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Discussion Paper Series

13 – 2011/II

THE FUNDAMENTAL THEOREMS OF WELFARE ECONOMICS, DSGE AND THE THEORY OF POLICY – – COMPUTABLE & CONSTRUCTIVE FOUNDATIONS[♠]

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September 2011

[♠] This is a revised and extended version of the paper originally titled DSGE and Beyond - Computable and Constructive Challenges, which was prepared for presentation at the National Bank of Poland conference on DSGE and beyond -- expanding the paradigm in monetary policy research?, to be held in Warsaw, on 29-30, September, 2011. This is one of a series of five papers, eventually to be collected in book form as, Unfashionable Macroeconomics. Apart from this one on Policy, the others are on Money, Cycles, Growth and Development. The methodological theme unifying all of them is what I have come to call `The Pernicious Influence of Mathematics in the Formalization of Economic Theory.' The pretense that mathematical formalism can provide unambiguous answers to problems that are essentially an intractable combination of economics, ethics, politics and philosophy is, I claim, at least due to a stunted acquaintance with only a small part of mathematics, mathematical logic and metamathematics. It does not follow that I stand to defend the alternative stance that a complete knowledge of these three fields would provide the unambiguous answers many economists seek -- even if `complete knowledge' can be given formal content in any meaningful way.

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**). It is shown that these transformations of the ADGE – including the fountainhead – are computably and constructively untenable. The policy implications of these (negative) results, via the Fundamental Theorems of Welfare Economics in particular, and against the backdrop of the mathematical theory of economic policy in general, are also discussed (again from computable and constructive points of view). Suggestions for going 'beyond **DSGE**' 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, Fundamental Theorems of Welfare Economics, Theory of Policy, Coupled Nonlinear Dynamics

1 A Preamble

> "... the dreadful permanence of a certain second in one's temporary life."
>
> James Kirkup: *These Horned Islands*, p.87 (The Macmillan Company, NY, 1962)

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 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 *Algorithmic Economic Theory*[1], within which the following eight results have been derived[2]:

i. Nash equilibria of (even) 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] More recently I have begun using *Diophantine Economics* to describe the field which began as *Computable Economics* more than twenty five years ago.

[2] Apart from the sixth result, which is due to the pioneering work of Michael Rabin ([50]) 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 an attempt is made to set the theme and mathematical content of this paper against the noble interwar traditions of *Polish Mathematics*, also as a tribute to the tenacity and verve with which it was developed by some of the 20th century's greatest mathematicians, many of whom - at least in terms of aspects of the very particular mathematization of economics – were Polish. In section 3, a brief foray – entirely inadequate from any serious point of view – into aspects of non-traditional mathematics (and non-traditional logic), relevant for making sense of the rest of the paper, is attempted. In section 4, I reflect on the (mathematical) epistemology of computation, hopefully in a sense relevant for the broader themes that underpin the main focus of the paper. Section 5, as an immediate prelude to the main technical section of this paper, provides an outline of the variety of ways in which the theory of economic policy was formalised mathematically without, however, being anchored in the *fundamental theorems of welfare economics*, although computation played some decisive role in *all* of them – *almost without* any computability or constructive underpinnings. I qualify with the word 'almost' because the *Phillips-inspired* derivation of proportional, derivative and integral macroeconomic stabilisation policies were developed within an explicit analogue computing framework[3]. In Section 6, aimed to contain the main focussed themes of the paper, 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*'.

[3]The full fascinating details of this story is documented, with the original articles by Phillips included, in the forthcoming *Special Issue* of **Economia Politica**, Vol. XXVIII, # 3, commemorating the 60th anniversary of the construction and implementation of the *Phillips Electro-Hydraulic Analogue Computing Machine*, also known as the *MONIAC* (**MO**netary **N**ational **I**ncome **A**nalog **C**omputer: a name coined by Abba Lerner, possibly with the idea of mimicking ENIAC in some way).

2 A Brief *Homage* to '*Polish Mathematics*' - and Economics.

> "The work of men who have founded and developed *Fundamenta Mathematicae* has had a deep influence on the mathematical progress of the past quarter-century. Starting with Jamiszewski and Sierpiński, there has grown up a fruitful movement with which American mathematicians have had intimate and effective relations. The work in Topology and in abstract spaces is now recognized throughout mathematics as of fundamental character; the *Polish School* under such men as Banach and Kuratowski constituted, before the present catastrophe (1939), one of the outstanding mathematical groups."
>
> Marshall Stone (quoted in [27], pp. 15-16; second set of italics, added)

Stone's handsome tribute, concentrating on *set theory*, *topology* and *functional analysis*, does not go far enough: he has forgotten the pioneering contributions made by the Polish School, in that heroic twenty-year period of 1919 – 1939, to *recursion theory*, *recursive analysis* and *metamathematics* – and forgotten, also, to include 'women'[4]. If the economic theoretic *crown jewels* of orthodox mathematical economics are the Arrow-Debreu equilibrium existence theorem and the fundamental theorems of welfare economics, then the *mathematical crown jewels* that underpin them are, surely relevant fixed point theorems (Brouwer, Knaster-Kuratwoski-Mazurkiewicz [KKM], Kakutani) and separating and supporting hyperplane theorems (especially the Hahn-Banach theorem)[5]. Elementary texts on equilibrium theory – for example [22] (with its copious misprints, inaccuracies – including the dating of the classic KKM theorem – and other infelicities) – pay at least lip service to what I have come to call the *Polish Fix Point Theorem* in proving the existence of an ADGE, and thereby the crucial role it plays in the *first* fundamental theorem of welfare economics (and in Negishi's method, see, below, next section)[6]; and reflections by

[4] I have in mind, here, the remarkable example of Helena Rasiowa (cf. [52], and refereneces therein). Indeed, a most readable and, although slightly dated by now, fairly comprehensive account of the contributions of Polish mathematicians in the interwar period – and the extraordinary role played by *Fundamenta Mathematiacae* in the dissemination of their pioneering contributions to set theory, topology and functional analysis – is also provided by a *woman scholar*, Sister Mary George Kuzawa (see [27] & [28]). Indeed, an unwritten part of the story of mathematical economics are the roles played by the mathematics disseminated by *Fundamenta Mathematicae* and *Composito Mathematica* - the latter founded by Brouwer, to counter his dismissal, by Hilbert, from the editorial borad of *Mathematische Annalen.* A fascinating description of the story of the founding and vicissitudes of *Composito Mathematica*, and Brouwer's active and tragic role in it, can be found in [73].

[5] Unlike many other historians of the development and evolution of mathematical economics, I do not subscribe to the view that *Bourbaki* was essential in this story. *Hilbert's Dogma* in proof theory and its metamathematics (see my recent paper, [80] for a fairly detailed definition and discussion) and von Neumann's *less than candid* acknowledgements to alternative trends in the possible mathematization of economics were the decisive factors in the eventual path and structure that was taken by orthodox mathematical economics.

[6] Few bread-and-butter mathematical economists and even fewer textbook writers seem to

the pioneers of mathematical economics (for example [12], pp. 268-9) emphasise the crucial role played by the Hahn-Banach theorem in demonstrating the (non-constructive) validity of the *second* fundamental theorem of welfare economics. Yet, it is von Neumann and Bourbaki who are considered the founding fathers of the mathematics and the mathematical methods of orthodox mathematical economics and formal economic theory - not the legions of interwar Polish mathematicians who framed, codified and formalised set theory, topology and functional analysis (to which the contributions of von Neumann and Bourbaki is a proverbial ε, at least in my opinion).

But the homage I wish to pay, particularly in the context of the computable and constructive philosophy and methodology underpinning the framework of this paper, is to the pioneering contributions by legions of great Polish recursion theorists, mathematical logicians and metamathematicians – all the way from Post in 1921 ([48]), via Mazur in (about) 1928 (see [43], especially chapter 6), Tarski in 1933 ([68]), culminating in the Banach-Mazur results of 1936-8, on computable analysis ([2]), fully documented, alas, only after WW II, in as late as 1963 ([34]).

A decent history of Polish contributions to computability theory, mathematical logic and metamathematics is not something that can be summarised in a brief section of a paper aimed at issues that are peripheral to deep questions of the development of scientific theories. I shall, therefore, confine myself to highlighting one specially important contribution, which can be said to be the fountainhead of computable economics.

In my various writings on *Computable Economics*, I have consistently maintained that the absolute pioneer of the subject, long before I gave it the name by which I am now referring to it, was Michael Rabin ([50]). His own starting point was the classic Gale-Stewart game ([16]). This line of research was later taken up by Jones and Matiyasevich, particularly in the context of the latter's fundamental results on the negative solution to Hilbert's Tenth Problem (see [33] and for a fairly full discussion also [74]). However, a proper study of the origins should really begin with the Banach-Mazur Game ([43]), show how the Gale-Stewart-Game is a special case of it, and then place the former in the context of the remarkable effectivisation achieved by Rabin. To this can be added the Polish contribution to finding a rationale for the axiom of determinateness to make such game determinate, and to free the subject from the 'shackles' of the axiom of choice (cf., [67]).

In this paper I shall not explore the possibilities offered by any of this line of research for reorienting the mathematization of economic theory in the direction of Computable Economics; nor will I be able to expand on using Computable Analysis, in the Polish tradition, for enriching algorithmic analysis of economic processes. But these are lines of research that are squarely within the discipline

be aware that the three classic results, the *Brouwer fix-point theorem*, *Sperner's Lemma* and *KKM*, are mutually equivalent in the sense that each one can be deduced from the other (see, [44]). Thus, in an elementary sense the proof of the non-constructivity of the one follows from that of any of the other two. This latter point has, to the best of my knowledge, never been acknowledged in the vast literature on mathematical economics.

of Computable Economics, and their Polish origins will be spelled out in greater detail in future work – by me and my collaborators and students.

Finally, I must acknowledge my debts as an economist to the remarkably original and stimulating works by three Polish economists of exceptional originality: Mihał Kalecki, Oskar Lange and Paul Rosenstein-Rodan. Almost all my work in macroeconomic dynamics, business cycle theory, growth and development theories were seriously inspired by the classic works by Kalecki and Rosenstein-Rodan in the early 1930s – and, then, through their various contributions in the 1940s and the quarter of a century after WW II. Lange's influential writings on general equilibrium theory, beginning with his contribution to the debate with Hayek, Ludwig von Mises, and others, on the socialist calculation debate, and subsequently through his work on welfare economics and general equilibrium theory inspired me, albeit negatively (compared to the way Kalecki and Rosenstein-Rodan stimulated me positively). I consider myself privileged to have been educated in Lund and Cambridge, where the classic articles by Kalecki, Lange and Rosenstein-Rodan were part of the assigned reading for graduate courses in macroeconomics, growth and cycle theories, development theory and general equilibrium theory. On reflection, now forty years after I began my education as an economist, it may not be an exaggeration to confess that – with the exception of the writings of the Swedish followers of Wicksell, the Norwegian 'School' of the successors of Frisch and Haavelmo and the Cambridge (both the 'old' and the 'new' Cambridges) 'descendents' of Keynes – the contributions by this trio of Kalecki, Lange and Rosenstein-Rodan have remained life-long sources of fertile ideas for me to reflect on, time and again.

I do not think I am being facetious when I also confess that Rosenstein-Rodan's variation of Marshall's unfortunate *epigraph* to his *Principles* ([54]), has been my intellectual motto – especially from the point of view of mathematical epistemology – after my 'conversion' to *Computable Economics*: *Natura Facit Saltum.*

3 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.*"
>
> [5], 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, etc. On the other hand, just because a fixed-point theorem is invalid from an intuitionistic point of view[7] 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 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[8]. 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[9]:

> "[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*[10] 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

[7] We are 'advised', in a recent advanced textbook in **Real Analysis with Economic Applications** ([42], 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).

[8] Explicit references to substantiate this claim can be found in [75] and [76], as well as in the sequel, below.

[9] 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.

[10] 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 [79].

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, and the *finite* numerical instances[11] with which they are, ostensibly, 'justified'. This epistemological deficit requires even 'deeper' mathematical and philosophical considerations in *Cantor's Paradise*[12] of ordinals[13] , 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.

4 Notes on the Epistemology of Computation in Economics

> "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 en-*

[11] Serényi's ([59]) very recent reflections and results on this issue will play an important part in the theoretical underpinnings to be developed in this project (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.*"

[12] Hilbert did not want to be driven out of '*Cantor's Paradise*' ([21]; 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 ([86]; 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." '

[13] 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.

> *courages us to ask questions about the limits of computability, about what we can and cannot know about the world around us.*"
>
> [14], p.xiii; italics added.

Feynman, with characteristic perspicacity, highlights the *epistemological* basis of computability – in contrast to the usual emphasis on the *philosophy* and *methodology* of recursion theory, especially in the applied sciences, whether mathematical, natural, biological, social, humanistic or even, self-referentially, in computer science itself. The great physicist, who combined a profound knowledge of the theoretical underpinnings of experimental physics and the mathematical basis of quantum mechanics, went on to embellish the above fruitful characterisation of computability with the important distinction between *knowing* and *proving*[14] (ibid, p.90; italics added):

> "The principle here is that *you can know a lot more than you can prove*! Unfortunately, it is also possible to think you know a lot more than you actually know. Hence the frequent need for proof."

This *epistemic deficit*, as one may call the gap between *knowing something* and *proving its truth*, even before investigating the *methods of knowing* and the *means of proving*, can be given a meaningful underpinning in terms of Michael Polanyi's famous notion of **Tacit Knowledge** ([47], p.4, italics in the original), especially when juxtaposed with Feynman's above points :

> "I shall reconsider human knowledge by starting from the fact that *we can know more than we can tell.*"

To this Polanyi adds the important caveat (ibid, p.7; italics added):

> "We have here examples of knowing, both of a more intellectual and more practical kind; both the 'wissen' and 'können' of the Germans, or the '*knowing what*' and the '*knowing how*' of Gilbert Ryle. These two aspects of knowing have a similar structure and *neither is ever present without the other.*"

The other side of this coin, from an economic and economist's point of view, is the current practice in mathematical economics and game theory, of 'proving' non-constructively the existence of a provably uncomputable equilibrium, first; then, at a second, entirely different stage, attempts are made to devise algorithms to locate the equilibrium. All of every variant of computable general equilibrium theory, algorithmic game theory and stochastic dynamic general equilibrium methods in macroeconomics, are completely schizophrenic in this sense.

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[14] Ultimately, in *Metamathematics*, between *truth* and *provability*, – or between *knowing* a proposition is *true* and being able to *prove*, formally, its *truth* – due mainly to the work of Gödel, Church and Turing (and Skolem). *Epistemology* – or, at least, *epistemics* – enters at the ground floor in the computability aspects of Metamathematics.

We are, perhaps, now ready to ask a simple, but obvious, question: **What is a computation**?[15], in the same sense in which Kant asked: **What is Man?** – and, then, proceeded to break it up into three sub-questions: *What can I know*? *What must I do*? *What may I hope*?

Analogously, we can subdivide the question on computation into three parts: *What* can be computed? What must be done (by *what*[16], to compute)? What can be *expected* (from a computation)?

In a sense there are simple, even simplistic, answers to these questions. A computation is that which is implementable via a Turing Machine. But that leads to further questions: are there other models of computation that are richer in some sense - in the nature of the data analysable, in the kind of processing speeds, in the class of computable functions, and so on. Mercifully, the *Church-Turing Thesis* obviates the need for any such elaboration. Note, this is a *Thesis*, not a *Theorem*. It came about as a result of trying to find a formal encapsulation of the intuitive notion of *effective calculability*. What is the difference between a Thesis and a Theorem? Perhaps one illuminating way to try to answer this question is to reflect on 'an imaginary interview between a modern mathematician [Professor X] and ... Descartes', devised by Rosser ([55], pp. 2-3; italics added), trying to decide and define precisely *the intuitive notion of continuity* in formal, mathematical, terms:

> "Professor X decides to acquaint Descartes (with the modern precise definition of continuity) [$\varepsilon - \delta$ definition] with the intention of persuading him to adopt it in place of *the vague intuitive idea* of tracing a curve without lifting the pencil from the paper. ....
>
> .. Professor X found Descartes very agreeable to his suggestions and quite willing to replace his vague idea of continuity by a precise one. However, Descartes raised one difficulty which Professor X had not foreseen. Descartes put it as follows:
>
> 'I have here an important concept which I shall call *continuity*. At present my notion of it is rather vague, not sufficiently vague that I cannot decide which curves are continuous, but too vague to permit of careful proofs. You are proposing a precise definition of this same notion. However, since my definition is too vague to be the basis for a careful proof, *how are we going to verify that my vague definition and your precise definition are definitions of the same thing*.'
>
> If by 'verify' Descartes meant 'prove', it obviously could not be done, since his definition was too vague for proof. If by 'verify' Descartes meant 'decide,' then it might be done, since his definition

[15] A discussion of this question can be found in the elegantly pedagogical essay by one of the great masters of computability theory in the post-Turing era: Martin Davis ([10]).

[16] By an *abacus*? By a *slide rule*? By a machine of the kind built by Babbage? By an analogue machine of the differential analyser type? By a servomechanism? By a human *computor*? By the *brain*? One of the most important results, against the backdrop of the *Church-Turing Thesis* – about which I will have more to say, below – of computability theory, is that whatever can be *effectively calculated* by any of these devices, can also be effected by a *Turing Machine*.

> was not too vague for purposes of coming to decision. Actually, Descartes and Professor X did finally decide that the two definitions were equivalent .. ."

How did they come to this conclusion? By comparing the classifications into continuous and discontinuous all those 'interesting curves' either of them could 'think of', using their own respective definitions – the intuitive and the (so-called) precise – and finding they resulted in identical characterisations. Thus, 'the evidence seemed "conclusive" that the two definitions were equivalent' (ibid, p.3).

However, even today we are aware that there are 'clear intuitive notions of *continuity* which cannot be [topologically i.e., using, for example the 'precise', $\varepsilon - \delta$ definition] defined'. (cf.[17], p.73).

Any and every computation that is implementable by a Turing Machine answers all such questions of the 'equivalence' between 'intuitive' notions of 'effective calculability' and formal definitions of computability unambiguously: every model of computation thus far formally defined – Turing Machines [70], Post's Machine, Church's $\lambda-$Calculus, General Recursiveness, the Shepherdson-Sturgis Register machines [60] – is formally equivalent to any other. This is the *epistemological content* [17] of the *Church-Turing Thesis* ([26], §62). As summarised by the classic and original definition of this concept by Kleene (op.cit, pp. 300-1)[18]:

- Any *general recursive function* (predicate) is *effectively calculable.*
- Every effectively calculable function (effectively decidable predicate) is general recursive.
- The '*Church[-Turing] Thesis*' is also implicit in the conception of a *computing machine* formulated by Turing and Post.

And, Kleene went on (ibid, pp. 317-8; italics added):

[17] It is this that is stressed by Gödel when he finally accepted the content of the Church-Turing Thesis ([18], p.84; italics added):

> "It seems to me that [the] importance [of Turing's computability] is largely due to the fact that with this concept one has for the first time succeeded in giving an *absolute definition* of an interesting *epistemological notion*, i.e., one not depending on the formalism chosen."

[18] Apart from the classic discussion in [26], the most lucid exposition and the clearest case for the intuitive (sic!) acceptance of the Church-Turing Thesis is, in my opinion, to be found in § 20 of Rózsa Péter's elegant book on *Recursive Functions, [46].* A typical argument proceeded as follows (*ibid*, p. 225; italics added):

> "The assertion that the values of a function are everywhere calculable in a finite number of steps has *meaning* only under the condition that this calculation *does not depend on some individual arbitrariness*, but constitutes a procedure capable of being *repeated and communicated to other people at any time. Hence it must be a mechanical procedure*, and thus one can imagine, in principle, a machine able to carry through the single steps of the calculation."

> "Since our original notion of effective calculability of a function (or of effective decidability of a predicate) is a somewhat *vague intuitive* one, *the thesis cannot be proved.*
>
> The intuitive notion however is real, in that it vouchsafes as effectively calculable many particular functions, .. and on the other hand *enables us to recognize that our knowledge about many other functions is insufficient to place them in the category of effectively calculable functions.*

Here, too, the epistemological role of a computation is explicitly recognised. Yet[19]:

> "Turing's analysis divides, ..., into a conceptual analysis and rigorous proof. The conceptual analysis leads *first* to a careful and sharper formulation of the intended informal concept, here, 'mechanical procedures carried out by a human computor', and *second* to the axiomatic formulation of determinacy, boundedness, and locality conditions. Turing's central thesis connects the informal notion and the axiomatically restricted one. Rigorous proof allows us then, *third*, to recognize that all the actions of an axiomatically restricted computor can be stimulated by a Turing machine. Thus, the analysis together with the proof allows us to 'replace' the boldly claimed **thesis**, **all effectively calculable functions are Turing computable**, by a carefully articulated argument that includes a sharpened informal notion and an axiomatically characterise one."
>
> [62], p.173; italics in the original; bold emphasis, added.

Finally, the 'duality' between effective calculability and effective undecidability, made explicit by the Church-Turing Thesis is described with characteristic and concise elegance by Rózsa Péter ([46], p. 254; italics in the original):

> "One of the most important applications of the [Church-Turing] thesis, making precise the concept of effectivitivity is the proof of the *effective undecidability of certain problems.*"

Mathematical economists and game theorists have never, to the best of my knowledge, made either *undecidability* or *unsolvability* (see essay 2) core concepts in their analytic framework. How could they? The orthodox mathematical economic and game theoretic framework is not developed against the backdrop of a model of computation; hence, the Church-Turing Thesis has no place in its foundations. However, of course, *ad hoc* considerations of uncomputability does

[19] I should add that this lucid and detailed analysis of the rigorous nature of the Church-Turing thesis also contains (*ibid*, § 6, pp. 173-176) an illuminating comparison of Dedekind's (successful, if measured by the standards of orthodox acceptance) definition of *continuity* by his '*construction*' of *sections*, to encapsulate the elusive intuitive notion, with Turing's brilliant three-part mode of deriving the equivalence between effective calculability and Turing Machine computation.

have a place in orthodox theory, albeit only in recent years. The closest to anything like this that has become part of the fabric of mathematical economics is Arrow's Impossibility Theorem - but devoid of computability or constructivity considerations (till very recently and, in general, in the sadly aborted research program of Alain Lewis).

There remains, of course, the notion of computation intrinsic to constructive mathematics, where there is no invoking of anything similar to a Church - Turing Thesis. I will have to leave any discussion of this important issue for another exercise. It means, of course, the answer to the question, 'What is a computation', may not be unambiguous!

5 A Concise Summary of the Evolution of the Mathematical Theory of Economic Policy[20]

> "[T]he story I am going to tell [is about] how macroeconomic policy and research changed as the result of the transformation of macroeconomics from constructing a system of equations of the national accounts to an investigation of *dynamic stochastic economies*. Before the transformation what is evaluated is a policy action given the current situation. .. After the transformation, what is evaluated is *a policy rule*.. Before the transformation, optimal policy selection was a matter of solving what the physical scientists called a control problem. .. After the transformation ... the time inconsistency of optimal plans necessitates *following rules*."
>
> [49], pp.370, 372-4; italics added.

It may be useful to record the origins of this approach[21]. In the early 1930s, the Social Democratic Minister of Finance of a Sweden grappling, like most other economies, with the ravages wrought in the labour market by the 'great depression', was Ernst Wigforss. He approached the two leading Swedish economists, Gunnar Myrdal and Erik Lindahl, both sympathtic to the political philosophy of the Social Democrats, and requested them to provide him with a 'theory for the underbalancing of the budget' so that he can justify the policy measures he was planning to implement to combat unemployment due to insufficent effective demand. He needed a 'theory', he told them, because the leader of the oppositon in that Parliament was the Professor of Economics at Stockholm University, Gösta Bagge, who was versed only in a theory that would justify a balanced budget. Thus was born, via the framework devised in Myrdal's famous

[20] This section is a drastically simplified and concise summary of the contents of my recent invited lecture in honour of Geoff Harcourt, delivered at the *Cambridge Journal of Economics* conference, held on 25/26, June, 2011, at Robinson College, Cambridge. The full version will be published in the Proceedigs of the Conference, to be published in the CJE, in 2012.

[21] It was narrated to me by Mrs Gertrud Lindahl, during personal conversations at her home in Lund, in 1983. I was, then, working in the 'Lindahl archives', which was nothing more than her personally ordered, immaculately organised, collections of her late husband's rich contributions to economic theory and the theory of policy, among other things.

memorandum to Wigforss ([35]), the classical theory[22] of economic policy, made mathematically formal, first, by Ragnar Frisch, Jan Tinbergen and Bent Hansen and famously known as the 'target-instrument' approach. It was built on the essential back of the 'paradox of saving', the main macroeconomic repository of the wedge between 'wholes' and 'parts' that makes a mockery of reductionism.

If this was the origin[23] – weaved into the fabric of, and underpinned by, the works of their great master, Knut Wicksell, long before Keynesian theory conquered all before it – its apotheosis was summarised in [45][24]. In between all and sundry had forgotten Lindahl's wise reflection:

> "[T]he papers [on monetary policy] by Henry C. Simons and Milton Friedman, although intellectually interesting as attempts to solve *the insoluble problem of almost entirely substituting rules for authorities*, do not give much guidance for the realization of this aim. Even if one highly sympathizes with the idea that the monetary authorities should be bound by *certain rules* – in the first place to maintain a stable price level – one feels the problem must be taken up in a more practical way."
>
> [30], p. 507; italics added.

The elevation of 'rules' to the status of a 'holy cow', replacing 'fine tuning', was at the altar of the two fundamental theorems of welfare economics and, hence, sanctified by general equilibrium theory (as substantiated in the implicit and explicit formalizations in the next section), which made them – 'rules' – intrinsically and naturally uncomputable and non-constructifiable, making them useless for actual policy implementations.

In the chronological order in which the mathematization of the theory of policy proceeded could be schematised in the following six-part sequence:

- Targets-Instruments/Static-Dynamic
- Proportional/Derivative/Integral Stabilization Policies
- Optimal Policies
- Policy Ineffectiveness/Credibility/Time Inconsistency

[22] By the 'classical theory of economic policy' I am not referring to the theory of policy of the *classical economists*.

[23] Which, even when circumscribed by the discipline of mathematical formalism, is not entirely acceptable simply because, almost as always, Ramsey was there, before all this [51], and had introduced the use of the calculus of variation. From this to the routine application of optimal control theory and dynamic programming – whether in their stochastic, filtering, versions, or not – in neoclassical growth models and the drivation of optimal 'rules' was a trivial step.

[24] The authors of this elegantly written book, written at the height of the faith in fine-tuning within a framework of optimal stochastic control, were blissfully unaware that they were tolling a bell that rang hollow – in that the 'Lucasian revolution' in policy nihilism was already in full swing.

- Inefficiency of Policy in an Intertemporal (Overlapping) Equilibrium Model – *Nonlinear Dynamics*

- Efficiency of Policy Underpinned by the Fundamental Theorems of Welfare Economics

The mathematical underpinnings were, in turn, provided by (at least): the Calculus of Variations, Mathematical Programming, Ordinary Differential Equations (ODEs)/Dynamical Systems Theory, Controllability/Stabilizability, Optimal Control Theory/Stochastic Optimal Control Theory, Dynamic Programming, Markov Decision Processes, Kalman Filtering, Separating Hyperplane Theorems/ Hahn-Banach Theorem and Fixed Point Theorems/Uzawa's Equivalence Theorem

The triptych of Dynamic Programming, Markov Decision Processes and Kalman Filtering, as mathematical tools and framework, with which and within which, to underpin newclassical macroeconomics, as *Recursive*[25] *Macroeconomics* was achieved with deceptive elegance by Sargent ([31]). In the next section it is shown, formally, that the underpinning of policy in DSGE models, via reliance on the fundamental theorems of welfare economics, using the last two sets of mathematical tools – separating hyperplane theorems/ Hahn-Banach theorems and fixed-point theorems/Uzawa equivalence theorem – is untenable from computable and constructive mathematical points of view. It is equally feasible to do the same for the other mathematical tools. For example, it is little understood by those who routinely appeal to existence/uniqueness theorems for solutions of ODEs – including, at the frontiers, to 'fashionable' nonlinear dynamics of the 'chaotic' variety – that, without exception, these theorems, even the classical ones, are intrinsically uncomputable (almost always also non-constructive) and appeal, indiscriminately to Zorn's Lemma (or the Axiom of Choice). The paradigmatic case is provided by any appeal to the Arzela-Ascoli theorem or one or another of an ostensibly 'simple' contraction mapping theorem (say, in dynamic programming formalisations). Using the Arzela-Ascoli theorem in *computing* approximations to the *unique* solution to an ODE, is exactly analogous to the use of *Sperner's Lemma* to construct a sequence of converging meshes to determine a Walrasian equilibrium by using, for example, Scarf's algorithm (see the next section). That I don't attempt these exercises in this paper is only due to the fact that their reliance on the fundamental theorems of welfare economics is less decisive – as well as their underpinning in the DSGE methodology.

6 Six 'Impossible' Computable and Constructive Claims in *ADGE*, *CGE*, *RCE* & *ABE*

Alice: There is no use trying; one can't believe impossible things.

[25] This notion of 'recursive' has nothing whatsoever to do with formal *recursion theory* (*computability* theory).

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 'six 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.

6.1 *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."
>
> [56], 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' ([56], 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)[26]. Instead, the sources of non-constructivity are the undecidable disjunctions - i.e., appeal to the *law of the excluded middle* in

[26] In [57], 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)."

infinitary instances - intrinsic to the choice of a convergent subsequence in the use of the Bolzano-Weierstrass theorem[27] 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 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 1 & 2) 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[28].

6.2 Scarf's Fixed Point Algorithms are Nonconstructive

The economic foundations of **CGE** models lie in *Uzawa's Equivalence Theorem* ([72], [11], p.719, ff); the mathematical foundations are underpinned by

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.

[27] 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

[28] Scarf uses, in addition, proof by contradiction where, implicitly, LEM (*tertium non datur*) is also invoked in the context of an infinitary instance (cf. [57], pp. 1026-7).

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 ([61]), the following explicit claim is made:

> "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 subsection 4.

6.3 *Negishi's Method* is Non-Constructive

> "The *method of proof* used in this essay [i.e., in [37]] has been found useful also for such problems as equilibrium in infinite dimensional space and *computation of equilibria.*"

[39], p. xiv; italics added.

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

A pithy 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* ([72]) and devising an algorithm for the excess demand functions that map a price simplex into itself to determine the fixed point ([56]).

- 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* ([4], [82], and [25][29]).

- 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.

Before proceeding any further, I should add that I am in the happy position of being able to refer the interested reader to a scholarly survey of Negishi's work. Takashi Negishi's outstanding 'contributions to economic analysis' are brilliantly and comprehensively surveyed by Warren Young in his recent paper

[29] 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 [37] in [39].

([87]). Young's paper provides a particularly appropriate background to the issues I tackle here. It – Young's paper – is especially relevant also because his elegant summary of Negishi's 'contribution to economic analysis' identifies [37] as one of the two crucial pillars[30] 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, that of (1960) [37] and 1961 [38], 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.* Young's perceptive - and, in my opinion, entirely correct - identification of the crucial role played by Negishi (1960) 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., [24], 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 [56]), 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., [7], p. 138, and above). 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 theorems 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):

[30] The other one being [38]. I am in full agreement with Young's important observation that it is [37] that is more important, which is why I have added italics to the phrase 'almost as influential', in the above quote.

> "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 ([40], 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 ([37]), 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[31]!

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 (see below). Secondly, all – to the best of my knowledge – of the current algorithms utilised in CGE, ACGE and DSGE modelling appeal to undecidable disjunctions and

[31] 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 distinguished 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 economics.

are effectively meaningless from a computablity point of view. Thirdly, and most importantly, *Negishi's theorem*[32] is, itself, *proved* nonconstructively.

There are two theorems in [37]. 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)[33].

Proposition 3 *The Proof of the Existence of Maximising Welfare Weights in the Negishi Theorem is Nonconstructive*

Remark 4 *Negishi's proof relies on satisfying the Slater (Complementary) Slackness Conditions ([64][34]). Slater's proof[35] of these conditions invoke the Kakutani fixed point theorem (Theorem 1 in [25]), 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., [5]).*

Proposition 5 *The vector of maximising welfare weights, derived in the Negishi Theorem, is uncomputable*

Remark 6 *A straightforward implication of Claim 1*

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 ([53], p. 125; italics added):

> "Even those who like algorithms have remarkably little appreciation of the thoroughgoing algorithmic thinking that is required for

[32] *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 [87] – is *not* free 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' (see, again, [7]).

[33] 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 aesthetics and mathematical philosophy, I shall refer to my two main results as 'Propositions' and their plausible validity as 'Remarks', and not as 'Theorems' and 'proofs', respectively.

[34] 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*!

[35] 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 Negishi ([37]) is aware of them is clear from his *Assumption 2* and *Lemma 1.*

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 [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."

6.4 The Uzawa Equivalence Theorem

The Uzawa Equivalence theorem is the fulcrum around which the *theory* of **CGE** modelling revolves. This key theorem[36] 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 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., [72], p. 60 or [65], p. 136).

Theorem 7 *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;

[36] 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 ([9]) and Starr ([65]), even refer to Uzawa's theorem.

iii). $p.X(p) = 0, \forall p \in S$ (Walras' Law holds: $\sum_{i=1}^{n} p_i x_i(p) = 0$, $\forall p \in S$)[37]

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[38] a continuous excess demand function, $X(p)$, satisfying Walras' Law (and homogeneity), from an arbitrary continuous function, say $f(.) : S \rightarrow S$, 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, $f(.) : S \rightarrow S$ 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)] 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.

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

[38] 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.

Theorem 8 *$X(p^*)$, 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 $f(.) : S \to S$, 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 $f(.) : S \to S$, 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 9 *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 10 *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.*

6.5 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', [29], p.168. However, go on Kydland and Prescott:

> "Their approach is ill-suited for the general equilibrium modelling of business fluctuations because dynamics and uncertainty are

crucial to any model that attempts to study business cycles. 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[39] 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."
>
> [8], 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, 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 that *a competitive equilibrium is Pareto optimal.* A textbook formulation of the theorem is as follows ([65], p. 145):

Theorem 11 *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 associated individual consumption bundles, and let y^{0j}, $j \in F$,be the associated firm supply vectors. Then ω^{0i} is Pareto efficient.*

[39] 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.

where:

F : set of firms.

Proof. See [65], p. 145-6. ■

Remark 12 *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[40].

Theorem 13 *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 $A \cap B = \emptyset$;

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 $u_i : X_i \to \Re$;

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);

[40] 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 ([41], pp. 85-91). Again, compare the difference between the 'classical' IVT and the constructive IVT to get a feel for the role of ε.

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. $\forall i, u_i : X_i \to \Re$ *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 14 *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* $\phi : S \to \Re$, *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'[41].

The Hahn-Banach theorem does have a constructive version, but only on subspaces of *separable* normed spaces. The standard, 'classical' version, valid on nonseparable normed spaces depends on *Zorn's Lemma* which is, of course, equivalent to the axiom of choice, and is therefore, non-constructive[42].

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*

[41] 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 ([72]), has not been shown.

[42] 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 [36]. 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 ([58]).

– i.e., normed spaces that have a countable dense subset. *Little is lost in restricting one's attention to separable spaces*[43]*, 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.

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 15 *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**.

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

6.6 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* [71], von Neumann on *The Theory of Self-Reproducing Automata* ([83]), and Ulam on *Nonlinear dynamics* ([15], [66]). A 'second generation' of pioneers were Conway ([3]) and Wolfram [85]), 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 ([19]), 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 ([63]).

Sadly, none of these classics have had the slightest impact on the current frontiers of agent-based computational economics (see, for example, [69]). 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* ([69], 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 explored 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. [84]) – 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 [13]).

No better characterisation of the practice of agent-based computational economists can be given that the one Arthur Burks gave (cf. [6], 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 questions – all of which are, in general, subject to algorithmic undecidabilities.

7 Towards a Computable Approach to Economic Modelling

> "This is a lecture on music which is **indeterminate** with regard to its performance. ..... . The *Music of Changes* is not an example. In the *Music of Changes*, structure, which is the division of the whole into parts; method, which is the note-to-note procedure; form, which is the expressive content, the morphology of the continuity; and materials, the sounds and silences of the composition, are all determined. Though no two performances of the *Music of Changes* will be identical . . ., two performances will resemble one another closely. Though chance operations brought about the determination of the composition, these operations are not available in its performance. . . . The *Music of Changes* is an object more inhuman than human. . . .The fact that **these things that constitute it**, though only sounds, **have come together to control a human being**, **the performer**, **gives the work the alarming quality of a Frankenstein monster.** This situation is of course characteristic of Western music, **the masterpieces of which are its most frightening examples, which when concerned with human communication only move from Frankenstein monster to Dictator.**
>
> John Cage: *Indeterminacy*; bold emphasis, added[44]

'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'. 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

[44] Lecture on *Indeterminacy*, p. 31, in: **Silence** (Wesleyan University Press, Middletown, CT, 1961).

> 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[45]. *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."
>
> [20], pp.154-5, italics added.

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? Analogous to Guckenheimer's thought experiment, if the data set generated by the economy as a computer is recursively enumerable but not recursive, inferences about 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.

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. This is the natural mode of interaction between the economy and the classical behavioural economist – i.e., Herbert Simon's version of behavioural economics (see [77]) – and the computable economist (see [78]); it is not the natural mode for the CGE theorist, nor for the agent-based computational economist. This is why

[45] 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.

there is a serious epistemological deficit in the practice of the latter two classes of economists.

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