mathematics (Brouwer, 1952). On that occasion he identified the reason why his original theorem was unacceptable in intuitionistic constructive - indeed, in almost any kind of constructive - mathematics, for example, in *Bishop-style constructivism*, which was developed without any reliance on a philosophy of intuitionism.

What of a vision for formalization in economics with Petty's old aim of 'number, weight and measure' in the forefront? No economic data, whether natural or artificial, can be, or is, a nonconstructive or uncomputable real number, or in a decidable real number interval – explicitly or by implication (example as a result of a solution to some economically relevant equation or inequality). It is the underlying – even if unstated – theme of this essay (of sustained critique) is that economics, to be quantitative, must place number, numerical computation and learning from experimental simulation (using digital methods), at the centre of teaching, research and empirical applications, with policy analysis in mind. Economics is not an exercise in pure mathematics. This theme is unachievable by the current way of modelling and formalizing economics, of any variety – orthodox or heterodox (which is why a variety of examples, exemplifying all of these aspects defining the theme, were chosen for discussion and critique). It will not be inappropriate to end this essay with Errett Bishop's wisdom, echoing William Petty's method Bishop, 1967, pp. 2-3):

> 'The primary concern of mathematics is number, and this means the positive integers. … When a man proves a positive integer to exist, he should show how to find it. If God of his own that needs to be done, let him do it himself. …
> [E]ven the most abstract mathematical statement has a computational basis.'

The God of orthodox mathematical economics has made mortal economists to do 'his' mathematics. It is, surely, time to call 'his' – i.e., God's - bluff.

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