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DISCUSSION PAPER SERIES

10 – 2011/II

DSGE AND BEYOND – COMPUTABLE AND CONSTRUCTIVE CHALLENGES♠

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JULY 2011

♠ I am, as always, deeply conscious of my debt to my friend and colleague, Stefano Zambelli, in the preparation of this paper. In particular, his recent work on linking coupled nonlinear dynamics at the microeconomic level with aggregate macrodynamics, has been a source of valuable support in the 'beyond' part of the title of this paper. He is, however, absolved from all responsibilities for the remaining infelicities in this paper.

ABSTRACT

The genesis and the path towards what has come to be called the *DSGE* model is traced, from its origins in the *Arrow-Debreu General Equilibrium* model (**ADGE**), via Scarf's *Computable General Equilibrium* model (**CGE**) and its applied version as *Applied Computable General Equilibrium* model (**ACGE**), to its ostensible dynamization as a *Recursive Competitive Equilibrium* (**RCE**). An outline of a similar nature, albeit very briefly, of the development and structure of Agent-Based Economics (ABE) is also included. It is shown that these transformations of the ADGE model are computably and constructively untenable. Suggestions for going 'beyond **DSGE and ABE**' are, then, outlined on the basis of a framework that is underpinned – from the outset – by computability and constructivity considerations.

JEL Codes: C02, C62, C68, D58, E61

Keywords: Computable General Equilibrium, Dynamic Stochastic General Equilibrium, Computability, Constructivity, Classical Behavioural Economics.

1 A Preamble

"... the dreadful permanence of a certain second in one's temporary life."

James Kirkup[1]

At least until the advent of the recent crisis, the dominance of the DSGE approach to macrodynamics seemed to have been the accepted benchmark to anyone attempting serious modelling of policy with rigorous microeconomic foundations. This consensus vision – controversial and not unchallenged even in the best of times - has come under some increasing sceptical scrutiny, to put it mildly, in the last three years.

Many competent critiques of the DSGE methodology, with alternative visions ably formulated, have come to be considered in all circles where, previously, there was an almost proverbial 'one-size-fits-all' philosophy to the mathematical modelling of rigorously founded macroeconomics. The New Keynesian monopoly of alternatives to DSGE visions has, thus, been diluted, albeit not – at least till now – entirely supplanted.

Most importantly and interestingly, the many contributions to varieties of boundedly rational, agent-based, economic dynamics, have taken on the DSGE visions and methodology squarely and critically, with seemingly challenging results in formal, rigorous, computational frameworks.

However, all the way from the core contributions to DSGE modelling, philosophy and visions, to current fashions in agent-based economic and financial modelling in ostensibly explicit computational frameworks, the underlying assumption seems to have been an uncritical acceptance of the claims on the mathematical structure of the computable and constructive foundations of the basic pillars of general equilibrium theory - from their origins in the classic of Arrow-Debreu General Equilibrium (ADGE), through Scarf's development of Computable General Equilibrium (CGE) theory, to DSGE via Recursive Competitive Equilibrium (RCE).

This paper is a contribution to the critique of the foundations of DSGE modelling, from an explicitly computable and constructive mathematical point of view. However, it is a part of the broader framework and vision that this author has come to call the *Computable Approach to Economic Theory*, within which the following eight results have been derived[2]:

i.Nash equilibria of finite games are constructively indeterminate.

ii.Computable General Equilibria are neither computable nor constructive.

iii.The Two Fundamental Theorems of Welfare Economics are Uncomputable and Nonconstructive, respectively.

iv.There is no effective procedure to generate preference orderings.

[1] James Kirkup, p. 87, in: **These Horned Islands** (The Macmillan Company, NY, 1962).

[2] Apart from the first and sixth results, which are due to the pioneering works of Tsuji, Da Costa and Doria ([39]) in 1998 and Michael Rabin ([28]) in 1957, the rest are due to this author.

v.Recursive Competitive Equilibria (RCE), underpinning the Real Business Cycle (RBC) model and, hence, the *Dynamic Stochastic General Equilibrium* (DSGE) benchmark model of Macroeconomics, are uncomputable.

vi.There are games in which the player who in theory can always win cannot do so in practice because it is impossible to supply him with effective instructions regarding how he/she should play in order to win.

vii.The theoretical benchmarks of Algorithmic Game Theory are uncomputable and non-constructive.

viii.Emergent formalisms in Agent-Based Economic Modelling have no foundations in any kind of rigorous algorithmic formalism and, hence, epistemologically vacuous.

In the next section a brief foray – entirely inadequate from any serious point of view – into aspects of non-standard mathematics (and non-standard logic), relevant for making sense of the rest of the paper, is attempted. In section three an attempt at dissecting the computable and constructive claims of the varieties of general equilibrium models that form the foundations of DSGE models is presented; a brief foray into the untenable claims of aspects of agent-based economic modelling is also included. The *brief* concluding section suggests some constructive and computable ways of going '*beyond DSGE*'.

2 An Ultra-Brief Non-Traditional Mathematical *Excursus*

> "This is a specimen of intuitionist reasoning in topology, and in particular an illustration of the consequences of the invalidity of the Bolzano-Weierstrass theorem in intuitionism, for *the validity of the Bolzano-Weierstrass theorem would make the classical and intuitionist forms of the fixed-point theorms equivalent.*"
> [4], p. 1; italics added.

Brouwer, in the above quote, is – of course – referring to his celebrated fixed-point theorem, widely used in mathematical economics in its original form, or in one or another of its 'generalizations', by Kakutani, KKM[3], etc. On the other hand, just because a fixed-point theorem is invalid from an intuitionistic point of view[4] does not necessarily mean that it is non-constructive or uncomputable from mathematical points of view claiming allegiance to other forms of constructivism and varieties of computability theories. The point here, however, is the role of the Bolzano-Weierstrass theorem and its intrinsic undecidable disjunctions, which make any theorem invoking it in its proof fundamentally

[3] An acronym for *Knaster-Kuratowski-Mazurkiewicz*.

[4] We are 'advised', in a recent advanced textbook in **Real Analysis with Economic Applications** ([27], p. 279, footnote 47), 'if [we] want to learn about intuitionism in mathematics', to do so 'in [our] spare time, please'! The footnote in which this 'advice' appears is replete with elementary mathematical and biographical errors (on Brouwer).

non-constructive and uncomputable from any (known) mathematical point of view.

In this author's considered and studied belief, the key advance from the pure mathematics of general equilibrium theory and game theory is the claim by adherents of CGE, RCE, RBC, SDGE and, most recently, also by those practitioners of algorithmic game theory (AGE), that the theoretically proved equilibrium existence theorems, in the respective fields, can be given constructive and computable content. This is a belief based on explicit claims by eminent practitioners of CGE, RCE, RBC, SDGE and AGE[5]. If these claims are to retain their validity from this particular point of view, the mathematics in which their formalism is clothed must be constructively or computably meaningful. As Jeremy Avigad perceptively noted, recently[6]:

> "[The] adoption of the *infinitary, nonconstructive*, *set theoretic*, algebraic, and structural methods that are characteristic to modern mathematics [. . . .] were controversial, however. At issue was not just whether they are consistent, but, more pointedly, whether they are meaningful and appropriate to mathematics. After all, *if one views mathematics as an essentially computational science*, then arguments without computational content, whatever their heuristic value, are not properly mathematical. .. [At] the bare minimum, we wish to know that the universal assertions we derive in the system will not be contradicted by our experiences, and *the existential predictions will be borne out by calculation*. This is exactly what *Hilbert's program*[7] was designed to do."
>
> [1], pp. 64-5; italics added

Thus, my claim is that the *existential predictions* made by the purely theoretical part of mathematical economics, game theory and economic theory '*will* [*not*] *be borne out by calculations.'* There is, therefore, a serious *epistemological deficit* – in the sense of economically relevant knowledge that can be processed and accessed computationally and experimentally – in all of the above approaches, claims to the contrary notwithstanding, that is unrectifiable without *wholly abandoning their current mathematical foundations*. This is an *epistemological deficit* even before considering the interaction between appeals to infinite – even uncountably infinite – methods and processes in proofs, where both the universal and existential quantifiers are freely used in such contexts,

[5] Explicit references to substantiate this claim can be found in [43] and [44], as well as in the sequel, below.

[6] Avigad's important observation was made in the context of *The Mathematics of Ergodic Theory*. It is only necessary for the critically minded mathematical economist or economic theorist simply to substitute 'economic' for 'ergodic' and nothing would change in the implications.

[7] I have tried to make the case for interpreting the philosophy and methodology of mathematical economics and economic theory in terms of the discipline of *Hilbert's program* in [47].

and the *finite* numerical instances[8] with which they are, ostensibly, 'justified'. This epistemological deficit requires even 'deeper' mathematical and philosophical considerations in *Cantor's Paradise*[9] of ordinals[10] , where combinatorics, too, have to be added to computable and constructive worlds to make sense of claims by various mathematical economists and agent based modeling practitioners.

3 Five 'Impossible' Computable and Constructive Claims In *ADGE*, *CGE*, *RCE* & *ABE*

> Alice: There is no use trying; one can't believe impossible things.
>
> White Queen: I dare say you haven't had much practice. When I was your age, I always did it for half an hour a day. Why, sometimes I've believed as many as six impossible things before breakfast.

Computable and constructive claims are routinely made by theorists and policy oriented practitioners whose work forms the basis of one or another aspect of an eventual DSGE model that ends up by being the foundation for serious policy applications. I shall, in a slight variation of Lewis Carrol's wisdom, take up only 'five impossible things' that are of relevance in the context of this paper. Given that I have listed eight results in the *Preamble*, I could easily multiply impossible examples quite liberally; but it will be sufficient to stick to this slight variation in Lewis Carrol's wisdom, at least for the limited purposes of the aims of this paper.

[8] Serényi's ([33]) very recent reflections and results on this issue will play an important part in the theoretical underpinnings to be developed in this paper, when viewed from the point of view of the computable approach to economics (p.49; italics added):

> "An argument deriving the truth of a universal arithmetical sentence from that of its numerical instances suggests that the truth of the numerical instances has some kind of *epistemological priority* over the truth of the sentence itself: our knowledge of the truth of the sentence stems from the fact that we know all its numerical instances to be true. .. I shall show that it is just the other way around. . . . *[T]he source of our knowledge of the truth of the totality of its numerical instances is the truth of the sentence itself.*"

[9] Hilbert did not want to be driven out of '*Cantor's Paradise*' ([15]; p.191):

> 'No one shall drive us out of the paradise which Cantor has created for us.'

To which the brilliant 'Brouwerian' response, if I may be forgiven for stating it this way, by Wittgenstein was ([54]; p.103):

> 'I would say, "I wouldn't dream of trying to drive anyone out of this paradise." I would try to do something quite different: I would try to show you that it is not a paradise – so that you'll leave of your own accord. I would say, You're welcome to this; just look about you." '

[10] Where '*Ramsey Theory*', '*Goodstein Sequences*' and the '*Goodstein theorem*', reign supreme. In work in progress these issues are dealt with in some detail, as they pertain to bridging the '*epistemological deficit*' in economic theoretical discourse in the mathematical mode.

3.1 *The Nonconstructive Aspect of Brouwer's Theorem*

In Scarf's classic book of 1973 there is the following characteristically careful caveat to any unqualified claims to *constructivity* of the algorithm he had devised:

> "In applying the algorithm it is, in general, *impossible* to select an ever finer sequence of grids and a convergent sequence of subsimplices. An algorithm for a digital computer must be basically finite and cannot involve an infinite sequence of successive refinements. .... *The passage to the limit is the nonconstructive aspect of Brouwer's theorem*, and we have no assurance that the subsimplex determined by a fine grid of vectors on S contains or is even close to a true fixed point of the mapping."
>
> [30], p.52; italics added

An algorithm, by definition, is a finite object, consisting of a finite sequence of instructions. However, such a finite object is perfectly compatible with 'an infinite sequence of successive refinements' ([30], p. 52), provided a stopping rule associated with a clearly specified and verifiable approximation value is part of the sequence of instructions that characterize the algorithm. Moreover, it is *not* 'the passage to the limit [that] is the nonconstructive aspect of Brouwer's [fix point] theorem' (ibid, p.52)[11]. Instead, the sources of non-constructivity are the undecidable disjunctions - i.e., appeal to the *law of the excluded middle* in infinitary instances - intrinsic to the choice of a convergent subsequence in the use of the Bolzano-Weierstrass theorem[12] and an appeal to the *law of double negation* in an infinitary instance during a *retraction.* The latter reliance invalidates the proof in the eyes of the Brouwerian constructivists; the former makes

[11] In [31], p. 1024, Scarf is more precise about the reasons for the failure of constructivity in the proof of Brouwer's fix point theorem:

> "In order to demonstrate Brouwer's theorem completely we must consider a sequence of subdivisions whose mesh tends to zero. Each such subdivision will yield a completely labeled simplex and, *as a consequence of the compactness* of the unit simplex, there is a convergent subsequence of completely labeled simplices all of whose vertices tend to a single point x^*. (This is, of course, the non-constructive step in demonstrating Brouwer's theorem, rather than providing an approximate fixed point)."

There are two points to be noted: first of all, even here Scarf does not pinpoint quite precisely to the main culprit for the cause of the non-constructivity in the proof of Brouwer's theorem; secondly, nothing in the construction of the algorithm provides a justification to call the value generated by it to be an approximation to x^*. In fact the value determined by Scarf's algorithm has no theoretically meaningful connection with x^* (i.e., to p^*) for it to be referred to as an approximate equilibrium.

[12] Just for ease of reading the discussion in this section I state, here, the simplest possible statement of this theorem:

> **Bolzano-Weierstrass Theorem:** Every bounded sequence contains a convergent subsequence

it constructively invalid from the point of view of every school of constructivism, whether they accept or deny intuitionistic logic.

Brouwer's proof of his celebrated fix point theorem was indirect in two ways: he proved, first, the following:

Theorem 1 *Given a continuous map of the disk onto itself with no fixed points, $\exists$ a continuous retraction of the disk to its boundary.*

Having proved this, he then took its *contrapositive:*

Theorem 2 *If there is no continuous retraction of the disk to its boundary then there is no continuous map of the disk to itself without a fixed point.*

Using the logical principle of equivalence between a proposition and its contrapositive (i.e., logical equivalence between theorems 7 & 8) and the law of double negation ($\nexists$ a continuous map with **no** fixed point $=$ $\exists$ a continuous map with a fixed point) Brouwer demonstrated the existence of a fixed point for a continuous map of the disk to itself. This latter principle is what makes the proof of the Brouwer fix point theorem via retractions (or the non-retraction theorem) essentially unconstructifiable. Scarf's attempt to discuss the 'relationship between these two theorems [i.e., between the non-retraction and Brouwer fix point theorems] and to interpret [his] combinatorial lemma [on effectively labelling a restricted simplex] as an example of the non-retraction theorem is incongruous. This is because Scarf, too, like the Brouwer at the time of the original proof of his fix-point theorem, uses the full paraphernalia of non-constructive logical principles to link the Brouwer and non-retraction theorems and his combinatorial lemma[13].

3.2 Scarf's Fixed Point Algorithms are Constructive

The economic foundations of **CGE** models lie in *Uzawa's Equivalence Theorem* ([41], [9], p.719, ff); the mathematical foundations are underpinned by *topological fix point theorems* (Brouwer, Kakutani, etc.). 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. It is a widely held belief that CGE models are both *constructive* and *computable.* That the latter property is held to be true of **CGE** models is evident even from the generic name given to this class of models; that the former characterization is a feature of such models is claimed in standard expositions and applications of **CGE** models. For example in the well known, and pedagogically elegant, textbook by two of the more prominent advocates of applied **CGE** modelling in policy contexts, John Shoven and John Whalley ([34]), the following explicit claim is made:

[13] Scarf uses, in addition, proof by contradiction where, implicitly, LEM (*tertium non datur*) is also invoked in the context of an infinitary instance (cf. [31], pp. 1026-7).

> "The major result of postwar mathematical general equilibrium theory has been to demonstrate the existence of such an equilibrium by showing the applicability of mathematical fixed point theorems to economic models. ... Since applying general equilibrium models to policy issues involves computing equilibria, these fixed point theorems are important: It is essential to know that an equilibrium exists for a given model before attempting to compute that equilibrium. .....
>
> ...
>
> The weakness of such applications is twofold. First, they provide *non-constructive rather than constructive proofs of the existence of equilibrium*; that is, they show that equilibria exist but do not provide techniques by which equilibria can actually be determined. Second, existence per se has no policy significance. .... Thus, fixed point theorems are only relevant in testing the logical consistency of models prior to the models' use in comparative static policy analysis; such theorems do not provide insights as to how economic behavior will actually change when policies change. *They can only be employed in this way if they can be made constructive* (i.e., be used to find actual equilibria). *The extension of the Brouwer and Kakutani fixed point theorems in this direction is what underlies the work of Scarf .... on fixed point algorithms ....*"
>
> ibid, pp12, 20-1; italics added

Quite apart from a direct implication of the results of the previous subsection falsifying the above claims, they are also untenable because the Uzawa Equivalence Theorem is provably undecidable. This is the topic of the next subsection.

3.3 The Uzawa Equivalence Theorem

The Uzawa Equivalence theorem is the fulcrum around which the *theory* of **CGE** modelling revolves. This key theorem[14] provides the theoretical justification for relying on the use of the algorithms that have been devised for determining general economic equilibria as fix points using essentially non-constructive topological arguments. The essential content of the theorem is the mathematical equivalence between a precise statement of *Walras' Existence Theorem* (**WET**) and Brouwer's (or any other relevant) Fix-Point Theorem. To study the algorithmic - i.e., computable and constructive - content of the theorem, it is necessary to analyse the assumptions underpinning **WET**, the nature of the proof of economic equilibrium existence in **WET** and the nature of the proof

[14] To the best of my knowledge, none of the standard advanced textbooks in mathematical economics, microeconomics or general equilibrium theory (Kreps, Varian, etc.), except the two by Cornwall ([8]) and Starr ([36]), even refer to Uzawa's theorem.

of equivalence. By the 'nature of the proof' I mean, of course, the constructive content in the logical procedures used in the demonstrations- whether, for example, the law of double negation or the law of the excluded middle (LEM: *tertium non datur*) is invoked in non-finitary instances. Therefore, I shall, first, state an elementary version of **WET** (cf., [41], p. 60 or [36], p. 136).

Theorem 3 *Walras" Existence Theorem (**WET**)*

Let the excess demand function, $X(p) = [x_1(p), ..., x_n(p)]$, be a mapping from the price simplex, S, to the $\mathbb{R}^N$

commodity space; i.e., $X(p) : S \to \mathbb{R}^N$

where:

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

ii). $X(p)$ is homogeneous of degree 0;

iii). $p.X(p) = 0, \forall p \in S$ (Walras' Law holds: $\sum_{i=1}^{n} p_i x_i(p) = 0$, $\forall p \in S$)[15]

Then:

$\exists p^* \in S$, s.t., $X(p^*) \leq 0$, with $p_i^* = 0$, $\forall i$, s.t., $X_i(p^*) < 0$

The finesse in this half of the equivalence theorem, i.e., that **WET** implies the Brouwer fix point theorem, is to show the feasibility of devising[16] a continuous excess demand function, $X(p)$, satisfying Walras' Law (and homogeneity), from an arbitrary continuous function, say $f(.) : S \to S$, 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, $f(.) : S \to S$ 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:

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[15] As far as possible I attempt to retain fidelity to Uzawa's original notation and structure even although more general formulations are possible. .

[16] I have to seek recourse to words such as 'devise' to avoid the illegitimate use of mathematically loaded terms like 'construction', 'choice', 'choose', etc., that the literature on **CGE** modelling is replete with, signifying, illegitimately, possibilities of meaningful - i.e., algorithmic - 'construction', 'choice', etc. For example, Uzawa, at this point, states: "We *construct* an excess demand function.." (op.cit, p.61; italics added; Starr, at a comparable stage of the proof states: "If we have *constructed* [the excess demand function] cleverly enough..." (op.cit., p.137; italics added). Neither of these claims are valid from the point of view of any kind of algorithmic procedure.

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

i.e.,

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

It is simple to show that (3) [or (4)] satisfies (i)-(iii) of Theorem 3 and, hence, $\exists p^*$ s.t., $X(p^*) \leq 0$ (with equality unless $p^* = 0$). Elementary (non-constructive) logic and economics then imply that $f(p^*) = p^*$. I claim that the procedure that leads to the definition of (3) [or, equivalently, (4)] to determine $p^*$is provably *undecidable*. In other words, the crucial scalar in (1) cannot be defined recursion theoretically (and, *a fortiori*, constructively) to effectivize a sequence of projections that would ensure convergence to the equilibrium price vector.

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

Proof. *Suppose, contrariwise, there is an algorithm which, given an arbitrary $f(.) : S \rightarrow S$, determines $X(p^*)$. This means, therefore, in view of (i)-(iii) of Theorem 1, that the given algorithm determines the equilibrium p^* implied by **WET**. In other words, given the arbitrary initial conditions $p \in S$ and $f(.) : S \rightarrow S$, the assumption of the existence of an algorithm to determine $X(p^*)$ 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 - $X(p^*)$ is untenable.* ■

Remark 5 *The algorithmically important content of the proof is the following. Starting with an arbitrary continuous function mapping the simplex into itself and an arbitrary price vector, the existence of an algorithm to determine $X(p^*)$ entails the feasibility of a procedure to choose price sequences in some determined way to check for p^* and to halt when such a price vector is found. Now, the two scalars, μ and λ are determined once $f(.)$ and p are given. But an arbitrary initial price vector p, except for flukes, will not be the equilibrium price vector p^*. Therefore the existence of an algorithm would imply that there is a systematic procedure to choose price vectors, determine the values of $f(.)$, μ and λ and the associated excess demand vector $X(p; \mu, \lambda)$. At each determination of such an excess demand vector, a projection of the given, arbitrary, $f(p)$, on the current $X(p)$, for the current p, will have to be tried. This procedure must continue till the projection for a price vector results in excess demands that vanish for some price. Unless severe recursive constraints are imposed on price sequences - constraints that will make very little economic sense - such a test is algorithmically infeasible. In other words, given an arbitrary, continuous, $f(.)$, there is no procedure - algorithm (constructive or recursion theoretic) - by which a sequence of price vectors, $p \in S$, can be systematically tested to find p^*.*

Remark 6 *In the previous remark, as in the discussion before stating Theorem 4, I have assumed away the difficulties with uncomputable functions, prices and* *so on. They simply add to complications without changing the nature of the content of Theorem 4.*

3.4 From CGE to RCE

The undisputed pioneers of RBC theory, Kydland and Prescott, appear to claim that the path towards RCE, and hence the benchmark for DSGE, begins with ADGE, and 'was greatly advanced by Shoven and Whalley, who built on the work of Scarf', [19], p.168. However, go on Kydland and Prescott:

> "Their approach is ill-suited for the general equilibrium modelling of business fluctuations becasue dynamics and uncertainty are crucial to any model that attempts to study business cycels. To apply general equilibrium methods to the quantitative study of business cycle fluctuations, we need methods to compute the equilibrium processes of dynamic stochastic economies, and specific methods for the stochastic growth model economy. Recursive competitive theory and the use of linear-quadratic economies[17] are methods that have proven particularly useful. These tools make it possible to compute the equilibrium stochastic processes of a rich class of model economies."
>
> ibid., p. 169

The power this particular dynamic extension of the traditional equilibrium concept plays a significant role in the mathematized macroeconomy is further described, four years later, by Cooley and Prescott:

> "Another great advantage of the RCE approach is that for an increasingly rich class of model economies, *the equilibrium process can be computed* and *can be simulated to generate equilibrium paths for the economy*. These paths can be studied to see whether model economies mimic the behavior of actual economies and can be used to provide quantitative answers to questions of economic welfare."
>
> [7], p.9; italics added.

Now, there are three problems with these claims and aims. First of all, and trivially, nowhere in the literature on mathematical economics, mathematical macroeconomics or even in formal computability theory is there any proposition on the *efficiency of processes*; in fact, it is quite easy to show that the dynamic programming formulation for the RCE is, in fact computationally intractable in a precise sense. Secondly, neither the first nor the second welfare theorems are computationally feasible in the precise senses of computability theory and constructive analysis. Thirdly, the approximation procedures used,

[17] It is not too much of an exaggeration to observe that the assumption of 'linear-quadratic economies' is as prevalent and as mendacious as the assumption of an aggregate production function of a Cobb-Douglas form; neither are approximation to what they claim to represent.

in computing the relevant RCE are provably intractable, simply because the equilibrium is uncomputable!

I shall only deal with the second of these infelicities in this paper. Companion pieces to this work tackle the whole set of issues more systematically.

The *First Fundamental Theorem of Welfare Economics* asserts the that *a competitive equilibrium is Pareto optimal*. A textbook formulation of the theorem is as follows ([36], p. 145):

Theorem 7 *Assume Weak monotonicity and continuity of preferences; Let $p^* \in \Re^N_+$ be a competitive equilibrium price vector of the economy. Let ω^{0i}, $i \in H$, be he associated individual consumption bundles, and let y^{0j}, $j \in F$,be the associated firm supply vectors. Then ω^{0i} is Pareto efficient.*

where:

F : set of firms.

Proof. See [36], p. 145-6. ■

Remark 8 *The theorem is proved non-constructively, using an uncomputable equilibrium price vector to compute an equilibrium allocation. Therefore, the contradiction step in the proof requires a comparison 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 law of the excluded middle and is, therefore, unacceptable constructively (because it requires algorithmically undecidable disjunctions to be employed in the decision procedure).*

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 Hahn-Banach theorem in this proposition is in establishing *the suitable price system.*

Lucas and Stokey state 'their' version of the Hahn-Banach Theorem in the following way[18].

Theorem 9 *Geometric form of the Hahn-Banch Theorem.*

Let S be a normed vector space; let $A, B \subset S$ be convex sets. Assume:

(a). Either B has an interior point and $A \cap \mathring{B} = \emptyset$, $\left(\mathring{B} : \text{ closure of } B\right)$;

(b). Or, S is finite dimensional and $A \cap B = \emptyset$;

[18] Essentially, the 'classical' mathematician's Hahn-Banach theorem guarantees the extension of a bounded linear functional, say ρ, from a linear subset Y of a separable normed linear space, X, to a functional, η, on the whole space X, with exact preservation of norm; i.e., $|\rho| = |\eta|$. The constructive Hahn-Banach theorem, on the other hand, cannot deliver this pseudo-exactness and preserves the extension as: $|\rho| \leq |\eta| + \varepsilon$, $\forall \varepsilon > 0$. The role of the positive ε in the constructive version of the Hahn-Banach theorem is elegantly discussed by Nerode, Metakides and Constable in their beautiful piece in the Bishop Memorial Volume ([26], pp. 85-91). Again, compare the difference between the 'classical' IVT and the constructive IVT to get a feel for the role of ε.

Then: $\exists$ *a continuous linear functional* ϕ, *not identically zero on* S, *and a constant* c *s.t:*
$\phi(y) \leq c \leq \phi(x)$, $\forall x \in A$ *and* $\forall y \in B$.

Next, I state the economic part of the problem in merciless telegraphic form as follows:

There are$I$ consumers, indexed $i = 1, ..., I$;

S is a vector space with the usual norm;

Consumer i chooses from commodity set $X_i \subseteq S$, evaluated according to the utility function $u_i : X_i \rightarrow \Re$;

There are j firms, indexed $j = 1, ..., J$;

Choice by firm j is from the technology possibility set, $Y_j \subseteq S$; (evaluated along profit maximizing lines);

The mathematical structure is represented by the following absolutely standard assumptions:

1. $\forall i, X_i$ *is convex*;

2. $\forall i$, *if* $x, x' \in C_i, u_i(x) > u_i(x')$, *and if* $\theta \in (0,1)$, *then* $u_i[\theta x + (1-\theta) x'] > u_i(x')$;

3. $\forall i, u_i : X_i \rightarrow \Re$ *is continuous*;

4. *The set* $Y = \sum_j Y_j$ *is convex*;

5. *Either the set* $Y = \sum_j Y_j$ *has an interior point*, or S *is finite dimensional*;

Then, the *Second Fundamental Theorem of Welfare Economics* is:

Theorem 10 *Let assumptions* $1-5$ *be satisfied; let* $\left[\left(x_i^0\right), \left(y_j^0\right)\right]$ *be a Pareto Optimal allocation; assume, for some* $h \in \left\{\bar{1}, ...\bar{I}\right\}, \exists \hat{x}_h \in X_h$ *with* $u_h(\hat{x}_h) > u_h(x_h^0)$. *Then* $\exists$ *a continuous linear functional* $\phi : S \rightarrow \Re$, *not identically zero on* S, *s.t:*
(a). $\forall i, x \in X_i$ *and* $u_i(x) \geq u_i(x^0) \Longrightarrow \phi(x) \geq \phi(x_i^0)$;
(b). $\forall j, y \in Y_j \Longrightarrow \phi(j) \leq \phi(y_i^0)$;

It is a pure mechanical procedure to verify that the assumptions of the economic problem satisfy the conditions of the Hahn-Banach Theorem and, therefore, the powerful *Second Fundamental Theorem of Welfare Economics* is 'proved'[19].

The Hahn-Banach theorem does have a constructive version, but only on subspaces of *separable* normed spaces. The standard, 'classical' version, valid

[19] To the best of my knowledge an equivalence between the two, analogous to that between the Brouwer fix point theorem and the Walrasian equilibrium existence theorem, proved by Uzawa ([41]), has not been shown.

on nonseparable normed spaces depends on *Zorn's Lemma* which is, of course, equivalent to the axiom of choice, and is therefore, non-constructive[20].

Schechter's perceptive comment on the constructive Hahn-Banach theorem is the precept I wish economists with a numerical, computational or experimental bent should keep in mind (ibid, p. 135).:

> "[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 that have a countable dense subset. *Little is lost in restricting one's attention to separable spaces*[21]*, 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."

So, one may be excused for wondering, why economists rely on the 'classical' versions of these theorems? *They are devoid of numerical meaning and computational content. Why go through the rigmarole of first formalizing in terms of numerically meaningless and computationally invalid concepts to then seek impossible and intractable approximations to determine uncomputable equilibria, undecidably efficient allocations, and so on?*

Thus my question is: why should an economist *force* the economic domain to be a normed vector space? Why not a *separable normed vector space*? Isn't this because of pure ignorance of constructive mathematics and a carelessness about the nature and scope of fundamental economic entities and the domain over which they should be defined?

On the other hand, the first fundamental theorem of welfare economics fails constructively and computably on three grounds: the dependence on the intermediate value theorem (non-constructive), the use of an uncomputable equilibirum price vector in the proof by contradiction (uncomputability) and the use of the law of the excluded middle in the proof by contradiction (non-constructivity).

Under these conditions, the equilibrium of the canonical *SDGE* model, *RCE*, cannot, in any formal algorithmic sense be effectively or constructively computed; therefore, no equilibrium process can effectively be determined to show convergence to a balanced growth path.

[20] 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 [21]. For an even better and fuller discussion of the Hahn-Banach theorem, both from 'classical' and a constructive points of view, Schechter's encyclopedic treatise is unbeatable ([32]).

[21] However, it must be remembered that Ishihara, [16], has shown the constructive validity of the Hahn-Banach theorem also for uniformly convex spaces.

Finally, the mathematical structure of the space on which the value function and the policy function are defined is such that the existence of a fix point for the contraction operator that is invoked is non-algorithmizable. This is because *Cauchy Completeness* is assumed for the space over which the contraction is implemented. Cauchy Completeness, can be stated as:

Theorem 11 *Every Cauchy sequence in* $\mathbb{R}$ *converges to an element of* $\mathbb{R}$

This theorem is, in turn, proved using the *Bolzano-Weierstrass theorem*, which contains an unconstructifiable - i.e., non-algorithmic and hence impossible to utilise in a consistent 'computational experiment' - *undecidable disjunction* in its proof!

In other words, the *computational* program of mathematizing macroeconomics by formulating optimal decision problems as dynamic programming problems is **impossible**.

3.5 Agent-based computational methods and adaptive dynamics

The origins of what has become agent based computational methods can be traced to the pioneering works of Turing on *Morphogenesis* [40], von Neumann on *The Theory of Self-Reproducing Automata* ([51]), and Ulam on *Nonlinear dynamics* ([12], [37]). A 'second generation' of pioneers were Conway ([2]) and Wolfram [53]), the former directly in the von Neumann tradition and the latter straddling the von Neumann and Ulam traditions – i.e., working on the interface between cellular automata modelling and dynamical system interpretation of the transition equations.

Remarkably, there was an independent tradition in economics, pioneered by Richard Goodwin ([13]), in his computational studies of coupled markets, which directly inspired Herbert Simon's approach to the computational study of evolutionary dynamics in terms of semi-decomposable linear systems ([**?**]).

Sadly, none of these classics have had the slightest impact on the current frontiers of agent based computational economics (see, for example, [38]). Had any awareness of the classics, their frameworks, the questions they posed, the tentative answers they obtained, the research directions they suggested had been absorbed, even in some rudimentary way, many of the exaggerated claims and assertions of the advocates of agent based computable economics would have been less absurd, more measured and, surely, also humbler in the expectations of what this line of computational research could and must achieve. An example of the utterly untenable claim of a senior advocate of agent based computational economics may convey our sadness of the lack of anchoring in the classics more vividly. In his chapter, titled *Agent-Based Macro* ([38], p. 1626; italics added), Axel Leijonhufvud asserts that:

> "Agent-based computational methods provide *the only way* in which the self-regulatory capabilities of complex dynamic models can be ex-

plored so as to advance our understanding of the adaptive dynamics of actual economies."

Quite apart from the many undefined – even formally undefinable unambiguously – concepts in this remarkably unscholarly statement, the extraordinary claim that 'agent-based computational methods provide *the only way'* to understand anything, let alone of the 'adaptive dynamics of actual economies', must make the scientific spirit of Goodwin and Simon writhe in intellectual pain – not to mention the noble ghosts of Ulam, von Neumann and Turing.

What are 'agent-based computational methods'? Do they transcend Turing Machine computation? If so, how – and why? How does one link a computationally implemented method with a complex dynamical system, even assuming that it is possible to define such a thing unambiguously and consistent with the dynamics of a computation?

On the other hand, agent based computable economic practice is closely tied to the belief that such models are capable of generating so-called 'emergent phenomena', in the sense that their existence cannot be predicted from the underpinning laws of individual agent interactions. Very little scholarship on the rich tradition of philosophical, epistemological, computational and dynamic research – with a solid contribution to the epistemology of simulation (cf. [52]) – on 'emergence' is manifested in the frontier research by agent based computational economists (a paradigmatic example of inflated claims and deficient scholarship on agent based computational modelling, the tortuous concept of 'reductionism' and the possibility of so-called 'emergent aggregative phenomena' can be found in [10]).

No better characterisation of the practice of agent based computational economists can be given that the one Arthur Burks gave (cf. [5], p. xviii), on a related 'procedure for investigating cellular spaces':

> "The investigator starts with a certain global behavior and wants to find a transition function for a cellular automaton which exhibits that behaviour. He then chooses as subgoals certain elementary behavioral functions and proceeds to define his transition function piece-meal so as to obtain these behaviors.
>
> .....
>
> The task of searching for a transition function which produces a specified behavior is an arduous task because there are so many possible partial transition functions to explore."

The formal difficulties of 'searching for a transition function' are provably intractable, at best; algorithmically undecidable, in general. Even when found, depending on the way the data generating process if characterised, whether the transition function – when viewed as a finite automaton – 'halts' at the prescribed state is, again, in general, algorithmically undecidable, Correspondingly, when viewed as a dynamical system, whether the global behaviour is an attractor or is in a particular basin of attraction of the dynamical system, is algorithmicall undecidable. Whether a set of initial conditions, for the transition function, can be algorithmically determined such that their halting state is the desired global behaviour, or such that the global behaviour is in the basin of attraction of the transition function as a dynamical system is decidable only for trivial sets.

Suppose we succeed in finding such a transition function – as many agent based computational economists claim they can, and have – and want to characterise it either in terms of computability theory or as a dynamical system. Suppose, also, that we ask the questions the pioneers asked: the feasibility of self-reproduction, self-reconstruction, evolution, computation universality, decidability of limit sets of the transition function when interpreted as a dynamical system, whether the transition function, viewed as an finite automaton, is subject to the Halting Problem, and so on. At the least, any reasonable notion of 'emergence' requires unambiguous answers to most of these question – all of which are, in general, subject to algorithmic undecidabilities.

4 A Brief Excursion on *Negishi's Method*

> "In [Negishi's Method] fixed point theorems are applied on the utility simplex."
>
> [6], p. 138

There are those who seem to think it is possible to circumvent the dilemmas posed by computability and constructivity issues in proving the existence of an Arrow-Debreu equilibrium by appealing to the first fundamental theorem of welfare economics and applying a relevant fixed point theorem on the utility simplex, rather than on the price simplex. This approach has come to be called *Negishi's Method* ([22], [17], in particular, pp. 52-57) in the mathematical economic and computational economics literature. It is easy to show that all of the computability and constructivity conundrums discussed above remain unscathed in the Negishi mathematical framework. A brief *excursus* on this claim may not be out of place here.

Negishi himself, reflecting on his youthful masterpiece more than three decades later (Negishi, 1994, p. xiv; italics added), remarked:

> "The *method of proof* used in this essay [i.e., in [22]] has been found useful also for such problems as equilibrium in infinite dimensional space and computation of equilibria."

What exactly was Negishi's *method of proof* and how did it contribute to the *computation of equilibria*?

A characterisation of the difference between the standard approach to *proving the existence* of an Arrow-Debreu *equilibrium*, and its *computation*, by a tâtonnement procedure – i.e., algorithm – of a mapping from the price simplex to itself, and the alternative *Negishi method* of iterating the weights assigned to individual utility functions that go into the definition of a social welfare function which is maximised to determine – i.e., compute – the equilibrium, captures the key innovative aspect of the latter approach. Essentially, therefore, the difference between the standard approach to the proof of existence of equilibrium Arrow-Debreu prices, and their computation, and the *Negishi approach* boils down to the following:

- The standard approach proves the existence of Arrow-Debreu equilibrium prices by an appeal to a fixed point theorem and computes them – the equilibrium prices – by invoking the *Uzawa equivalence theorem* (Uzawa, 1960) and devising an algorithm for the excess demand functions that map a price simplex into itself to determine the fixed point ([30]).

- The Negishi approach *proves*, given initial endowments, *the existence* of individual welfare weights defining a social welfare function, whose *maximization* (subject to the usual constraints) *determines* the identical Arrow-Debreu *equilibrium.* The standard mapping of excess demand functions, mapping a price simplex into itself to determine a fixed point, is replaced by a mapping from the space of utility weights into itself, appealing to the same kind of fixed point theorem (in this case, the Kakutani fixed point theorem) to prove the existence of equilibrium prices.

- In other words, the method of proof of existence of equilibrium prices in the one approach is replaced by the *proof of existence* of 'equilibrium utility weights', both appealing to traditional *fixed point theorems* ([3], [50], and [18][22]).

- In both cases, the computation of equilibrium prices on the one hand and, on the other, the computation of equilibrium weights, algorithms are devised that are claimed to determine (even if only approximately) the same fixed points.

Takashi Negishi's outstanding 'contributions to economic analysis' are brilliantly and comprehensively surveyed by Warren Young in his recent paper ([55]). Young's paper provides a particularly appropriate background to the issues here. It – Young's paper – is especially relevant also because his elegant summary of Negishi's 'contribution to economic analysis' identifies [22] as one of the two crucial pillars[23] on which to tell a coherent and persuasive story of what he calls the Negishi 'research program' (ibid, p. 162; second set of italics, added):

> "To sum up, a number of major research programs can be identified, therefore, as emanating from Negishi's now *classic* papers,

[22] There is a curious – albeit inessential – 'typo' in Negishi's reference to Kakutani's classic as having been published in 1948. The 'typo' is not 'corrected' even in the reprinted version of [22] in [24].

[23] The other one being [23]. I am in full agreement with Young important observation that it is [22] that is more important, which is why I have added italics to the phrase 'almost as influential', in the above quote.

> that of (1960) [[22]] and 1961 [[23]], respectively. Negishi's 1960 paper forms the basis for both 'theoretical' and 'applied' research programs in general equilibrium analysis, and his 1961 paper ... has been *almost as influential* in demarcating ongoing research up to the present in the field of imperfect competition and non-tatonnement processes. These papers ... attest to Negishi's considerable influence on the development of modern economic theory and analysis."

However, no one – to the best of my knowledge – has studied Negishi's *method of proof* from the point of view of *constructivity* and *computability*, the issues that are central here. Young's perceptive - and, in my opinion, entirely correct - identification of the crucial role played by [22] in 'both "theoretical" and "applied" research program in general equilibrium analysis' is, in fact, about *methods of **existence** proofs* and *computable general equilibrium* (**CGE**), and its offshoots, in the form of *applied computable general equilibrium analysis* **ACGE**) – even leading up to current frontiers in computational issues in *DSGE* models (cf., [17], pp. 52-57, for example).

Before I turn to these issues of the constructivity and computability of Negishi's method of existence proofs and the underpinning of some aspects computation in CGE and ACGE models in Negishi's approach (rather than, for example, in the standard approach pioneered by Scarf, [30]), there is one important economic theoretic confusion that needs to be sorted out. This is the question of the role played by *the fundamental theorems of welfare economics* in Negishi's method of the proof of the existence of a general (Walrasian) equilibrium.

It is generally agreed that the *Negishi method of existence proof* is an applications of fixed point theorems on the *utility simplex*, in contrast to the 'standard' way of applying such theorems to the *price simplex* (cf., the above quote from Cheng). This fact has generated a remarkable confusion on the question of which fundamental theorem of welfare economics underpins the Negishi method! For a method that has been around for over half a century, it is somewhat disheartening to note that frontier research and researchers seem still to be confused on which of the two fundamental theorem of welfare economics is relevant in Negishi's method. Thus, we find Judd, as recently as only a few years ago (op.cit, pp. 52-3) claiming, unreservedly, that (italics added):

> "T*he Negishi method exploits the first theorem of welfare economics*, which states that any competitive equilibrium of an Arrow-Debreu model is Pareto efficient."

On the other hand, Warren Young (op.cit, p.152; italics added) equally confidentially stating that:

> In his pioneering 1960 paper, Negishi provided a completely new way of proving the existence of equilibrium, *via the Second Welfare Theorem*. He established equivalence between the equilibrium problem set out by Arrow-Debreu and what has been called 'mathematical programming', thereby developing a 'method' that has been used

> with much success by later economists working in both theoretical and applied general equilibrium modelling ... ."

Fortunately, Negishi himself returned to a discussion of the 'Negishi method, or Negishi approach' more recently ([25], p. 168) and may have helped sort out this conundrum (ibid, p. 167; italics added):

> "The so-called Negishi method, or Negishi approach, has often been used in studies of dynamic infinite-dimensional general equilibrium theory, and the numerical computation of such equilibria ... . This method is an application of the Negishi theorem (Negishi 1960), which demonstrates the existence of a general equilibrium *using the first theorem of welfare economics*, which states that any competitive equilibrium of an Arrow-Debreu model is Pareto efficient. In other words, a general equilibrium of a competitive economy is considered as the maximization of a kind of social welfare function (i.e., the properly weighted sum of individual utilities, where the weights are inversely proportional to the marginal utility of income."

Negishi is one of those rare economists who is both a scholar of the history of economic theory and one of the most competent general equilibrium theorists and – even if he had not been the originator of the Negishi method – therefore one may feel forced to reject Warren Young's claim[24]!

As a matter of fact, from my *Computable Economics* – i.e., from a constructivist and recursion theoretic - point of view, this conundrum is a non-problem for several reasons. First of all, both fundamental theorems of welfare economics are proved non-constructively and lead to uncomputable equilibria. Secondly, all – to the best of my knowledge – of the current algorithms utilised in CGE, ACGE and DSGE modelling appeal to undecidable disjunctions and are effectively meaningless from a computablity point of view. Thirdly, and most importantly, *Negishi's theorem*[25] is, itself, *proved* nonconstructively.

There are two theorems in [22]. I shall concentrate on *Theorem 2* (ibid, p.5), which (I think) is the more important one and the one that came to play the important role justly attributed to it via the *Negishi Research Program* outlined by Young (op.cit)[26].

[24] The puzzle here is that the Young and Negishi articles appear 'back-to-back', in the same issue of the *International Journal of Economic Theory* and the two distingusihed authors thank each other handsomely in their respective acknowledgements! Just for the record, my own view is the following. My strong conviction is that Negishi's theorem provides the '*only if*' part of the first fundamental theorem of welfare conomics.

[25] *Negishi's theorem* is one thing; *Negishi's method* is quite a different thing. The latter *should* refer to the 'method of proof', but the vast literature on the issue – admirably documented in [55] – is *not* fee of confusion on this point. Essentially, the 'method' refers to the fact that a mapping is defined, not on the price simplex, but on the 'utility simplex' (as mentioned above with a reference to [6]).

[26] To demonstrate the *nonconstructive* elements of Theorem 1 (ibid, p.5), I would need to include almost a tutorial on constructive mathematics to make clear the notion of *compactness* that is *legitimate in constructive analysis*. For reasons of 'readability' and 'deeper' reasons of

Proposition 12 *The Proof of the Existence of Maximising Welfare Weights in the Negishi Theorem is Nonconstructive*

Remark 13 *Negishi's proof relies on satisfying the Slater (Complementary) Slackness Conditions ([35][27]). Slater's proof[28] of these conditions invoke the Kakutani fixed point theorem (Theorem 1 in [18]), and Kakutani's Min-Max Theorem (Theorem 3, ibid). These two theorems, in turn, invoke Theorem 2 and the Corollary (ibid, p.458), which are based on Theorem 1 (ibid, p. 457). This latter theorem is itself based on the validity of the Brouwer fixed point theorem, which is Non-constructifiable (cf., [4]).*

Proposition 14 *The vector of maximising welfare weights, derived in the Negishi Theorem, is uncomputable*

Remark 15 *A straightforward implication of the previous proposition.*

Discovering the exact nature and source of appeals to nonconstructive modes of reasoning, appeals to undecidable disjunctions and reliance on nonconstructive mathematical entities in the formulation of a theorem is a tortuous exercise. The nature of the pervasive presence of these three elements – i.e., nonconstructive modes of reasoning, primarily the reliance on *tertium non datur*, undecidable disjunctions and nonconstructive mathematical entities – in any standard theorem and its proof, and the difficulties of discovering them, is elegantly outlined by Fred Richman:

> "Even those who like algorithms have remarkably little appreciation of the thoroughgoing algorithmic thinking that is required for a constructive proof. This is illustrated by the nonconstructive nature of many proofs in books on numerical analysis, the theoretical study of practical numerical algorithms. I would guess that most realist mathematicians are unable even to recognize when a proof is constructive in the intuitionist's sense.
>
> *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

aesthetics and mathematical philosophy, I shall refer to my two main results in this section as'Propositions' and their plausible validity as 'Remarks', and not as 'Theorems' and 'Proofs', respectively.

[27] This classic by Slater must easily qualify for inclusion in the class of pioneering articles that remained forever in the '*samizdat*' status of a *Discussion Paper*!

[28] I should add that the applied general equilibrium theorists who use Negishi's method to 'compute' (uncomputable) equilibria do not seem to be fully aware of the implications of some of the key assumptions in Slater's complementary slackness conditions. That [22] is aware of them is clear from his *Assumption 2* and *Lemma 1.*

[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."

[29], p.125; italics added.

5 Beyond DSGE & ABE –Towards a Computable Approach to Economic Modelling

"Computer science ... is not actually a science. It does not study natural objects. Neither is it, as you might think, mathematics; although it does use mathematical reasoning pretty extensively. Rather, computer science is like engineering - it is all about getting something to do something, rather than just dealing with abstractions ... .But this is not to say that computer science is all practical, down to earth bridge-building. Far from it. Computer science touches on a variety of deep issues. ... . It naturally encourages us to *ask questions about the limits of computability, about what we can and cannot know about the world around us.*"

[11], p.xiii; italics added.

'Does nature compute?', is a question natural scientists ask with increasing frequency. The differential equations, or maps, that seem to characterise the dynamical systems of nature are hardly ever analytically 'solvable'. Either we must try to devise and evolve an epistemology to come to terms with 'unsolvability' and, therefore, accept a 'truth deficit' – that 'true' solutions are inherently unreachable – or find other ways to represent nature's processes. One such alternative way is to interpret nature's processes as computations. But computations, too, may not 'halt'.

What, then, if the economy is itself a computer? Do economic processes, whether aggregative or not, embody the results of a computation? Do we, as economists, observing the economy's computational processes, impute computability properties to the economy?

A master dynamical system theorist outlined the dilemma cogently:

"We regard the computer as an 'oracle' which we ask questions. Questions are formulated as input data for sets of calculations. There are two possible outcomes to the computer's work: either the calculations rigorously confirm that a phase portrait is correct, or they fail to confirm it. .... The theory that we present states that if one begins with a structurally stable vector field, *there is input data that will yield a proof that a numerically computed phase portrait is correct. However, this fails to be completely conclusive from an algorithmic point of view, because one has no way of verifying that a vector field is structurally stable in advance of a positive outcome.*

Thus, if one runs a set of trials of increasing precision, the computer will eventually produce a proof of correctness of a phase portrait for a structurally stable vector field. Presented with a vector field that is not structurally stable, the computer *will not confirm* this fact:; it will only fail in its attempted proof of structural stability[29]. *Pragmatically, we terminate the calculation when the computer produces a definitive answer or our patience is exhausted. ....*

The situation described in the previous paragraph is analogous to the question of producing a numerical proof that a continuous function has a zero. ..... Numerical proofs that a function vanishes can be expected to succeed only when the function has qualitative properties that can be *verified* with finite-precision calculations."

[14], pp.154-5, italics added.

Analogous to Guckenheimer's thought experiment, if the data set generated by the economy as a computer is recursively enumerable but not recursive, inferences abut the computability properties of the economy will remain incomplete. On the other hand, if we – as observers – feed the economy with data sets that are also recursively enumerable but not recursive, then whether the economy, as a computer, will be able to process it in a definitive way will remain unknown for an indeterminate period. This does not, of course, mean that in some Mathematical Platonic universe, the economy is not algorithmically decidable.

Whether definitive knowledge of the structure of the economy can be obtained by observing its processes will depend on the metaphors we use to characterise it; for example, characterising the economy as a finite automaton or a dynamical system whose limit sets are stable limit points makes it easy to infer structural properties by observations of the outcome of its processes. This is the standard approach to modelling and inference of economic dynamics.

In the *computable approach to economics*, the starting point is that the economy is a Turing Machine and the data it generates forms a set that is recursively enumerable but not recursive. If so, what can be inferred about the structure of the economy may only be explored by Turing Machine computation, without any guarantee that a definitive answer will be obtained.

Computation in economics becomes *epistemologically* meaningful only when the economic modeller, using computational metaphors to analyse the data generated by the economy, begins to accept, at least *pro tempore*, that the economy, its constituents and its institutions are themselves computers[30]. This is the natural mode of interaction between the economy and the *classical behavioural economist* and the *computable economist*. The latter is a field I have explored

[29] A reader, equipped with the standard knowledge of classical recursion theory, would immediately invoke the distinction between *recursive* and *recursively enumerable* sets to make precise sense of this important observation.

[30] Throughout this paper the reference is to the digital computer. However, in view of several important results by Rubel, Pour-El and others, summarised in [48], there is no loss of serious generality in interpreting the references to 'computers' as applying to a large class of analog computers, too.

and summarised in [42] and [46]. A 'bird's eye' view of the former is given in [45].

It is not the natural mode for the CGE theorist, nor for the agent based computational economist. This is why there is a serious epistemological deficit in the practice of the latter two classes of economists.

Any attempt at going beyond DSGE and ABE, from the point of view of a *computational epistemology* – i.e., seeking knowledge from numerically meaningful modelling exercises – may not do much worse than adopt the methods, philosophy and framework of the classical behavioural economist and the computable economist. This is especially so in view of the fact that the economic foundations of computable economics is largely consistent with the basics of classical behavioural economics. Developing this theme fully would, perhaps, be a sequel to this paper.

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