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COMPUTATION IN ECONOMICS[♥]

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♣ For further information please contact Selda Kao or V. Ragupathy at: assru.tm@gmail.com or see http://www.assru.economia.unitn.it/. The ASSRU logo depicts a *Counting table* (*woodcut* probably from Strasbourg). The spaces between the lines function as the wires on an *abacus*. The place value is marked at the end.

♥ Forthcoming in: The Elgar Companion to Recent Economic Methodology, edited by John Davis & Wade Hands, Edward Elgar Publishing, Cheltenham, Glos., & Northampton, MA, (2011). We are greatly indebted to the Editors for the kind invitation to contribute and the immense patience with which they tolerated the various ways in which we transcended generous deadlines. The title has metamorphosed into the ultra-simple final form it has taken, having begun its life as Computational Economics, become the Computational Paradigm in Economics, then Computational Economics, Computable General Equilibrium Theory & Computable Economics and, finally, Classical Behavioural Economics, Computable General Equilibrium Theory, Computable Economics and Agent-Based Computational Economics. Each of the transitional titles seemed, at least to the authors, of emphasizing particular kinds of ways the notion of machine computation, and its underpinning theory, were implemented in a variety of economic theories. To avoid any such connotation it seemed best to choose as neutral a title as possible, without losing focus on the main theme which is, of course, the foundations of the *methodology of computing in economics*. We are deeply indebted to our two graduate students, *Selda Kao* and *V. Ragupathy*, for invaluable logistical and intellectual help. *Alas, they refuse to take any blame for the remaining infelicities*.

Abstract

This is an attempt at a succinct survey, from methodological and epistemological perspectives, of the burgeoning, apparently unstructured, field of what is often – misleadingly – referred to as *computational economics*. We identify and characterise four frontier research fields, encompassing both micro and macro aspects of economic theory, where machine computation play crucial roles in formal modelling exercises: *algorithmic behavioural economics*, *computable general equilibrium theory*, *agent based computational economics* and *computable economics*. In some senses these four research frontiers raise, without resolving, many interesting methodological and epistemological issues in economic theorising in (alternative) mathematical modes.

Keywords: *Classical Behavioural Economics*, *Computable General Equilibrium theory*, *Agent Based Economics*, *Computable Economics*, *Computability*, *Constructivity*, *Numerical Analysis*.

1. A Preamble

> "Computing is integral to science – not just as a tool for analyzing data, but as *an agent of thought and discovery*."
> [20], p. 369, italics added.

No one economist – although he was more than *just* an economist – considered, modelled and implemented the idea of '*computing ..... as an agent of thought and discovery*' better or more systematically, in human problem solving, organization theory, decision making in economics, models of discovery, evolutionary dynamics, and much else of core relevance to economic theory, than Herbert Simon [1]. In these two senses computing is clearly an epistemic and epistemological agent. On the other hand, the computer is undoubtedly also a 'tool for analyzing data', an aspect precisely and perceptively characterised by Richard Stone and Alan Brown in their pioneering work on *A Computable Model of Growth*[2]:

> Our approach is quantitative because economic life is largely concerned with quantities. We use computers because they are the best means that exist for answering the questions we ask. It is our responsibility to formulate the questions and get together the data which the *computer* needs to answer them.
> [79], p.viii; italics in original.

Remarkably – though not unexpectedly, at least to us[3] – thirty four years later,

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[1]When we refer to *Classical Behavioural Economics*, it is to the kind of computationally underpinned research program in these fields broached by Simon that will be meant (see [93] & [43], [47], [63], [65], [66], [67], [68], [69], [70], [71], [72], [73]).

[2]It is little recognized by one wing of so-called computational economists (eg., [22]) that the research program of the *Cambridge Growth Project* under the direction of Richard Stone emerged independently of – even prior to – Johansen's justly famous work on a computational *Multi-Sectoral Growth Model* (cf., [35]).

[3]'Not unexpectedly' because newclassical scholarship on traditions and foundations – whether of the doctrine historical variety in economics or of knowledge of mathematical traditions – is both selectively doctrinaire and unusually narrow, bordering on being comprehensively *ahistorical*.

we find two of the undisputed pioneers of real business cycle (RBC) theory, the core constituent of the *Stochastic Dynamic General Equilibrium*[4] (SDGE) model, considered one of the two dominant, frontier, 'schools' of macroeconomics, defining and asserting the meaning of *a computational experiment* in economics as follows ([40], p.67:

> "In a computational experiment, the researcher starts by posing a well-defined *quantitative question*. Then the researcher uses both theory and measurement to construct a model economy that is a *computer representation* of a national economy. ..... The researcher then calibrates the model economy so that it mimics the world along a carefully specified set of dimensions. Finally, the *computer* is used to run experiments that answer the question."

Enormous developments in the theoretical and practical technology of the computer have made a tremendous impact on *economic methodology* in general, but also in economic theory in particular. It must be emphasised that these references are to the *digital computer*. There are also *analog* and *hybrid* computers[5] that can be harnessed for service by the economist [6] – or any other analyst, in many other fields – to realise the intentions indicated by

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[4]We prefer what we think is the more descriptively correct *Recursive Macroeconomics* (see [45]) for this 'school' (in the sense of [52]) macroeconomics. The recursive in this description and encapsulation of *Newclassical Macroeconomic methodology* refers to the notion of *recursion in the sense of intuitive iteration* that underpins *Filtering*, *Markov Decision Processes* and *Dynamic Programming* associated with the names of Kalman, Wald and Bellman. The rational agent is, thus, formally equivalent to an optimal signal processor in Newclassical Macroeconomics. This should be contrasted with Simon's computational behavioural agent as an *Information Processing System* (IPS) and the *algorithmically rational agent* (ARA) of computable and constructive economics. The latter two notions are grounded in formal recursion theory or constructive mathematics. The notions of recursive and iteration in Recursive Macroeconomics have nothing whatsoever to do with the rigorous notion of recursive, recursion and iteration in Recursion Theory, Constructive Mathematics.

[5]Not to mention *quantum*, *DNA* and other physical and natural computers that are beginning to be realised at the frontiers of theoretical technology.

[6]Charles Babbage, viewed in one of his many incarnations as an economist, can be considered the only one to have straddled both the digital and analog traditions. There is a story to be

Stone & Brown and Kydland & Prescott, as well as to act as 'an agent of thought and discovery'. Indeed, in many ways, the analog computer should be more suitable for the purposes of the economic analysts simply because theorising in economics is in terms of *real numbers* and the underpinning mathematics is, almost without exception, in terms of *real analysis*[7]. The seemingly simple observations above capture one of a handful of insightful visions that the ubiquity of the computer has conferred upon the *intellectual adventurer* in economics – in particular the epistemically oriented economist. Stone & Brown and Kydland & Prescott seem to appeal to the raw *quantitative* economic analyst to respect the language and architecture of the computer in pursuing precise numerical investigations in economics.

However, as noted above, economic theorists tend to 'formulate the questions' in the language of a mathematics that the *digital* computer does *not* understand - real analysis - but 'get together the data' that it does, because the natural form in which economic data appears or is constructed is in terms of integer, natural or rational numbers. The transition between these two domains remains a proverbial black box, the interior of which is occasionally viewed, using the lenses of *numerical analysis*, *recursion theory* or *constructive mathematics*. With the possible exception of the core of economic theory, i.e., general equilibrium theory, in its incarnation as *computable general equilibrium* (CGE) theory, and the newer fields of computable and constructive economics,

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told here, but this is not the forum for it. We shall reserve the story for another occasion.

[7]Real analysis, as used by the mathematical economist, in turn, founded on set theory plus the

there have been no systematic attempts to develop any aspect of economics in such a way as to be consistent with the use of the computer, respecting its mathematical, numerical and, hence, also its *epistemological* – 'as an agent of thought and discovery' – constraints.

Somewhat surprisingly, the adherents and aficionados of Leif Johansen's classic work on A Multi-Sectoral Study of Economic Growth ([35]) claim that this was '*the first CGE model*' ([22], p.6)[8]. Their rationale for this claim is the following (p. 6; last two italics, added):

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

This is an untenable claim[9]; but we will not attempt at a substantiation of our counter-claim in this paper, reserving it for a different, more focused, exercise. Here our aim is to structure and organise the computing tradition in economics in the age of the digital computer. For this reason, we accept, *pro*

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axiom of choice, whether explicitly acknowledged or not.

[8]This claim is repeated in a curiously uninformed and seriously incomplete expository chapter on *Computational Economics* by Paul Humphreys in an otherwise prestigious recent 'Handbook' ([34]).

9 Our stance on this issue is reflected exactly by the view held by our friend, Lance Taylor. After attending the recent 50th anniversary celebrations of the Johansen Model, held in Oslo, Lance wrote as follows to the first author (E-mail, 27 August, 2010; italics added):

> " [Most participants at the] conference in honor of the 50th anniversary of Johansen's MSG model [held in Oslo in May, were] thinking that Leif was taking off from Arrow-Debreu when in fact he was doing disaggregated macro planning, moving around the numbers in a set of accounts that they had been constructed to satisfy. There is certainly no mention of A-D in his book."

tempore, this claim by Dixon and Parmenter (and like-minded economists). Yet, we cannot refrain from raising at least half-an-eyebrow at the notion of equating the notion of *computable* with *numerical*. This was not the equivalence that underpinned the way the Arrow-Debreu General Equilibrium (ADGE) was turned into the formal, rigorous, CGE model of Scarf, which will be discussed below in section 4.

In footnote 2, above, we claimed that the *Computable*[10] *Growth Model* that was being developed at the Department of Applied Economics, at Cambridge, within the *Cambridge Growth Project* framework under the direction of Richard Stone, was one that was parallel in aims and structure to the Johansen exercise. Surely, then, this model has a claim to be a 'joint first' CGE exercise, with the Johansen MSG model (except for minor details on the way the 'price and income elasticities' were derived)? Unsurprisingly, this claim, too, has been asserted by no less an authority than Graham Pyatt ([54], p.246; italics added):

> "By the end of the [1950s], a new release of creative energies was evident with the launch of the Cambridge Growth Project .... The central idea was to synthesize demand analysis with input-output in an exercise which paralleled the work in Norway of Leif Johansen and *can be seen in retrospect as an immediate precursor of applied or general equilibrium models*."

This claim – that the Cambridge Growth Project work 'can be seen in retrospect as an immediate precursor of applied or general equilibrium

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[10] *Computable*, in this context, is simply *numerical* in the same sense in which it was referred to

models' – is at least as untenable as the previous claim by [22]. Here too, we accept this (untenable) claim, *pro tempore*, for the specific purpose of the aims of this chapter. Here, too, what 'emerged' as the applied general equilibrium modelling tradition, directly down the ADGE and (Scarf) CGE line, had nothing whatsoever to do with the way the Cambridge Growth Project modelling exercise was implemented. In the Cambridge Growth Project tradition – as well as the Johansen MSG exercise – the starting points were the necessary balances intrinsic to *Social Accounting Matrices* (SAM). The numerical methods that were used to iterate towards the necessary balances in a SAM did not imply the computability of the model as a whole; nor did it have anything to do with the theoretical – economic and mathematical – underpinnings in an ADGE model.

Suppose, now, we teach our students the rudiments of the mathematics of the digital computer - i.e., recursion theory and constructive mathematics - simultaneously with the mathematics of general equilibrium theory - i.e., real analysis. As a first, tentative, bridge between these three different kinds of mathematics, let us also add a small dose of lectures and tutorials on computable and constructive analysis, at least as a first exposure of the students to those results in these fields that have bearings at least on computable general equilibrium theory a la Scarf. Such a curriculum content will show that the claims and, in particular, the policy conclusions emanating from *applied general equilibrium* theory are based on untenable algorithmic

in [22], above.

mathematical foundations. This is true in spite of the systematic and impressive work of Herbert Scarf, Rolf Mantel and others who have sought to develop some allegedly constructive[11] and computable foundations in core areas of general equilibrium theory. In this precise mathematical sense, the epistemic and epistemological status of applied claims and assertions – for example, in policy domains, especially with anchorings in one or both of the fundamental theorems of welfare economics – are, to put it mildly, questionable. Some of these points are discussed in section 4, below.

In this paper we have decided to eschew any description or discussion of the various uses of numerical methods in economic analysis. We have *computable* reasons for this decision. Most of the models of economic analysis – whether micro or macro, game theory of IO – are founded on real analysis underpinned by set theory plus the axiom of choice. Results obtained in this framework are seriously deficient in numerical content. To infuse numerical content via numerical methods do not make the theories computational in any rigorous sense. In fact, there is – provably – almost no meaningfully approximate connection between a 'rigorously' proved, say, equilibrium, and its numerically computed approximation – despite many claims to the contrary, even at the frontiers of economic analysis.

[11] I should mention that Douglas Bridges, a mathematician with impeccable constructive credentials, made a couple of valiant attempts, one of them with Fred Richman, to infuse a serious and rigorous dose of constructivism at the most fundamental level of mathematical economics (cf: [11], [12]).

In the spectacular developments achieved in dynamical systems theory in the second half of the 20th century, the digital computer played a decisive part. However, there is a close connection between algorithms and dynamical systems via numerical analysis. The use of the digital computer to study continuous dynamical systems requires the analyst or the experimenter to first discretise the system to be studied. The discretisation processes for nonlinear dynamical systems are often intractable and undecidable. On the other hand, paradoxically, until very recently the mathematical foundation for numerical analysis was not developed in a way that was consistent with the mathematical foundation of the digital computer - i.e., computability theory. As a result we have, in economics, a plethora of attempts and claims about *computational economics* that are not well founded on recursion theoretic, constructive or a numerical analysis based on formal algorithmic foundations.

Now, there are at least two ways out of the dilemma faced by the *computational economist*. Either be rigorous about the theory of approximations and numerical analysis in discretising the continuous; or, look for a mathematical foundation for numerical analysis taking heed of the following observations remarks by Blum, et.al (to which we will refer as *BCSS*):

> "There is a substantial conflict between theoretical computer science and numerical analysis. These two subjects with common goals have grown apart. For example, computer scientists are uneasy with calculus, whereas numerical analysis thrives on it. On the other hand numerical analysts see *no use for the Turing machine.*
> The conflict has at its roots another age-old *conflict*, that *between the continuous and the discrete.* Computer science is oriented by the digital nature of machines and by its discrete foundations given by Turing machines. For numerical analysis, systems of equations and differential

equations are central and this discipline depends heavily on the continuous nature of the real numbers. ...
Use of Turing machines yields a unifying concept of the algorithm well formalized. ....
The situation in numerical analysis is quite the opposite. Algorithms are primarily a means to solve practical problems. *There is not even a formal definition of algorithm in the subject*. ....
A major obstacle to reconciling scientific computation and computer science is the present view of the machine, that is, the digital computer. As long as the computer is seen simply as a finite or discrete subject, it will be difficult to systematize numerical analysis. We believe that the Turing machine as a foundation for real number algorithms can only obscure concepts.
Towards resolving the problem we have posed, we are led to expanding the theoretical model of the machine to allow real numbers as inputs."
[9], p.23; italics added.

This is a strategy that is a compromise between using an analog computer and a digital one, on the one hand, and, on the other, between accepting either constructive or computable analysis and classical real analysis. The model of computation developed with great ingenuity by Smale and his co-workers may well be the best way to retain much of classical mathematical economics while still being able to pose and answer meaningfully questions about decidability, computability and computational complexity - and to retain numerical meaning in the whole framework.

Yet, we are not convinced at all that the BCSS model is of much relevance to the issue of computable foundations for numerical analysis. Our reasons are as follows:

(a) What is wrong with the analogue model of computation over the reals and why was it not invoked to provide the mathematical and physical foundation for numerical analysis by the authors of the BCSS model? This is particularly relevant in this paper, given that a noble analogue computing tradition existed – and flourished – in economics, in different eras of the development of the subject.

(b) What is wrong with the computable and recursive analytic model, with its rich complexity theoretic analysis of classic optimization operators routinely used in economic theory (optimal control, dynamic programming, etc.,), of the perennial paradoxes of the initial value problem on ordinary differential equations and their solution complexities and of much else in a similar vein.

(c) We don't think there is any historical or analytical substance to the Newtonian vision frequently invoked as a backdrop against which to justify the need for a new mathematical foundation for numerical analysis.

(d) Finally, there is an important strand of research that has begun to interpret numerical algorithms as dynamical systems; from this kind of interpretation to a study of undecidability and incompleteness of numerical algorithms is an easy and fascinating frontier research topic within the framework of computable analysis, which owes nothing to - and has no need for - the BCSS kind of modelling framework – even though there are claims to the contrary in [9] regarding such issues[12]. This is also a point of relevance for the problem

[12]Primarily in relation to the decidability problems of the Mandelbrot and Julia sets, as posed

of being rigorous about the theory of approximations and numerical analysis in discretising the continuous. It is not just a question about accurate or rigorous discretizations of a real analytic model for implementation on a digital computer; but we will have to leave it at that, for now.

Therefore, against the general backdrop provided in this Preamble, we will concentrate only on the four areas of Algorithmic Behavioural Economics, Computable General Equilibrium Theory, Computable Economics and Agent Based Computational Economics in discussing the role of *Computation in Economics*. With this in mind, in section 2, an ultra-brief outline of The Computing Tradition in Economics is given. In sections 3, 4, 5 & 6, algorithmic behavioural economics, computable general equilibrium theory, computable economics and agent based computational economics are outlined and critically discussed, as afar as possible in methodological and epistemological terms. The concluding section, titled '*Towards an Algorithmic Approach to the Social Sciences*' is squarely epistemological in the vision we try to cultivate, from the lessons of approximately six decades of machine computing traditions in economics – both theoretical and applied.

Before concluding the 'preamble' it may be apposite to ask a simple, but obvious, question: **What is a computation**?[13] In a sense there is a simple, concise, answer to this question. A computation is that which is

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by Penrose, [51], p. 124, ff.

[13]A splendid and characteristically clear, simple – yet deep – discussion of this question can

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 analyzable, 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: any and every computation that is implementable by a Turing Machine answers all such questions unambiguously: every model of computation is formally equivalent with respect to each of these – and many other – questions. There remains, of course, the notion of computation intrinsic to constructive mathematics, where no invoking of anything similar to a Church-Turing Thesis. We 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 be unambiguous!

2. The *Machine* Computing Tradition in Economics

> The Method I take to do this, is not yet very usual; for instead of using only comparative and superlative Words, and intellectual Arguments, I have taken the course (as a Specimen of the Political Arithmetick I have long aimed at) to express my self in Terms of *Number, Weight, or Measure*; .....
> Now the Observations or Positions expressed by *Number, Weight, and Measure*, upon which I bottom the ensuing Discourses, are either true, or not apparently false, and which if they are not already true, certain, and evident, yet may be made so by the Sovereign Power, *Nam id certum est quod certum reddi potest*, and if they are false, not so false as to destroy the Argument they are brought for; but at worst are sufficient as Suppositions *to shew the way to that Knowledge I aim at*.
> William Petty, Preface to **Political Arithmetick** (3rd Edition), 1690[14]; italics (non Latin) added.

be found in [17].

[14]Accessed at: http://www.marxists.org/reference/subject/economics/petty/

Petty, *'to shew the way to that Knowledge [he] aimed at'* – i.e., for reasons of *epistemics* and *epistemology* – aimed 'to express [himself] in Terms of Number, Weight or Measure' – a tradition nobly inherited and resolutely preserved and enhanced by his Physiocratic and Classical Economic successors. Calculating, estimating, comparing, constructing and reasoning with numerical ratios, averages, series, tables areas, volumes and so on – in short, 'analyzing data', whether natural or artificial – underpinned much inference and some deduction is the way our classical and Physiocratic predecessors came to policy precepts. However, with the exception of Charles Babbage and, possibly, Jevons, till Irving Fisher ([27]), in 1891, constructed his 'remarkable hydraulic [analogue computing] apparatus for calculating equilibrium prices' ([10], p. 57)[15], resorting to *actually constructed machine models of computing* in economics seems to have remained an isolated example. Fisher's own description of the functioning of his hydraulic analogue computing machine clarifies an important feature of such computations: their independence from any intermediation via numerical analysis:

> "The [hydraulic] mechanism just described is the physical analogue of the ideal economic market[16]. The elements which contribute to the

[15] As Scarf, ([58], p. 207), points out: "In *Mathematical Investigations in the Theory of Value and Prices*, published in 1892, Irving Fisher described a mechanical and hydraulic analogue device intended to calculate equilibrium prices for a general competitive model. ...
At least two versions of Fisher's device were actually constructed and apparently performed successfully. ....
The equipment seems remarkably quaint and old-fashioned in this era of high-speed digital computers."

[16] In an early analogue approach to the study of macroeconomic dynamics, [81], p. 557, indicated the nature of what they mean by .analog., in these contexts (italics added):
"If a single group of equations can be written which defines the *assumed performance* for two separate systems (each of which within itself represents an orderly or definable behavior), one system may be called the complete analogue of the other."
Obviously, Fisher's system satisfies this condition - as would any analogue computing system,

> determination of prices are represented each with its appropriate rôle and open to the scrutiny of the eye. We are thus enabled not only to obtain a clear and analytical *picture* of the interdependence of the many elements in the causation of prices, but also to employ the mechanism as an instrument of investigation and by it, study some complicated variations which could scarcely be successfully followed without its aid."
> ([27], p.44, italics in original)

There were, of course, the famous computing machine *metaphors* used by Walras, Pareto – and, then, inspired by Barone, in the important 'Socialist Calculation Debate', most comprehensively summarised, both critically and constructively by Hayek ([32] & [33]). Lange, returning to the theme over thirty years later, in his Dobb Festschrift article on The Computer and the Market ([41]), muddied the issue by unscholarly and unsubstantiable claims for the possibilities of a digital computer (having, in the meanwhile, also forgotten that the initial discussions were with reference to analog computing machines and, in particular, the metaphor of the market as an analogue computer). None of the participants had any technical knowledge of the mathematical underpinnings of computing, in a sense understandably so, since the mathematical foundations of computing were being placed on a rigorous basis just during those very years that the debate was at its height[17].

Analogue computing techniques in economics had the proverbial still birth. There was a flurry of activities in the late 1940s and early 1950s, at the hands of A.W.H. (Bill) Phillips, Richard M. Goodwin, Herbert A. Simon, Robert H.

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by definition.

[17]Unless one expected such true economic scholars, before the kind of mathematization of economics that we are familiar with now, to be familiar with Brouwerian constructive mathematics, which was reaching its zenith, also during those very years.

Strotz, Otto Smith, Arnold Tustin, Roy Allen, Oscar Lange and a few others. Phillips built his famous *MONIAC*[18] hydraulic national income machine at the end of the 40s and it was used at many Universities - and even at the Central Bank of Guatemala - for teaching purposes and even as late as the early 70s Richard Goodwin, at Cambridge University, taught one of us elementary principles of coupled market dynamics using such a machine. Strotz and his associates, at Northwestern University, built electro-analogue machines to study inventory dynamics and nonlinear business cycle theories of the Hicks-Goodwin varieties. Otto Smith and R.M. Saunders, at the University of California at Berkeley, built an electro-analogue machine to study and simulate a Kalecki-type business cycle model. Roy Allen's successful textbooks on Macroeconomics and Mathematical Economics of the 50s - extending into the late 60s - contained pedagogical circuit devices modelling business cycle theories (cf: [2] especially chapter 9; and [3], especially chapter 18). Arnold Tustin's highly imaginative, but failed textbook attempt to familiarise economists with the use of servomechanism theory to build analogue machines as models of economic dynamics ([87]) and Oscar Lange's attractive, elementary, expository book with a similar purpose ([44]) also suffered the fate of 'stillbirth', at the dawn of the digital computing age.

Humphreys ([34]) refers to nonlinear business cycle theories[19] as examples of

[18]***Mo****netary* ***N****ational* ***I****ncome* ***Ac****counting Analogue Computing Machine.*

[19] In an earlier footnote we referred to this article as curiously uninformed and seriously incomplete! A concrete example of the reason for us to characterise it as such is his reference to [39] for references to 'nonlinear business cycle theories'. Anyone who takes seriously this

computational 'studies' that straddle 'the pre-computational era and the era of computational economics', claiming that 'there is no sharp divide between 'the two eras'. This claim can be substantiated by a more finessed study of the particular example of a canonical nonlinear business cycle equation, using – as was, indeed, actually done – analogue computing machines as in the 'pre-computational era' and comparing it with its study using a digital computing machine of the 'era of computational economics'.

The example we have chosen here encapsulates a noble tradition of *computation in economics* in every sense of this concept, to study a precisely specified mathematical system on both analogue and digital computers. It is, in a precise sense, also a substitute for an analytical study (because such a study is provably 'unlikely' to succeed in any meaningful way). Moreover, it can be viewed as an explicit example of an epistemological tool to interpret the results (most of which were unexpected), finally, to gain insight into the link between a computing machine and *its* theory and the theory of nonlinear dynamical systems. The latter point is turning out to be the most significant from the point of view of the epistemology of computation, since the interaction can only be explored by representing the one system by the other – and, therefore, even an exploration into a new domain: studying the

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kind of flippant, frivolous, reference and does check-up on Krugman's booklet, would and the strange claim (ibid, p. 7):

"I may be the only economist in my generation who has even heard of [these nonlinear business cycle theories]."

Krugman is 57 years old; we could easily list a dozen eminent economist of his generation, give or take a few years, who are seriously competent in nonlinear business cycle theories of repertoire of digital machine behaviour with analogue computing machines, and *vice versa*.

Consider, therefore, the following equation, representing a classical Keynesian nonlinear multiplier-accelerator model of the dynamics of national income, y:

$$\varepsilon\dot{y}(\mathrm{t}) + (1 - \alpha)y(t) = \phi[\dot{y}(t-\theta)] + \beta(t) + l(t) \tag{1}$$

Now, there are *at least* six different ways to investigate solutions to this nonlinear difference-differential equation:

- In old fashioned analytical modes;
- Using Non-standard analysis;
- Graphically, i.e., in terms of the geometry of dynamic behaviour, as usually done in the qualitative theory of differential equations;
- By the method of equivalent linearization;
- Using an electro-analogue[20] computer;
- Using digital computers;

It is, of course, only the last two alternatives that are of relevance in this

the Goodwin-Kaldor-Hicks era, developing it at some of the current frontiers of macroeconomic theory.

[20]In parallel, but slightly earlier, work of a related nature, [81], p. 557, indicated the nature of what they mean by .analog., in these contexts (italics added):

"If a single group of equations can be written which defines the *assumed performance* for two separate systems (each of which within itself represents an orderly or definable behavior), one system may be called the complete analogue of the other."

discussion. Assuming, for example, $\beta(t)+l(t)$ a constant [21] and reinterpreting $y(t)$ as a deviation from the unstable equilibrium of (1) $\frac{\beta(t)+l(t)}{1-\alpha}$ one obtains a mixed nonlinear difference-differential equation:

$$\varepsilon\dot{y}(t+\theta)+(1-\alpha)y(t+\theta)=\phi[\dot{y}(t)] \qquad (2)$$

In the first case, expanding (2) by a Taylor series approximation and *retaining only the first two terms*, one obtained the famous (unforced) *Rayleigh (- van der Pol) - type* equation: $\ddot{y}+\left[\frac{\chi(\dot{x})}{\dot{x}}\right]\dot{x}+x=0$ (3)

With this approximated reformulation began an 'industry' in the endogenous theory of the business cycle, where the cardinal desideratum was the existence of a unique, stable, limit cycle, independent of initial conditions. All four desiderata were violated when the approximations were more precise – in a purely technical sense – and the analysis proceeded via studies by means of analogue and digital computing machines. Even more interestingly, the insights obtained from an analogue computing machine study provided hints in setting up a computing study of (1) by means of digital computing machines.

Now, using an electro-analog computer, it was found, in [82], that the approximation of (1) retaining the first four terms of a Taylor series expansion,

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[21]If $\beta(t)+l(t)$ was not assumed a constant, the obdurate *forced* version of (3) would have to be confronted, without any hope of a disciplined solution even with the help of computing

generated twenty-five limit cycles, and a potential for a countable infinity of limit cycles with further higher order terms included in the approximations. Moreover, in its original formulation, one of the desired criteria for the nonlinear formulation of the endogenous model of the business cycle, was to generate self-sustaining fluctuations, *independent of initial conditions*. This latter property was lost when the approximation was made more precise.

Next, coupling two equations of type (3), via the Phillips Electro-Mechanical-Hydraulic Analogue Computing Machine ([30]), Goodwin and Phillips were able to generate – unexpectedly – the quasi-periodic paradox (cf., [1]). Neither Goodwin, nor Phillips, who did the coupled-dynamics computation on the Phillips Machine, had any clue – theoretical or otherwise – about interpreting and encapsulating this outcome in any economic theoretical formalization. The key point is that they were surprised by the outcome and did not know how to interpret it when it emerged. This is where the richness of the epistemology of computation manifests itself most dramatically. There was no macrodynamic theory to which they could relate the observed behaviour, which was contrary to expected behaviour.

Finally, one of us – Zambelli ([97] – repeated the exercise in [82], but this time on a *digital* computer. Our results came as a surprise to us: although we can confirm the results in [82], the outcomes are richer and more varied and we

machines, whether analog or digital.

would have no idea which way to proceed, if we are wedded to an equilibrium norm to which the results have to conform.

It goes without saying that one of he key differences between analogue and digital computing is that in the latter the intermediation between the continuous and the discrete is achieved by means of numerical procedures; this intermediation is circumvented in the analogue tradition. In this sense, there *is* a sharp difference between 'the pre-computational era and the era of computational economics'. Much of what is routinely referred to as computational economics in the modern era is simply variations on the theme of numerical analysis, without any anchoring in the mathematical theory of the computer, whether digital or analogue.

3. Classical Behavioural Economics

> "If we hurry, we can catch up to Turing on the path he pointed out to us so many years ago."
> [72], p. 101

*3.1. **A Brief Note on Classical vs. Modern Behavioural Economics***

Herbert Simon combined and encapsulated, in an intrinsically *dynamic, decison-theoretic* framework, a *computationally founded* system of *choice* and *decision*, both entirely rational in a broad sense. 'Computational' has always meant 'computable' in the Turing sense, at least in our reading of Simon's magisterial writings. In particular, in the context of bounded rationality, satisficing and their underpinnings in the architecture of human thinking, it was the path broached by Turing that guided Simon's fundamental contributions. In this section we try, in fairly precise and formal ways, to suggest *computable foundations* for *boundedly rational* choice and *satisficing* decisions. In a nutshell, the aim is to reformulate bounded rationality and satisficing *in a computable framework* so that their intrinsic (complex) dynamics is made explicit in as straightforward a way as possible.

Bounded rationality, satisficing and decision problems are the basic foundational pillars on which what we refer to as Classical or Algorithmic Behavioural Economics rests. A minor digression on the distinction between Classical or Algorithmic Behavioural Economics (CBE [22]) and Modern Behavioural Economics (MBE) may be useful to place the discussion in context.

The defining works of CBE were the three pioneering contributions by Herbert Simon (and his close, early, collaborators: Alan Newell and Cliff Shaw), [65], [66] and [74]. These three defining contributions to CBE were brought to an initial completion in the monumental book on *Human Problem Solving* by Newell and Simon, [47].

Meanwhile, almost simultaneously, contrary to current attributions, the seeds were laid by Ward Edwards for what is now an orthodox vision of Behavioural Economics – which we refer to as *Modern Behavioural Economics*

[22]We would have preferred to refer exclusively to Algorithmic Behavioural Economics and, therefore, ABE. However, ABE has become one of the usual ways to refer to Agent Based

(MBE)[23] – starting from the work of Leonard Savage, who had, himself, become a believer in De Finetti's approach to probability. The three defining, absolutely pioneering contributions, by Ward Edwards, works presaging the subsequent works by Nobel Laureate Daniel Kahneman (and Amos Twersky) on *Prospect Theory*, a key foundational basis for Modern Behavioural Economics, were, [24], [25] and [26][24].

Both traditions emerged from the infelicities in the axiomatic treatment of rationality that came to underpin expected utility maximization, emanating from the ground-breaking work of von Neuman-Morgenstern. Both found the framework and basis provide by von Neumann-Morgenstern wanting in realism – of a basic sort – and lacking in consistency in some of the underpinnings. For example, Edwards found the lack of consistency between a subjective theory of utility and an objective theory of probability that underpinned expected utility maximization. Edwards sought a 'reconciliation' via an appeal and a utilisation of the emerging De Finetti-based theory of subjective probability theory that Savage was developing just about at that time. The flaw detected, and perceptively tackled by Edwards, persists in the post-Prospect theory of behavioural

Economics; hence we opt for CBE.

[23]The beginning of *Modern Behavioural Economics* is generally identified with Thaler ([84]), for example by Camerer, Loewenstein and Rabin (cf., [15], p.xxii),

[24]A discerning reader would already have noticed that five of the six classic contributions were published in frontier Psychological Journals! One possibly obvious inference from this elementary observation may well be that the two classes of contributions emerged independently, focussing on those cognitive aspects that were neglected in more orthodox economic theory of decision making, by individual agents and in organisations. But this inference – we think – would be most misleading.

economics, now called Modern Behavioural Economics. These issues will be discussed, critically and exhaustively, in the relevant introductions to the respective volumes envisaged in this series.

Simon's starting point was *computational cognitive science*, in its psychological variants, and its confrontation with the theories of decision making economists were developing, applying and refining, all of which were variations on the theme of the von Neumann-Morgenstern starting point, further developed by Nash and Arrow-Debreu. The key notion was *computationally underpinning rational decision making*, thereby naturally and intrinsically taking into account *the theoretical limits that comes with computability theory*. In addition, this framework came with natural measures of computational complexity and they were imaginatively, and with great originality, incorporated into the kind of theories of decision making Simon developed within the formal frame work of what is called, in metamathematics and mathematical logic, *decision problems* (of which optimization is a special case).

From the line of research initiated with single-handed determination by Ward Edwards we have seen the emergence of modern Behavioural Economics, Behavioural Finance, Behavioural Game Theory and Behavioural Neuroeconomics.

From the work initiated by Herbert Simon, we have seen the emergence of rich and deep concepts like Bounded rationality and Satisficing and wholly refreshing fields like Evolutionary Growth Theory, at the hands of classical behavioural economists like Richard Nelson and Sidney Winter; adaptive economic dynamics by Richard Day; *Models of Discovery* by Simon and his many associates; the problem of causality and evolution in semi-decomposable systems by Simon and others; and much else.

*3.2. **Classical Behavioural Economics - Computable Foundations***

A *decision problem* asks whether there exists an *algorithm* to *decide* whether a mathematical assertion does or does not have a proof; or a formal problem does or does not have an algorithmic solution. Thus the characterization makes clear the crucial role of an underpinning *model of computation*; secondly, the answer is in the form of a *yes/no* response. Of course, there is the third alternative of '*undecidable*', too, but that is a vast issue outside the scope of this paper. It is in this sense of *decision problems* that we interpret the word 'decisions' here.

As for 'problem solving', we shall assume that this is to be interpreted in the sense in which it is defined and used in the monumental classic by Newell and Simon ([47]).

Finally, the *model of computation* is the *Turing model*, subject to the *Church-Turing Thesis*. To give a rigorous mathematical foundation for bounded rationality and satisficing, as decision problems[25], it is necessary to underpin them in a dynamic model of choice in a computable framework. However, these are not two separate problems. Any formalization underpinned by a model of computation in the sense of computability theory is, dually, intrinsically dynamic.

Remark1 *Decidable-Undecidable, Solvable-Unsolvable, Computable-Uncomputable, etc., are concepts that are given content algorithmically.*

Now consider the Boolean formula:

$$(x_1 \vee x_2 \vee x_3) \wedge (x_1 \vee \{\neg x_2\}) \wedge (x_2 \vee \{\neg x_3\}) \wedge (x_3 \vee \{\neg x_1\}) \wedge (\{\neg x_1\} \vee \{\neg x_2\} \vee \{\neg x_3\}) \qquad \text{---(4)}$$

Remark 2 *Each subformula within parenthesis is called a clause; the variables and their negations that constitute clauses are called literals; It is 'easy' to 'see' that for the truth value of the above Boolean formula to be* $t(x_i) = 1$ *all the subformulas within each of the parenthesis will have to be true. It is equally 'easy' to see that no truth assignments whatsoever can satisfy the formula such that its global value is true. This Boolean formula is unsatisfiable.*

---

[25] The three most important classes of decision problems that almost characterise the subject of computational complexity theory, underpinned by a model of computation in general, the model of computation in this context is the Nondeterministic Turing Machine are the P, NP and NP-Complete classes. Concisely, but not quite precisely, they can be described as follows:

[1] P denotes the class of computable problems that are solvable in time bounded by a polynomial function of the size of the input;

[2] NP is the class of computable problems for which a solution can be verified in polynomial time;

[3] A computable problem lies in the class called NP-Complete if every problem that is in NP can be reduced to it in polynomial time.

Problem 3 SAT – The Satisfiability Problem

Given m clauses, $C_i (i = 1,....m)$, containing the literals (of) $x_j (j = 1,....n)$, *determine* if the formula is $C_1 \wedge C_2 \wedge C_3 ... \wedge C_m$ *satisfiable*.

Determine means 'find an (efficient) algorithm'. To date it is not known whether there is an *efficient* algorithm to solve *the satisfiability problem* – i.e., to determine the truth value of a Boolean formula. In other words, it is not known whether $SAT \in P$ But:

Theorem 4 $SAT \in NP$

Definition 5 *A Boolean formula consisting of many clauses connected by conjunction (i.e., $\wedge$) is said to be in Conjunctive Normal Form (CNF).*

Finally, we have Cook's famous theorem:

Theorem 6 *Cook's Theorem*

SAT is ***NP–*** *Complete*

It is in the above kind of context and framework within which we are interpreting Simon's vision of behavioural economics. In this framework optimization is a very special case of the more general decision problem approach. The real mathematical content of *satisficing*[26] is best interpreted in terms of the satisfiability problem of computational complexity theory, the framework used by Simon consistently and persistently - and a framework to which he himself made pioneering contributions.

Finally, there is the computably underpinned definition of bounded rationality.

Theorem 7 *The process of rational choice by an economic agent is formally equivalent to the computing activity of a suitably programmed (Universal) Turing machine.*

Proof. By construction. See 3.2, pp. 29-36, *Computable Economics* [89]

Remark 8 *The important caveat is 'process' of rational choice, which Simon – more than anyone else – tirelessly emphasized by characterizing the difference between 'procedural' and 'substantive' rationality; the latter being the defining basis for Olympian rationality ([69], p.19), the former that of the computationally underpinned problem solver facing decision problems. Any decision – rational or not – has a time dimension and, hence, a content in terms of some process. In the Olympian model the 'process' aspect is submerged and dominated by the static*

[26] In [73], p. 295, Simon clarified the semantic sense of the word *satisfice*, by revealing the way he came to choose the word:
"The term 'satisfice', which appears in the *Oxford English Dictionary* as a Northumbrian synonym for 'satisfy', was borrowed for this new use by H. A. Simon (1956) in 'Rational Choice and the Structure of the Environment' [i.e., [66]]"

optimization operator, By transforming the agent into a problem solver, constrained by computational formalisms to determine a decision problem, Simon was able to extract the procedural content in any rational choice. The above result is a summary of such an approach.

Definition 9 *Computation Universality of a Dynamical System*

A dynamical system – discrete or continuous – is said to be capable of computation universality if, using its initial conditions, it can be programmed to simulate the activities of any arbitrary Turing Machine, in particular, the activities of a Universal Turing Machine.

Lemma 10 Dynamical Systems capable of Computation Universality can be constructed from Turing Machines

Proof. See [89].

Theorem 11 Non-Maximum Rational Choice

No trajectory of a dynamical system capable of universal computation can, in any 'useful sense' (see Samuelson's Nobel Prize lecture, [55]), be related to optimization in the Olympian model of rationality.

Proof. See [89]

Theorem 12 *Boundedly rational choice by an information processing agent within the framework of a decision problem is capable of computation universality.*

Proof. An immediate consequence of the definitions and theorems of this sub-section.

Remark 13 *From this result, in particular, it is clear that the Boundedly Rational Agent, satisficing in the context of a decision problem, encapsulates the only notion of rationality that can 'in any useful sense' be defined procedurally.*

We have only scratched a tiny part of the surface of the vast canvass on which Simon sketched his vision of a computably underpinned behavioural economics. Nothing in Simon's behavioural economics – i.e., in Classical Behavioural Economics – was devoid of computable content.

We should not end this subsection on Classical Behavioural Economics without also indicating where the framework we have developed falls short of encapsulating the deep and full force of Simon's visions. One important narrowness of vision in our approach is the concentration on *time* computational complexity. The key results here, which we have used above, are theorems 4 and 6, particularly the latter, i.e., Cooke's celebrated theorem that *SAT is NP-Complete*. Now, because SAT is NP-Complete, it is reasonable to believe that it is unsolvable with a polynomial time algorithm. On the other hand, SAT is solvable even with a linear *space* algorithm. The theorem in space computational complexity that corresponds to Cooke's fundamental theorem in time computational complexity is, arguably, **Savitch's Theorem** (see [93]). We have neglected this theorem and also did not discuss the implications of the following series of plausible -- not, as yet, entirely definite -- series of inclusion relations:

$$\mathbf{P \subseteq NP \sqsubseteq PSPACE = NPSPACE \sqsubseteq EXPTIME}$$

We should have asked ourselves the obvious question: Why didn't Herbert Simon ever occupy himself, ever, with the **P vs NP** question (one of the seven *Clay Millennium Problems*)? We think a plausible answer to this (counterfactual) question is that Simon was intrinsically more interested in *Space* Computational Complexity, as the domain in which human problem solving was best considered.

An additional subsection here should generalize the definition of satisficing in terms of the SAT problem in *space* computational complexity. When that task is undertaken it will be possible to go beyond *Chess* -- a paradigmatic canvas on which Simon sketched many of his conjectures on *human problem solving* -- and begin to try to study *GO* in terms of the notions of classical behavioural economics. This is especially and challengingly so because GO is known to be *PSPACE-hard*, but not known, as yet, to be *PSPACE-complete*.

4. Computable General Equilibrium Theory[27]

> "It is not natural for 'A implies B' to mean 'not A or B', and students will tell you so if you give them the chance. ... [W]e should not be surprised to find that certain classically accepted modes of inference are no longer correct. The most important of these is the principle of the excluded middle -- 'A or not A'. Constructively, this principle would mean that we had a method which, in finitely many purely routine steps, would lead to a proof or disproof of an arbitrary mathematical assertion A. Of course we have no such method, and nobody has the least hope that we ever shall. It is the principle of the excluded middle that accounts for almost all of the important unconstructivities of classical mathematics."
> [8], pp. 3, 10-11.

The main culprits – although *not* the only ones – in the failure of so-called *Computable General Equilibrium* (**CGE**) theory to be computable or constructive are the 'classically accepted modes of inference'. Unfortunately, to the best of our knowledge, *none* of the practitioners of **CGE**, nor any one of its 'offshoots' or alleged 'generalizations' – such as *Applied General Equilibrium* (**AGE**) theory, *Recursive Competitive Equilibrium* (**RCE**), or *Dynamic General Equilibrium* (**DGE**) theory – are either aware of the uncomputability and non-constructivity of their equilibria; *a fortiori*, they seem entirely uninterested in *why* this is so[28].

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27 Entirely for reasons of space we do not deal with the burgeoning field of *Algorithmic Game Theory* from the point of view of the methodology of computation as conceived in this paper. However, all of the strictures that are presented here 'against' the computable foundations of CGE apply, *pari passu*, to the claims and assertions of *Algorithmic Game Theory*. Computing the uncomputable, deciding the undecidable and completing the incompleatable is endemic in mathematical economics, of every variety.

28 Perhaps Fred Richman's perceptive reflection suggests the exact reason for these peculiar blinkers:

> "Even those who like algorithms have remarkably little appreciation of the thoroughgoing algorithmic thinking that is required for a constructive proof. This is illustrated by the nonconstructive nature of many proofs in books on numerical analysis, the theoretical study of practical numerical algorithms. I would guess that most realist mathematicians are unable even to recognize when a proof is constructive in the intuitionist's sense.
> It is a lot harder than one might think to recognize when a theorem depends on a nonconstructive argument. One reason is that proofs are rarely self-contained, but depend on other theorems whose proofs depend on still other theorems. These other

One of the great achievements of mathematical economics in the twentieth century was the Walrasian economic equilibrium existence proof of Arrow and Debreu ([5]). It is listed as the seventh of ten significant[29] achievements in *applied mathematics* in Piergiorgio Odifreddi's overall list of the 30 great solved problems of '*The Mathematical Century*' ([48]). Its extension to dynamics is listed as the eighth of 18 problems for the 21st century - in 'Hilbertian mode' - by Steve Smale ([76]). Given its undoubted and acknowledged significance in the intellectual canvas of 20th century mathematical economics, economic theory and applied mathematics, it is not surprising that attempts have been made, most notably by Herbert Scarf, to devise algorithmic methods to *compute* Arrow-Debreu equilibria. These attempts have resulted in the development of an independent discipline of *Computable General Equilibrium* (**CGE**) theory. It will not be an exaggeration to claim that, till Scarf's pioneering work on **CGE** theory and modelling, the Arrow-Debreu achievements remained in the realm of pure theory - whether of economics or mathematics; after Scarf, it is, surely, also a significant chapter in applied mathematics[30].

> 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." [55]

[29] #3.7 in chapter 3, pp. 122-5.

[30] Thus meriting inclusion in Odifreddi's list (op.cit) as a significant contribution to applied mathematics.

On the other hand, the key feature of the **CGE** research program is its schizophrenic nature: all of the mathematical economic theory of general equilibrium is practised in the domain of real analysis, and founded on *set theory* **plus** *the axiom of choice*. However, all of the computational content of **CGE** is allegedly based on constructive mathematics (although the 'computable' in **CGE** may suggest a basis in recursion theory). This schizophrenia is ostensibly resolved by an appeal to what is known as *Uzawa's equivalence theorem ([88]*). Debreu's admirably concise acknowledgement of the importance of Uzawa's equivalence theorem is a testimony to the 'bridging role' it plays, between economic equilibrium existence theorems and fixed-point theorems, *[18]*, p. 719-720:

> "[The equilibrium existence] theorem establishes the existence of a price vector yielding a negative or zero excess demand as a direct consequence of a deep mathematical result, the fixed-point theorem of Kakutani. And one must ask whether the .. proof uses a needlessly powerful tool. This question was answered in the negative by Uzawa (1962) who showed that [the theorem] directly implies Kakutani's fixed-point theorem."

Scarf's insight was, then, to utilize algorithms that had been developed to approximate (Brouwer's) fixed-point theorem – invoking Uzawa's equivalence theorem – to determine approximations to (Walrasian or Arrow-Debreu) equilibria. Scarf himself was well aware that these were not 'approximations' of a useful nature (unless conjoined to those intangible non-formal concepts like intuition, experience and insight):

> "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."
> [59], p.52; italics added

Scarf, however, misses an important point here: it is not 'the passage to the limit' that 'is the nonconstructive aspect of Brouwer's theorem' implying non-assurance of useful approximations; it is, instead the intrinsic undecidable disjunctions that characterize the Bolzano-Weierstrass theorem. In one of only two of the standard textbooks on mathematical general equilibrium theory where the Uzawa equivalence theorem is explicitly discussed ([16], [78]), Starr's clear and detailed presentation of the proof of Brouwer's fix point theorem is based on the excellent and almost elementary exposition in [85] (particularly, pp.424-7). There, in turn, the appeal to the Bolzano-Weierstrass theorem is made almost as with a magician's wand[31]:

> "Making [the] assumption [that given any simplex *S*, there are subdivisions that are arbitrarily fine] we can now finish the proof of Browuer's fixed-point theorem. We take an infinite sequence of subdivisions of *S* with *mesh*, that is, length of the longest one-dimensional edge, approaching **0.** From each subdivision, we *choose* one simplex that carries all labels, and in this simplex we *choose* a single point. We thus have an infinite sequence of points in the original simplex *S,* and *we can choose a subsequence that converges to a single point*. This point .. is the limit point of the sequence of all vertices of all the simplexes from which the points of the convergent subsequence were

[31] In the clear and elementary proof of the Brouwer fix point theorem given in Starr's textbook (op.cit), the appeal to the Bolzano-Weierstrass theorem is made when proving the KKM theorem (p. 62). In Scarf's own elegant text (op.cit) invoking of this theorem occurs, during the proof of Brouwer's theorem, on p. 51:

> "As the vectors are increasingly refined, a convergent subsequence of subsimplices *may be found*, which tend in the limit to a single vector x*." (italics added)

Scarf is careful to claim that the required subsequence '*may be found*', but does not claim that it can be found algorithmically. One may wonder: if not found algorithmically, then how?

originally *chosen*." ([85], p.427; all italics, except the first one, are added)

The deceptive use of the word '*choose*' in the above description of mathematical processes conveys the impression that the '*choices*', in each case, are *algorithmically* implementable. However, it is only the first use of the word '*choose*' and the implied *choice* - i.e., choosing simplexes from increasingly fine subdivisions - that can be algorithmized constructively. The part that invokes the Bolzano-Weierstrass theorem, i.e., '*Choosing a subsequence that converges to a single point*' - incidentally, this point is the sought after fixed-point of the Brouwer theorem – entails undecidable disjunctions and as long as any proof relies on this aspect of the theorem, it will remain unconstructifiable[32].

Why, then do two of the most renowned practitioners of applied general theory, especially in its policy aspects, John Shoven and John Whalley ([62]), make the following explicit claim:

> "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

[32] Over fifty years ago, when Brouwer returned to the topic of his famous theorem with an Intuitionist version of it, he made the trenchant observation that seems to have escaped the attention of mathematical economists:
"[T]he validity of the Bolzano-Weierstrass theorem [in intuitionism] would make the classical and the intuitionist form of fixed-point theorems equivalent." ([13], p.1).
The invalidity of the Bolzano-Weierstrass theorem in any form of constructivism is due to its reliance on the law of the excluded middle in an infinitary context of choices (cf. also [23], pp. 10-12).

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

Those who claim that they work with 'computable' general equilibrium models – the self-proclaimed followers of Leif Johansen, mentioned in the opening section, for example, and a host of applied general equilibrium, policy-motivated, theorists and applied economists – continue to anchor their work on an appeal to formal Arrow-Debreu equilibrium theory or its CGE variant. For example such a claim is most explicitly made in Part II of Kermal, et.al (1982). The exact claim is that the equilibria they – and others – *compute*, using *their versions* of general equilibrium models, can be linked to, and theoretically substantiated by, the Arrow-Debreu equilibrium of pure theory. Thus, [21] p.153 (italics added):

"[I]t is reasonable to ask if, in fact, a solution exists [for the CGE model] and, if so, whether or not it is unique. Most applied model builders, in contrast to theorists, have not worried too much about general existence problems. After all, a solution is *numerically computed* and an existence proof may appear unnecessary. The models are always quite well behaved and, given that very general existence proofs have been established for theoretical models of which CGE models form a rather *well-behaved subset*, it is reasonable to expect that nonexistence problems will not arise in practice. "

This is complete nonsense.

The Arrow-Debreu equilibrium is provably uncomputable, both from the point of view of the mathematics of constructivism and recursion theory. The equilibria computed by any and every computable general equilibrium model used for development policy exercises – or those that are linked to, and derived from, variations of the Johansen model – have nothing whatsoever to do with the theoretical equilibria of general equilibrium theory.

The technical results of these untenabilities, infeasibilities and infelicities are rigorously demonstrated in [90] and [91].

Computable General Equilibrium theory has no grounding in computability or constructivity. Claims by applied general equilibrium theories *of any variety* that their work is anchored in any form of CGE is vacuous from a theoretical computational point of view. At best exercises by applied general equilibrium theorists can be considered ad hoc numerical exercises, seeking consistency and balance in accounts. Nothing more – especially nothing in theoretical anchors of any sort – is warranted. As long as a methodology that theorises in a kind of mathematics that is devoid of numerical meaning and computationally vacuous and relies on a schizophrenic appeal to a mathematics that is grounded in computational feasibilities, any claim of computability, constructivity or numerical feasibility must remain dubious, at best.

5. Computable Economics

> "[W]e want to stress that solutions that are not *effectively computable* are not properly solutions at all."
> [6], p.17; italics added.

In computable economics, as in any computation with analogue computing machines or in classical behavioural economics, all solutions are based on *effectively computable* methods. Thus computation is intrinsic to the subject and all formally defined entities in computable economics – as in classical behavioural economics – are, therefore, algorithmically grounded.

5.1 Briefly .....

Given the algorithmic foundations of computability theory and the intrinsic dynamic form and content of algorithms, it is clear that this will be a 'mathematics with dynamic and algorithmic overtones'[33]. This means, thus, that computable economics is a case of a new kind of mathematics in old economic bottles. The 'new kind of mathematics' implies new questions, new frameworks, new proof techniques - all of them with algorithmic and dynamic content for digital domains and ranges.

Some of the key formal concepts of computable economics are, therefore: *solvability & Diophantine decision problems*, *decidability* & *undecidability*, *computability & uncomputability*, *satisfiability*, *completeness* & *incompleteness*, *recursivity* and *recursive enumerability*, *degrees of solvability* (*Turing degrees*), *universality* & the *Universal Turing Machine* and *Computational*, *algorithmic* and

[33]"I think it is fair to say that for the main existence problems in the theory of economic equilibrium, one can now bypass the fixed point approach and attack the equations directly

stochastic complexity. The proof techniques of computable economics, as a result of the new formalisms, will be, typically, invoking methods of: *Diagonalization*, *The Halting Problem for Turing Machines*, *Rice's Theorem*, *Incompressibility theorems*, *Specker's Theorem*, *Recursion Theorems*. For example, the *recursion theorems* will replace the use of traditional, non-constructive and uncomputable, topological fix point theorems, routinely used in orthodox mathematical analysis. The other theorems have no counterpart in non-algorithmic mathematics.

In the spirit of pouring new mathematical wines into old economic bottles, the kind of economic problems of a digital economy that computable economics is immediately able to grant a new lease of life are the classic ones of: computable and constructive existence and learning of rational expectations equilibria, computable learning and complexity of learning, computable and bounded rationality, computability, constructivity and complexity of general equilibrium models, undecidability, self-reproduction and self-reconstruction of models of economic dynamics (growth & cycles), uncomputability and incompleteness in (finite and infinite) game theory and of Nash Equilibria, decidability (playability) of arithmetical games, the intractability (computational complexity) of optimization operators; etc.

5.2 *Formally ....*

Suppose the starting point of the computable economist whose visions of

to give existence of solutions, with a simpler kind of mathematics and even mathematics with dynamic and algorithmic overtones."[75], p.290; italics added.

actual economic data, and its generation, are the following:

Conjecture 14 *Observable variables are sequences that are generated from recursively enumerable but not recursive sets, if rational agents underpin their generation.*

An aside: In 1974 Georg Kreisel posed the following problem:

> "We consider theories, ... and ask if every sequence of natural numbers or every real number which is well defined (*observable*) *according to the theory* must be recursive or, more generally, *recursive in the data*. .... Equivalently, we may ask whether any such sequence of numbers, etc., can also be generated by an ideal computing or Turing Machine if the data are used as input. The question is certainly not empty because most objects considered in a ... theory are not computers in the sense defined by Turing. ...."
> [37], p.11

The above conjecture has been formulated after years of pondering on Kreisel's typically thought-provoking question. More recently, a reading of Osborne's stimulating book ([49]), was also a source of inspiration in the formulation of the conjecture as an empirical disciplining criterion for computable economics.

The conjecture is also is akin to the orthodox economic theorist and her handmaiden, the econometrician, assuming that all observable data emanate from a structured probability space and the problem of inference is simply to determine, by statistical or other means the parameters that characterise their probability distributions. If, therefore, the computable economist's starting point is the above conjecture then it follows that:

Theorem 15 *Only dynamical systems capable of computation universality can generate sequences that are members of sets that are recursively enumerable but not recursive.*

Theorem 16 *Only dynamical systems capable of universal computation can extract patterns inherent in arbitrary, digitally generated, data, without assuming their generation by an underlying probability model.*

Corollary 17 *Asymptotically stable dynamical systems are not capable of computation universality.*

Proposition 18 *Only dynamical systems capable of computation universality are consistent with the **no arbitrage** hypothesis.*

Theorem 19 *Rational economic agents in the sense of economic theory are equivalent to suitably indexed Turing Machines; i.e, decision processes implemented by rational economic agents - viz., choice behaviour - is equivalent to the computing behaviour of a suitable indexed Turing Machine.*

Put another way, this theorem states that the process of rational choice by an economic agent is equivalent to the computing activity of a suitably programmed Turing Machine. This is exactly parallel to the formalisation with which choice in classical behavioural economics is implemented.

Conjecture 20 *Dynamical systems capable of computation universality can persist in disequilibrium configurations for long time periods.*

Theorem 21 (**Rabin, 1957**) *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 should play in order to win.*

The next item has been mentioned twice already in this essay; but I restate it here just for completion.

Theorem 22 Undecidability of Hilbert's tenth problem

There is no algorithm which, for a given arbitrary Diophantine equation, would tell whether the equation has a solution or not.

Theorem 23 Halting Problem for Turing Machines

Suppose we are given a Turing Machine computable function $f_n(m)$. *Then there is no general algorithm for determining, for arbitrary* $n \geq 0$ *and* $m \geq 0$, *whether* $f_n(m)$ *is defined.*

Theorem 24 Rice's Theorem*: Let C be a class of partial recursive functions. Then C is not recursive unless it is the empty set, or the set of all partial recursive functions.*

Claim 25 *Validity of the Church-Turing Thesis on Effective Calculability*

Theorem 26 ***Specker's Theorem*** *in Computable Analysis ([77], pp. 145-58)*

A sequence exists with an upper bound but without a least upper bound.

Theorem 27 *The Pour-El/Richards Theorem*

There exists an Ordinary Differential Equation (ODE) s.t: $\varphi'(t) = F[t, \varphi(t)]$ *with* $\varphi(0) = 0$*, s.t: F(x,y) is computable on the rectangle* [0 ≤ x ≤ 1, -1 ≤ y ≤ 1] *, but no solution of the ODE is computable on any interval* $[0, \delta], \delta \geq 0$

Theorem 28 Fix Point Theorem

Suppose that $\Phi : F_m \rightarrow F_n$ *is a recursive operator (or a recursive program P). Then there is a partial function* f_ϕ *that is the least fixed point of* Φ

Theorem 29 $\Phi(f_\phi) = f_\phi$;

If $\Phi(g) = g$, then $f_\phi \subseteq g$

Remark 30 *If, in addition to being partial,* f_ϕ *is also total, then it is the* ***unique least fixed point.***

Finally, related to invariance theorems in the domain of algorithmic complexity theory and the fix point theorem of classical recursion theory, we have the *recursion theorem*, essential for understanding self-reproduction and self-reconstruction (for computable growth theory):

Theorem 31 ***Recursion Theorem*** *Let T be a Turing Machine that computes a function:*

$$t : \Sigma^* \times \Sigma^* \rightarrow \Sigma^* \qquad (5)$$

Then, there is a Turing Machine R *that computes a function*:

$$r : \Sigma^* \rightarrow \Sigma^* \qquad (6)$$

such that, $\forall \omega$:

$$r(\omega) = t(< R >, \omega) \qquad (7)$$

where, <R>: denotes the encoding of the Turing Machine into its standard representation as a bit string; and the *(star) operator denotes its standard role as a *unary operator* defined as: $A^* = \{x_1, x_2, ... x_K \mid k \geq 0, \forall x_i \in A\}$:

The idea behind the *recursion theorem* is to formalize the activity of a Turing Machine that can obtain its own description and, then, compute with it. All malicious 'hackers', perhaps with no knowledge of this theorem, are invoking this theorem every time they generate viruses! More seriously, this theorem is essential, too, for formalizing, recursion theoretically, a model of *growth* in a digital economy and to determine and learn, computably and constructively, *rational expectations equilibria*. The *fix point theorem* and the *recursion theorem* are also indispensable in the computable formalization of *policy ineffectiveness* postulates, *time inconsistency* and *credibility* in the theory of macroeconomic policy. Even more than in microeconomics, where topological fix point theorems have been indispensable in the formalizations underpinning existence proofs, the role of the above *fix point theorem* and the related *recursion theorem* are absolutely fundamental in what I come to call *Computable Macroeconomics*.

Anyone who is able to formalize these theorems, corollaries and conjectures and work with them – and accept the claim – as those that are to discipline economic theoretical criteria, would have mastered all the necessary mathematics of computable economics. Unlike so-called computable general equilibrium theory and its offshoots, computable economics – and *its* offshoots – are intrinsically computational and numerical.

6. Agent Based Computational Economics

> "It is suggested that a system of chemical substances, called morphogens, reacting together and diffusing through a tissue, is adequate to account for the main phenomena of morphogenesis. ...
> Most of an organism, most of the time, is developing from one pattern into another, rather than from homogeneity into a pattern. One would like to be able to follow this more general process mathematically also. The difficulties are, however, such that one cannot hope to have any very embracing *theory* of such processes beyond the statement of the equations. It might be possible, however, to treat a few particular cases in detail with the aid of a digital computer. ..... The essential disadvantage of the method is that one only gets results for particular cases. ... The morphogen theory of phyllotaxis, to be described, ..., in a later paper, will be covered by this computational method. Non-linear equations will be used."
> [86], pp. 37, 71-2; italics in the original.

The origins of what has become agent based computational methods can be traced to the pioneering works of Turing on *Morphogenesis* [86], von Neumann on *The Theory of Self-Reproducing Automata* ([94]), and Ulam on *Nonlinear dynamics* ([28], [79]). A 'second generation' of pioneers were Conway ([7]) and Wolfram [96]), 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 ([29]), 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 ([64]).

Sadly, none of these classics have had the slightest impact on the current frontiers of agent based computational economics (see, for example, [83]). 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*

([83], p. 1626; italics added), Axel Leijonhufvud asserts that[34]:

> "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

[34] When one of us first read this extraordinary statement, his mind went back to the witticism with which Dennis Robertson reacted when he supposedly first heard of revealed preference., [56], p.19:

> "Dare I confess that when I first heard this term ... I thought that perhaps to some latter-day saint, in some new Patmos off the coast of Massachusetts, the final solution of all these mysteries had been revealed in a new apocalypse?"

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. [95]) – 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 [19]).

No better characterisation of the practice of agent based computational economists can be given that the one Arthur Burks gave (cf. [14], 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 algorithmically 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.

And so on!

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.

Agent based computational economics is vacuous from an epistemological point of view, when viewed either from the point of view of computation theory or from a dynamical systems point of view, contrary to many and varied claims to the contrary. We locate the vacuity on the lack of anchoring in the noble traditions broached by the pioneers. That the Fermi-Pasta-Ulam problem remains impervious to analysis, computational experiments or dynamical system explorations should be a lesson for those economists who think they have found a panacea to all modelling ills. Above all, it is strange that the overwhelming majority – if not, in fact, all – of agent based computational economists are not aware of the disciplining criteria with which the pioneers embarked on computational explorations in cellular space. This is why agent based computational economics is essentially an exploration of cellular spaces with finite automata that do not have the power of Turing Machines – i.e., the transition functions that are routinely used for cellular space exploration by agent based computable economists are not partial recursive functions, if, indeed, many, or any, of them are even aware of such finessed distinctions between classes of functions; there is certainly no evidence of any such awareness in any of the contributions in [4], [81] or in [19].

7. Towards an Epistemology of Computation in Economics

> "Do we overpass ... the Turing-Church 'barrier' and compute the uncomputable? Not exactly. We just move the discussion in another territory that of *processes that handle information*. This syntagma is so general that in these terms 'everything is a computation'; it is a matter of point of view ('for every process there is an observer which can interpret the process as a computation')"
> [50], p. 345

'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 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[35]. *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."
>
> [31], pp.154-5, italics added.

We have discussed and described alternative visions of computation in economics. What, then, if the economy is itself a computer? Do economic processes, whether aggregative or not, embody the results of a computation?

[35]A reader, equipped with the standard knowledge of classical recursion theory, would immediately invoke the distinction between *recursive* and *recursively enumerable* sets to make

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 abut the computability properties of the economy will remain incomplete. On the other hand, if we – as observers – feed the economy with data sets that are also recursively enumerable but not recursive, then whether the economy, as a computer, will be able to process it in a definitive way will remain unknown for an indeterminate period.

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.

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precise sense of this important observation.

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 is itself a computer. This is the natural mode of interaction between the economy and the classical behavioural economist and the computable economist; it is not the natural mode for the CGE theorist, nor for the agent based computational economist. This is why there is a serious epistemological deficit in the practice of the latter two classes of economists.

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