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THE EPISTEMOLOGY OF SIMULATION, COMPUTATION AND DYNAMICS IN ECONOMICS -- ENNOBLING SYNERGIES♣, ENFEEBLING 'PERFECTION'

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♣ "[T]his interplay between analysis and computers, to further our understanding of nature, has now become general practice. The importance of this interplay, which Ulam (1960) emphasized, and referred to as `synergetics', has been adopted to and adapted several research philosophies (Zabusky, 1981, Haken, 1983). However, the importance of the computer, and the lessons we are learning from it, may even exceed the importance anticipated by `synergeticists'." (Jackson, 1990,, pp. 264-5, italics added).

♠ This author is eternally indebted to his first teacher in economics, Professor Björn Thalberg, whose own research (Thalberg, 19660), and lectures on macroeconomics, almost forty years ago, were the catalysts that encouraged him on a path of research that has always emphasized the power, necessity and versatility of simulational studies of nonlinear dynamical systems in economics.

Abstract[1]

Lehtinen and Kuorikoski ([73]) question, provocatively, whether, in the context of 'Computing the Perfect Model', economists avoid - even positively *abhor* - reliance on 'simulation'. We disagree with the mildly qualified affirmative answer given by them, whilst agreeing with some of the issues they raise. However there are many economic theoretic, mathematical (primarily recursion theoretic and constructive) - and even some philosophical and epistemological - infelicities in their descriptions, definitions and analysis. These are pointed out, and corrected; for, if not, the issues they raise may be submerged and subverted by emphasis just on the unfortunate, but essential, errors and misrepresentations.

Keywords: Simulation, Computation, Computable, Analysis, Dynamics, Proof, Algorithm

[1] We are eternally in debt to the spirit, the practice, the philosophy and methodology of our late and beloved teacher and friend, Richard Goodwin, who underlined the importance of computation and simulation in nonlinear, interdependent, multisectoral economic analysis in almost all his professional writings and teaching (in particular – but not only – in, [45], [46], [48], [49], [50]). More immediately, however, we are deeply grateful to our friend Joe McCauley and our students, N. Dharmaraj, Kao Selda and V. Ragupathy, who have, each in his and her own way, helped us strengthen our belief in the 'synergy'. Joe McCauley's work in nonlinear dynamics and financial market dynamics – in the latter, he was following the tradition of Maury Osborne ([98]) – have been important inspiring sources for our work and belief in the 'synergy', in particular the significance he has increasingly come to attach to Poincaré's '*recurrence theorem*' ([81], chapter 3 and [82]). This theorem, implicitly and explicitly, lies at the heart of Fermi's inspiration in formulating what came to be known, justly and famously, as the *Fermi-Pasta-Ulam Paradox* ([37] and [36]) and '*The Genesis of Simulation in Dynamics*' ([165]). This 'paradox' *should* be our paradigmatic 'case study', to substantiate a belief in the validity of an extended synergetics (between *simulation*, *computation* and *analysis*). However, space limitations are a constraint for this good intention.

"Tobin exclaimed at Nozick: 'There's nothing more dangerous than a philosopher who's learned a little bit of economics.' To which Nozick immediately responded: 'Unless it's an economist who hasn't learned any philosophy'."

Hutchinson, [64], p. 187

"In what other way, if not ***simulation*** *by a Turing machine, can we understand the process of making free choices? By making them, perhaps."*

Nozick, [97], p.303; bold italics added.

1 A Preamble on Simulational Serendipities

> "Mature as he was, she might yet be able to help him to the building of the *rainbow bridge*[2] that *should connect the prose in us with the passion*. Without it we are meaningless fragments, half monks, half beasts, unconnected arches that have never joined into a man."
>
> E.M.Forster: Howard's End, Chapter XXII (italics added)

The May-June, 2009, issue of the **American Scientist** (Vol. 97, No. 3) contained, *serendipitously*, four – possibly five[3] – articles on the fundamental role played by *simulations* - in its synergetic interactions with *theoretical analysis*, *experiment*, *computation*, *prediction* and *dynamics* – in macroeconomics ([60]), physics ([105]), engineering ([101][4]) and a 'revisit' to the *Limits to Growth* report ([55]). In particular, the repository of simulation in the example of macroeconomics by Brian Hayes, is an exemplary exposition of the *Phillips Machine*, devised and constructed as an electro-mechanical-hydraulic *analogue computing machine*, encapsulating early Keynesian Monetary Macrodynamics, and capable of interacting with macroeconomic theory and even settling controversial theoretical debates decisively. The workings of the machine, entirely transparent, were such that almost any nonlinear dynamical system, then current in macroeconomic theory, could have been exactly[5] simulated, without any recourse to

[2] The phrase used by Dr Allan McRobie to refer to the *Millennium Bridge* – also referred to as the *Blade of Light* (cf. [134]) – during a personal conversation with the first author, in Cambridge, on 17th May, 2010. Dr Allan McRobie was instrumental is detecting the source of the dramatic failure of the Millennium Bridge via an inspiring simulation study using a home-made analogue device in his University laboratories at Cambridge. Velupillai had gone to Cambridge to view a demonstration of the resurrected *Phillips Machine*, an analogue computing machine devised by the famous economist A.W.H. Phillips to model, simulate and study Keynesian Business Cycle *Theories* in policy contexts. The Phillips Machine, also known as the *MONIAC* – clearly a play on the name given to the first, large-scale, digital computing device, MANIAC – in Cambridge was resurrected single-handedly by Dr McRobie. Velupillai had been educated with it as a computing tool for simulating *Keynesian Nonlinear Multiplier-Accelerator models* by its first - and only – economist custodian, Richard Goodwin, whose theoretical work informed decisively the construction of the analogue computing machine by Phillips ([102]).

[3] This is because the article on *The Origin of Life* in this issue of the **American Scientist** ([145]) traces the emergence of 'experimental research in origin-of-life studies' in the *analogue device* with which Harold Urey and Stanley Miller studied – via *simulations* of hypothetical conditions satisfying the Oparin-Haldane hypothesis of chemical evolution – the 'chemical processes that might have occurred on the planet soon after its birth.' The ultimate methodological message of Trefil, Morowitz and Smith – the authors of *The Origin of Life* – appears to be an intensive experimental research program tied to the development of the appropriate theory, itself guided by experimental results. The experimental program, of studying 'complex cooperative networks', is, surely, through simulational studies of the fruitful interaction of analogue experimental setups and digital computing methods.

[4] Most interestingly, Dr Petroski's visit to Japan, which resulted in his fascinating article on the *Akashi Kaikyo Bridge*, was sponsored by the **Association for the Study of Failure** (*Shippai Gakkai*), as its Invited Speaker at their International Conference in November, 2008. Needless to say, as economists we are only painfully aware - simulations or not - of the need for such a society in economics!

[5] Subject, of course, to engineering precision constrains in the manufacture of the electrical,

approximations or discretizations, normally required in a digital computer - unless, of course, continuous data was available (which was not) to exploit its – the Phillips Machine's – full analogue potential. Moreover, the machine was capable of displaying, in all its *transparent* detail, the propagation mechanisms of policy and 'shocks', whether monetary or 'real', confirming and disconfirming, as the case may be, orthodox and non-orthodox propositions on policy and even inculcating a sense of humility in the then emerging consensus on the feasibility of what came to be known as *fine-tuning* (see [4], in particular, §5, p. 108, ff). The Machine was also capable of generating *surprises*, a *sine qua non* of an experimental device or design, in its interaction with theories that underpin and interact with it. At the request of Nicholas Stern at the LSE, when attempts were being made to resurrect one of their two Phillips Machines, Richard Goodwin wrote a memoir[6] on his own experiences in working and teaching with it. In a PS to the covering letter (dated 16 August 1991) he sent Stern, together with the memoir, Goodwin noted as follows[7]:

> "I was very pleased that Phillips had 2 machines in London and I was able to show him (*which he had doubted*) that we could produce aperiodic, 'chaotic', motion with the two interconnected" (italics added)

In other words, every desideratum specified, implicitly, explicitly and vaguely by Lehtinen and Kuorikoski (op.cit; henceforth referred to as L&K), from an epistemic, epistemological and methodological point of view as economists and economics, is handsomely satisfied by a Phillips Machine simulation, if structured and implemented in any traditional experimental sense (whether in the form suggested by L&K or even in more senses than that, as shown in the famous Fermi-Pasta-Ulam exercise, to which we will turn now, briefly).

In their fascinating recapitulation of the circumstances under which Enrico Fermi, John Pasta and Stanislaw Ulam tried to resolve a theoretical conundrum – and still completely unresolved – with a discrete approximation of continuum model implemented on one of the first available digital computers – the MANIAC – Porter, et.al ([105]) point out the many ways in which the simulations interacted with the analytical theory to enrich both in surprising ways. The epistemic, epistemological and methodological implications of the series of simulations that have been implemented, with increasing precision, detail and

mechanical and hydraulic components. The constraints of natural laws, in the processing of data, for example, are common to any physical mechanism - whether analogue or digital.

[6] A truncated version of which appears in [72], chapter 13, pp. 118-9. Copies of the full memoir and the covering letter to Nicholas Stern were sent by Goodwin to the first author, who will be happy make them available to interested readers.

[7] In the unabridged memoir, this 'PS' appears as (italics added):

> "Furthermore, I was very excited to find Phillips had two of his magical machines in London, so I could reproduce what I had analyzed back in 1947 in my dynamical coupling paper. If I remember correctly, *Phillips did not believe we could produce erratic behaviour by coupling his machines – but we did.*"

generalizations in the fifty-five years since the original ‘experiment’,are exhaustively discussed and dissected in the admirable monograph by Thomas Weissert[8] ([165]). As they perceptively and clearly[9] note (ibid, pp. 214-6; italics added):

> "....Fermi had long been fascinated by a fundamental mystery of statistical mechanics that physicists call the ‘arrow of time’ [irreversibility]. .... Fermi believed that the key [to the unlocking of the mystery of irreversibility] was nonlinearity .... He knew that it would be far too complicated to find solution to nonlinear equations using pencil and paper. Fortunately, because he was at Los Alamos in the early 1950s, he had access to one of the earliest digital computers [the MANIAC]. .... The FPU problem was one of the first open scientific investigations carried out with the MANIAC, and it ushered in the age of what is sometimes called experimental mathematics. .....[by] which we mean computer-based investigations designed to give insight into complex mathematical and physical problems that are inaccessible, *at least initially*, using more traditional forms of analysis. .... With Pasta and Ulam, Fermi proposed to investigate what he assumed would be a very simple nonlinear dynamical system. .. The Key question FPU wanted to study was how long it would take the oscillations of the masses and nonlinear springs to come to equilibrium. ... *They were absolutely astonished by the results.*"

The FPU problem exemplifies every aspect of epistemic, epistemological and methodological issue that can be conceived – not all of which, though confronted, have been adequately resolved even after fifty five years of deeply serious theoretical and empirical attempts. We mention again the FPU problem, briefly, below, from an epistemic and epistemological point of view, especially in conjunction with a computational dynamic macroeconomic problem one of us (see [173]) has attempted to resolve by structured simulation studies, in close combination with established macrodynamic and interindustrial economics.

Petroski’s brief but illuminating description and general discussion of the analogue – ‘the 40-meter long replica’ – model that was used in the ‘wind-tunnel tests’ emphasizes those elements that were neglected in the construction of the *Millennium Bridge*. As emphasized by Dr Allan McRobie, during his conversation with Velupillai (see first footnote in this section), the construction

[8] Although even this admirable monograph is now – thirteen years after publication – clearly out of date, given the massive research and results on variations of the Fermi-Pasta-Ulam (henceforth referred to as FPU) problem that have been, and are being, conducted at the frontiers of what has come to be called ‘experimental nonlinear dynamics’.

[9] One of the authors of this crystal clear exposition of the FPU problem, Norman Zabusky, was himself a pioneer in extracting new theoretical directions of research – and, indeed, together with his co-author, Martin Kruskal, to whose memory this particular article is dedicated in (re-)discovering and giving a mathematical formalism to ‘solitary waves’, now called *solitons*. By retaining the original continuum domain of the FPU theoretical framework, and eschewing the discretizations necessary for digital computer implementation, they were able to predict the existence of solitons (see, in particular, [169]).

of a bridge is *less* about physics and engineering than about people because when a bridge is in use, especially a pedestrian dominated suspension bridge, it becomes a 'nonlinear *biological* system'. This implies an analogue computation model for simulation that is *a coupled system of the interaction between engineering structures and human beings*. Failing to take this into account in the analogue computing simulation of the Millennium Bridge construction at its design and testing stages led to the bridge having to be closed within 20 minutes of the long-awaited opening, due to the fearful wobbling when pedestrians began their presence felt. In other words, the analogue simulation – buttressed, of course, by various uses of the digital computer – failed to study the design problem as one that should have been studies as a nonlinear, coupled, oscillator - just as the FPU problem was, and just as it still remains a mystery, so will bridge building be, in the sense that there is, at present, no complete characterization of the dynamics of nonlinear, coupled, oscillators. Every epistemic, epistemological and methodological conundrum faced, many solved, by the FPU problem has to be faced in the construction of every bridge, especially if it is a suspension bridge. Naturally, every model of an economy to be studied by computer – whether digital or analogue – simulations, and underpinned by economic theory is naturally and intrinsically coupled, but nonlinearly. It is this latter fact that is often neglected in much of the recent simulation-dominated literature, to which we will return in the sequel.

The fourth of the serendipitous articles is by Hall & Day ([55]), reviving and reminding us, in this age of increasing environmental concerns, the simple, but powerful, message of Malthus. The much maligned dichotomy between an entity growing exponentially while relying for its growth on something else growing arithmetically was made (in)famous, particularly in economics and public policy, by the well-meaning Malthus, to be revived, in one form or another, particularly in economics, whenever even a shadow of an exhaustible resource was seen in the horizon. Perhaps the most spectacularly dramatic example of a neo-Malthusian apocalyptic scenario for economic societies, smug in their reliance on the manna of exogenous, technological, factors to propel them through the golden era – and beyond – of Keynesian prosperity, was the ill-timed release of the **Club of Rome** document on *Limits to Growth* ([83]). It was ill-timed in both positive and negative senses: the first oil price hikes, the great stagflation of the 1970s, the collapse of the Bretton Woods compromises, the demise of the Neoclassical Synthesis, the rise of varieties of Monetarism and, eventually, the emergence of endogenous growth theory and the Miracle Economies of East Asia, together with the all-embracing dominance attained by Newclassical economics, discrediting any attempt at active policy in any domain, were all in the horizon. With hindsight it may arguably be remarked that almost nothing discredited – although Hall & Day (*ibid*) show, convincingly, that the reasoning and analysis underpinning the original *Limits to Growth* manifesto have stood the test of time most admirably – the intellectual credibility of *simulation-based analysis* and projections than this one single work, directed by the founding Father of *System Dynamics*, Jay Forrester[10]. It is little remembered or recounted that Forrester's initial fame owed as much to his use of what has come to be called '*hand-held simulations*' to resolve an internal conundrum of employment stability, independent of the economy-wide business cycle, at General Electric, as to his insight into the need for understanding corporate dynamism in terms of the interaction between *engineering and management synergies.* In our opinion, however, the main reason for the discrediting could be found in the philosophy underpinning the *Limits to Growth* methodology, which was unanchored in theory. It was, to the economist at least, a case of not even 'measurement without theory', for a generation of economists who were to extol the virtues of 'theory ahead of measurement'. Moreover, the epistemological justification of the *Limits to Growth* policy prescription would have to be made on inferring general propositions from *induction*, forgetting *Hume's dictum* and *Popper's strictures* against this noble practice by distinguished empiricists and scientists, all the way from Newton to Darwin. In the spirit of the disciplining criterion for simulation modelling of complex, intractable, dynamical systems mentioned above, we ourselves locate the weakness of the case made in the *Limits to Growth* literature - then and now, thirty years later ([84] – in its eschewing a *nonlinear, coupled, dynamics* framework in modelling the interaction between a natural system and its dependent human, economic, 'sink'. The dynamics of such coupled, nonlinear, dynamical systems cannot be breached – even provably so – by known analytical approaches and require, as one learns from the fifty-five year unresolved saga of the FPU problem, the helping hand of computer simulations to get a handle on plausible dynamical evolutions and possible policy responses, even if only in limited senses. Of course, there was also the problem of the lack of theoretical underpinnings, above all in economic theory.

If anything is to be learned from the four examples, serendipitously brought together in just celebrations of their various anniversaries, it is that the *synergies* between intractable coupled nonlinear dynamics, underlying theoretical conundrums and exploitation of the ubiquity of the emerging power of new and innovative paradigms of computations, could – at best – be exploited for advancing the respective disciplines only with an attitude of *modesty* in epistemological aims, *humility* in the face of methodological – in the limited sense of *methods of mathematics* – limitations and a *generosity* of spirit in the light of philosophical confusions. Ultra reliance and untrammeled confidence in the

[10] This is the general view, even of those who were – and remain – sympathetic to the message, if not the full paraphernalia of methods, of The Club of Rome report ([43], p.6; italics added):

> "The Club of Rome simulations which predicted global environmental catastrophe made a major impact, but also *gave simulation an undeservedly poor reputation* as it became clear that the results depended very heavily on the specific quantitative assumptions made about the model's parameters. Many of these assumptions were backed by rather little evidence."

With hindsight, too, it was regrettable that The Club of Rome team were not familiar with the art, science, methodology and epistemology of simulation that was being learned as the attempt to solve the FPU problem was being 'played out'.

power of one kind of mathematical analysis, if coupled (sic!) to unreflective confidence in the power of the emerging paradigms of computation to solve the unsolvable, has caused much mischief in the sciences – both natural and social. Shunning simulation – an attitude not confined to the economists – is akin to the precept warned against by that old adage, not to throw away the baby with the bathwater.

The rest of this paper is organized as follows. In the next section we outline, as succinctly as possible, L&K's claims about 'the economist's perfect model'.. The list of infelicities in their claims is embarrassingly and surprisingly long. Some of the misrepresentations and infelicities are corrected as we go along, in section 2 itself; others require detailed dissections and remedies, some of which are attempted in section 3, which is devoted to their efforts at 'reinventing the square wheel', i.e., redefining economics!. Section 4 is on *Computation, Discretization, Proof and Other Mathematical Infelicities*. In section 5 we attempt to summarize, as concisely as possible, the core areas of economics which initiated and maintained what we call 'the noble tradition of simulation in economics'.The final section tries to draw the threads together to outline a bright vision for economics, yet remembering the melancholy failures of past claims.

2 The Economist's Perfect Model

> "*Perfection*, of a kind, was what he was after,
>
> And the poetry he invented was easy to understand;
>
> ......
>
> When he laughed, respectable senators burst with laughter,
>
> And when he cried the little children died in the streets."
>
> Epitaph On A Tyrant by W.H. Auden (italics added)

In their fundamental, pioneering, simulation-based, development of evolutionary growth theory, Richard Nelson and Sidney Winter carefully – but unambiguously – point out that:

> "It is, in short, a very pernicious doctrine that portrays *simulation as a nontheoretical activity*, in which the only guiding rule is to 'copy' reality as closely as possible. If reality could be 'copied' into a computer program, that approach might be productive – but it cannot, and it is not."[11]
>
> [93], p.209; italics added

[11] L&K take on board the criticisms in [138] to finesse their 'definition' of the perfect model (see especially p. 314 in L&K). Teller's possibly caricatured starting point of a definition of a mythical *model* of a *perfect model* is worth recalling:

> "The photograph provides a good icon: The ambition has been to produce a perfect likeness of nature, *a perfect model*. ...... Of course characterizing our efforts to describe nature as aimed at producing *a perfect model* is itself a model of the human knowledge-gathering enterprise. Hence we may call it *the Perfect*

It is regrettable that the L&K vision of simulation systematically 'portrays [it] as a nontheoretical activity'. We think this vision is an inevitable result of their faulty norm of *The Economist's Perfect Model*, their less than desirable mastery of economics – in its theoretical, empirical and methodological senses – and, even more decisively, due to their highly slippery grounding in the theory of computation, foundations of mathematics and the theory of numerical analysis.

Mercifully, however, L&K eschew any such exclusive 'guiding rule'. Instead they opt, in their (admittedly loose) definition of 'the economist's perfect model', to be guided by Hausman's claim that 'economics is an *inexact* science', by which – at least according to L&K – he is supposed to have 'meant that, unlike the natural sciences[12], it has the capacity to characterize economic relationships only *inexactly* because the idealization[13]and abstractions necessary to produce generalizations are not fully eliminable' (p.314; italics added). Against this particular backdrop to a meaning for 'inexactness', L&K go on to outline – we are reluctant to use the words 'characterize' or 'define' - what *they* intend to mean by 'the economist's perfect model', which has to be 'analytically tractable' – even at the cost of 'sacrificing numerical accuracy' – and contains 'idealizations and abstractions', and they go on (p.314; italics added):

> "[B]ut [the economist's perfect model] is exact in the sense that it is formulated in terms of *an exact formal language*. The perfect model should capture the important relationships as logical connections between a few privileged economic concepts[14]. Thus the Haus-

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> *Model* **Model**."
>
> [138], p. 393; italics & emphasis added

[12]Are 'point masses', frictionless pendula and the like not 'idealizations'? What are the underpinnings of the laws that characterise 'all of classical physics' (cf. [34], §18.3 and Table 18-1)? Gravitation, the four Maxwell equations, and the three laws of motion ('Newton's law, with Einstein's modification'), force and conservation of charge, characterize 'all of classical physics' (ibid). Which of these laws, if any, are derived without 'idealizations and abstractions'. Having reached 'the top of K-2 – we are nearly ready for Mount Everest, which is quantum mechanics' (ibid, p. 18-5). Do the 'idealizations and abstractions' diminish as we grope our way forward to an understanding, say, of Schrödinger's equations – or even to be able to manipulate the hitherto non-axiomatizable 'Feynman diagrams'? In fairness to L&K these objections should be directed at Hausman, but not quite; it is L&K who choose, explicitly, to be guided by their own interpretations of Hausman in the senses noted above.

[13]David Ruelle, who has imaginatively combined deep mathematical theory, computer-aided proof methods and numerical simulations in his pioneering work on nonlinear dynamics observed, with characteristic directness ([112], 121; italics in the original):

> "*[M]athematical physics deals with idealized systems.* We know that a water molecule is composed of oxygen and hydrogen nuclei surrounded by electrons and that the nuclei also have a composite structure. There is good reason to believe that these complications are not essential to understanding [*phase transitions*]. A reasonable approach (in fact, the only feasible approach) is to study a variety of idealized systems."

[14]By this we presume L&K – correctly, in our opinion – mean concepts like rationality and equilibrium. But there are, of course, many different ways to define these concepts, even in an 'exact formal language', especially because there are many such 'exact formal languages'.

> manian inexactness of economics leads to a requirement for formal exactness in the models."

However, then they go on to 'spoil' this by claiming[15] that (ibid; second set of italics added):

> "Simulation models are, at best, merely *approximations of* such models. It is also instructive to realize that, even though simulation results are expressed in an exact numerical form, in economics they cannot be perfected. This is .... because, unlike *some natural sciences*, economics does not have any natural constants to discover in the first place. "

We can list several objections to this claim. First of all, as pointed out in the previous footnote, there are perfectly valid and accepted simulation-theoretic economic models – the example we have given so far is the Nelson-Winter evolutionary growth model, but more will be 'offered' in the next section – that are not approximations to any 'such model', allegedly formulated in terms of 'an exact formal language'. Secondly, it is entirely feasible – and achieved with increasing frequency, even in the form of publications in the five journals commonly considered the most prestigious' (L&K, p. 305)[16] – to formulate economic theory in the exact formal language underpinning recursion theory or any form of constructive mathematics such that the 'simulation model' is naturally exact and not an approximation to anything – perfect or not. Thirdly, even in those natural sciences that are allegedly in possession of 'natural constants to discover', it is not clear how 'naturally constant' they are, when even the constancy of the gravitational constant is subject to some qualifications[17]. Fourthly, nowhere in

The formal language corresponding to, say, set theory, is quite different from that which encapsulates proof theory or model theory.

[15] Contrary to the spirit, letter and practice of the noble work that went into the founding and flourishing practice of a *rigorous*, *simulation-theoretic*, Schumpeterian Evolutionary Theory of Growth, pioneered by Nelson and Winter ([93]). Incidentally, one of the '47 hits in JSTOR with "simulation" in the title' (L&K, p. 305) would have been (cf. [92]) Richard Nelson's classic piece on *Simulation of Schumpeterian Competition*. The Nelson-Winter simulation-theoretic evolutionary growth models are no 'approximations' to any such 'economist's perfect formal model'; they are exact in their own right, in the formalism of the programming language with which they simulate. Surely, L&K must know that every programming language, if correct, must be exact and correspond to the mathematics of either recursion theory or one or another variety of constructivism. That they are not quite in command of this will become clear in the sequel.

[16] See, for example, [164], for a fairly complete list of such works.

[17] Dirac, in the *George Gamov Memorial Volume* ([29]), discussed '*The Variability of the Gravitational Constant*', especially now that the 'steady state model of the universe has fallen into disfavour'. In a cosmological world underpinned by the steady state theory (of Hoyle, Gold and others), 'all variations of natural constant are ruled out.' In a telling caveat to his precise discussion, Dirac notes (ibid, p. 58):

> "When one is making such a drastic change in Newton's laws as allowing the constant of gravity to vary, special consequences of Newton's laws like the conservation of angular momentum require reconsideration. One need some general principle as a guide to how Newton's laws are affected."

L&K can one find a definition – or meaning for – '*analytically tractable*'. It is possible they mean 'analytically *solvable*' in the sense that in the particular economic theory under discussion, or for the particular differential equation or, more generally, dynamical system, in consideration, it can be *proved* there *exists* an *equilibrium* or *a solution* (or a multiplicity of solutions). But this kind of *solvability* is quite independent of *tractability* in any sense in which that word is used in formal mathematics. We will have more on this aspect in the sequel. Finally, what can they mean by '*approximation of such models*', if the models are formulated in an 'exact formal language' that is devoid of any computable – i.e., recursion theoretic – or constructive underpinning? For example, it is clear that formal general equilibrium theory of the Arrow-Debreu genre, or orthodox game theory in its Nash incarnations, are undoubtedly 'formulated in terms of exact formal language' – at least in the eyes of the economic theorist who adheres to one kind of mathematics (real analysis underpinned by Zermelo-Fraenkel set theory + the axiom of choice). However, the equilibria and rationality associated with, and underpinning them, are provably uncomputable and non-constructive. How, then, can one devise a simulation model as approximations to them? The exact formal language in which these theories and their models are framed are provably impossible to approximate in any meaningful sense.

Our conclusion at this stage is, therefore, that the L&K starting point, to make a reasonable case for their main aim, i.e., that economists shun simulation, is untenable. They are, obviously, not economists[18], for if they were, they would not have tried to embark on this futile attempt to define 'the economist's perfect model'. Economists, in particular the practising ones, are better informed than try to appeal to a 'perfect model' which they have to 'approximate' in their empirical work. On the other hand, if L&K were to base their case with the starting point in two perceptive reflections on what, in another sense, can be interpreted as the futility of norming economic practice on the altar of the 'perfect model', they may have done their case – even if untenable on other grounds – more service. Clower, in his Presidential Address to the Southern Economic Society ([16], pp. 805-6) noted with characteristically acerbic wit:

> "Before I proceed, let me emphasize that by 'pure theory' I do not mean what most working economists ... mean when they use the word 'theory' without further qualification. Generally speaking, we mean by 'theory' the fact-oriented creative mixture of intuition, casual empirical knowledge, and seat-of-the pants logic that is found in virtually all 'applied economic analysis' and, indeed, in virtually everything called 'economics' before 1950. ... By pure theory I mean the axiomatically-based neowalrasian analysis of Arrow-Debreu .. . and closely related offshoots that serve as a standard of 'economic correctness' in all modern teaching not only in microeconomics but

[18] Their credentials in mathematics, mathematical physics and even the foundations of mathematics is equally questionable, if one is to infer anything from the many incorrect and infelicitous remarks they make, at various points, in the paper. Examples of these infelicities, regrettably, will have to be presented in the ensuing discussions.

in macroeconomics, money and banking, finance, and econometrics."

By '*economic correctness*' Clower means what Howitt referred to as '*the neowalrasian code*' (and what we think L&K should have meant when they tried to define 'the economist's perfect model') :

> "[A]dherence to an increasingly complex code of formal ideas has become the overriding criterion of success, rather than the fruitful modelling of observed phenomena. The code of modern economics has become for the most part that of neowalrasian analysis, with its rules for modelling all behaviour as the outcome of rational choice. .... But accounting for some phenomenon in a discipline dominated by an elaborate code consists not of telling stories designed to convince others that this is why the phenomenon exists, or why it appears the way it does, but of telling stories, no matter how *ad hoc,* that incorporate some aspect of the phenomenon, no matter how trivial, *without violating the code.* ....
>
> In many ways, modern economic theory has ....become a purely logical discipline in which the objective is to follow a set of a priori rules with no connection to the external world. ..... Economists building 'rational models' to account for things not found in conventional theory think of themselves as seeking explanation in the usual sense, whereas in fact they are just addressing *purely semantic questions* that do not even arise once one ventures out of the neowalrasian cloister. Only by the rarest fluke could someone working under such delusions come up with a convincing scientific explanation of anything."
>
> [63], pp. 75-6; italics added.

If we are to take seriously the L&K 'definition' of a 'simulation model' – given above – then they are, at best, purely semantic entities! None of the natural sciencs, certainly not physics or biology, even in their various modern incarnations, – whether applied or pure – have succeeded in defining its subject matter in terms of *an exact formal language.* Why should economists have tried to do it? In fact nothing or no one can be accused of this particular vice. That they are wedded to the neowalrasian code is what L&K should have meant and, then, they could make a case for the impossibility of its computability and, therefore, by direct implication the impossibility of any meaningful approximation. The result will, of course, have to be the logical non sequitur that those wedded to the neowalrasian code will have to *shun simulation.* For, what else can they do – especially since a strong factual case can be made, as we will show in the next section, that those who abandoned the neowalrasian code have been wedded, instead, to simulation, theoretically, methodologically and epistemologically.

3 *Reinventing the Square Wheel* - Yet Another 'Definition' of Economics

" ....

New lamps for old

Bright shiny gold

Innocent youth

Falsehood for truth

The eye of the needle, the loss of the thread..."

`Words` by Keith Reid (italics added)

Ricardo, with the characteristic integrity one has come to associate with him, in his now famous October 9, 1820 letter to Malthus ([107], pp. 278-9), delineated the scope of *classical economics*, in its 'magnificent dynamics' mode (*pace* Baumol[19]), in terms of the search for an understanding of the laws of (functional) income distribution:

> "Political Economy, you think, is an enquiry into the nature and causes of wealth – I think it should rather be called an enquiry into the laws which determine the division of produce of industry amongst the classes that concur in its formation. No law can be laid down respecting quantity, but a tolerably correct one can be laid down respecting proportions. Every day I am more satisfied that the former enquiry is vain and delusive, and the latter the only true object of the science."

Lionel Robbins effectively encapsulated the neoclassical dethroning of 'magnificent dynamics' and its preoccupation with efficient allocation of scarce resources with his famous definition of economics as ([108]; p. 24; italics added):

> "Economics, we have seen, is concerned with that aspect of *behaviour* which arises from the scarcity of means to achieve given ends."

Ricardo's definition is purely 'macroeconomic'[20], devoid of any reference to individual behaviour; Robbins, in true neoclassical spirit, enthrones the sanctity

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[19]

> "We consider these older dynamic systems [of the classical economists] simply because, although imperfect, they represent an approach of which there are few recent examples ... for the approach is of a *magnificent cast,* ambitiously attempting to analyze the growth and development of entire economies over relatively long periods of time – decades or even centuries."
>
> *[5], pp. 13-14; italics added.*

[20] Of course, the word 'macroeconomics' had not, then, been coined; it emerged in its modern senses, but within a context that would have been completely consistent with the classical usage of the phrase 'political economy', in the mid to late 1930s. A fairly complete note on the origins of the term 'macroeconomics' is given in [158].

of individual behaviour – and thereby, eventually, the fulcrum of rationality on which behaviour would be swivelled – in the search for efficiency with which ends can be met, given the scarcity of means.

Between them, outside the Marxian tradition, was that *Ricardian Methodological Individualist* (if this is not an oxymoron) *par excellence:* John Stuart Mill. 'Political economy' – the *Ricardian* Mill says – is concerned with man '*solely as a being who desires to possess wealth*, and who is capable of judging of the comparative efficiency of means for obtaining that end' ([87], p. 137)– the Mill who Robbins would have approved[21].

Now we have two philosophers *reinventing the square wheel*, by trying to redefine economics, wearing blinkers provided by their dependence on agent-based economics. 'Economics', these two philosophers state (*ibid*, p. 304), 'is concerned with aggregate outcomes of interdependent individual decision-making in some institutional context.' Indeed, among the many 'concerns' of economists, whether of micro or macro persuasions, whether game theorists or IO theorists and practitioners, whether those concentrating on law & economics or Institutional economics, whether the many interested in varieties of public choice and social choice theories, whether of behavioural or experimental economic concerns, and many other permutations and combinations of mentioned and unmentioned fields, this one, narrow, aspect is also of relevance – particularly to the practitioners of varieties of agent-based modelling in economics and finance. In passing, it may be useful to point out to them – and to the many agent-based modellers to whom they refer — that the origins of what these authors call 'generative science' (p. 310)[22] lies in the noble work of the 'last' of the Classical Economists, John Stuart Mill ([86], and, then, codified by his friend George Henry Lewes, [75]).

With this dubious reinvention of the square wheel, the authors then go on to state, use and invoke incorrectly, a series of economic concepts, to make sense of their claims and aims (of which there are many, not all of them consistent with each other, or the several references they invoke). It will be both tedious and somewhat embarrassing to list, discuss and dissect all of their infelicities. We shall only point out some of the more glaring ones, partly to put in perspective the discussion in the next main section, on the noble tradition of simulation in economics.

L&K, immediately after their codification of what they assert to be the 'concerns of economics', follow with two highly dubious claims (pp. 304-5): one, that 'microeconomic theory ascribes only relatively simple rules to individuals'

[21] A nuanced, almost, exhaustive discussion of these issues can be found in the excellent book by Wade Hands ([57]) - which should appeal to two philosophers who seem to have opinions - but very little knowledge - of methodological and epistemological problems in economics.

[22] It is possible that L&K bent over backwards to avoid using the current hypeword and phrase, *emergence* and *emergent science*, which are, however, freely and free-swingingly used by agent-based modellers, particularly those with macroeconomic persuasions and pretensions. The concept of *emergents* was introduced by Lewes (op.cit), on the basis of Mill's ideas (op.cit) on *heteropathic* laws. We have, in other writings, emphasized that a close and careful reading of these two classics leave us no alternative but to interpret their thoughts on these issues *algorithmically.*

choice behaviour' and, secondly, that 'market forms can usually be given an exact description'. The former claim, in a kind of variation on a theme, is later mangled into a patently false assertion on revealed preference, which simply strengthens our conviction that the authors have no grounding whatsoever in any kind of serious economics (p. 321; italics added):

> "[E]conomists consider revealed preference theory as .. a successful black-boxing [sic!] theory because it is taken to allow *for studying aggregate-level relationships* while making the internal workings of *individual minds* irrelevant."

Paul Samuelson must not only be turning, but positively writhing in pain, in his grave. Nothing in revealed preference theory, choice theory in general, or even in modern behavioural and neuroeconomics has anything to do with the 'internal working of the *mind*' of an agent[23], whether rational or not; at best, in modern economic analysis, there are references to the 'internal workings of the *brain*'. That two philosophers can be so sloppy on this distinction borders on the absurd. And, then, had the authors done what is expected in a scholarly article, with wide-ranging claims, of checking with the fountainhead of revealed preference theory, they would have read, in [116], p. 243 (italics added: where, by the way, Samuelson, with characteristic perspicacity, allows for what modern behavioural economists try to highlight):

> "[T]he *individual guinea-pig*, by his market behaviour, reveals his preference pattern – if there is such a consistent pattern."

Nothing in revealed preference theory, moreover, has anything to do with aggregate-level behaviour, unless, of course, L&K adhere to the neowalrasian code of the newclassical economists and work with the representative agent paradigm of macroeconomics, which they avowedly disown[24].

As for the possibility of giving 'an *exact description* of market forms' a wholly different set of economic theorists – living and dead – would be flabbergasted with the assertion, to put it mildly, especially if L&K mean by 'exact description' they mean in terms of an 'exact formal language'. We need only refer to the Tirole's encyclopedic text on The Theory of Industrial Organization, [144], for L&K to refresh their knowledge and understanding of the indeterminacies inherent in the interaction between market forms and individual behaviour. The relatively weak assumptions on individual behaviour – if we are charitable to grant this as the meaning of 'relatively simple behaviour' referred to by L&K – is bought at the price of a special class of competitive equilibrium market

[23] In fact, Samuelson does refer to the 'mind's eye', but not in the context of the agent's mind as the repository of individual preferences. The reference was, as a matter of fact, in the context of something that is highly relevant to the issues raised in L&K: numerical computations, approximations, algorithmic procedures and the like!

[24] What L&K need to refer to, at this point, is what we have come to call the Sonnenschein-Mantel-Debreu theorem (see, for example, [129] or [80]). It is this theorem, in the framework of the *neowalrasian code*, that makes it possible to be precisely 'loose' about individual preferences when using, say, an aggregate demand function.

behaviour (cf., just for starters, p. 7, [144]). However, as we go from this kind of market form to increasingly non-competitive market forms, the specification of individual behaviour becomes less determinate, superimposed upon the many indeterminacies in defining market forms. It may be apposite to recall Tirole's cryptic, but telling, point (*ibid*, p. 12):

> "The notion of a market is by no means simple. ... There is *no simple recipe* for defining a market, as is demonstrated by the many debates among economists and antitrust practitioners about the degree of monopoly power in specific industries."

If there is 'no simple recipe for defining a market', wherein is the origin for the claim that 'market forms can be given an exact description'? The rest of the first paragraph of L&K is replete with incoherent claims due to a lack of understanding that individual behaviour, in interacting economies, cannot be defined independently of the particular market form under which they interact – and even then there is no hope of either 'exact description', especially if 'exact' is to be 'in the sense of an exact formal language'[25], or defining 'simple rules to individuals' choice behaviour'. Indeed, it is easy to show, formally, that even the formalization of rational behaviour in its *simplest*[26] version requires the agent to be able to be more powerful than the most powerful, ideal, Turing Machine (cf. [152], chapter 3).

L&K claim 'that economists shun simulation for epistemic and understanding-related reasons is a factual one'. This claim is coupled to two of their explicitly stated aims (p. 306): firstly, in aiming 'to contribute to the recent philosophical discussion on scientific understanding' and that the 'economists' image of understanding emphasizes analytical rather than numerical exactness, and adeptness in logical argumentation rather than empirical knowledge of causal mechanisms'. Secondly, 'to explain and evaluate these reasons – for 'shunning simulations' – by considering the philosophical presuppositions of economists'. Now, these aims are interesting, even laudable; but 'the factual claim' on which their aims are explained and evaluated are, at best, flimsy and more generally simply untrue (as we will show in our discussions of the noble tradition of simulation in

[25] In particular, this is also a frequent mistake made by thoughtless computer simulations by agent-based modellers, especially in their so-called macroeconomics, seeking to mimic aggregative patterns, seemingly observable in actual economic data. In this endeavour – of seeking to mimic so-called patterns in aggregative data – they are not alone: the formidable orthodoxy of the newclassical economist's ubiquitous DSGE (Dynamic Stochastic General Equilibrium) methodology seeks the same goal. In this simplistic inductive methodology – partly the reason for the scepticism many theorists, even orthodox and eminent ones, express – they show very little respect for Trygve Haavelmo's early warning against such practice (even before his fundamental work which codified the Cowles Foundation methodology of Econometrics), in [52]). The only possibility in defense against Haavelmo's perceptive and serious criticisms of the kind of economics endorsed by L&K, untrammeled agent-based modelling, is to respond by claiming, correctly, that in this kind of work there is no *a priori* serious theory to begin with!

[26] There is, then, the question of what one should mean by '*simple*' and '*simplest*', which we have tackled in our work on algorithmic complexity theory, but that is quite another story (see [152], chapter 5).

economics). Even apart from this, if we grant them their 'definition' of simulation, then it is clear that anyone who does not subscribe to the *neowalrasian code* – *pro tempore*, to be identified with L&K's 'economist's perfect model' – will not endorse this kind of simulation. If, therefore, they are serious about their definition of simulation, then apart from the tiny orthodox group, ensconced in the *neowalrasian cloister*, all other economists and economics will, naturally and definitionally, shun simulation. This will, then, include past and present giants who have made simulation in economics are noble art, as well as a science: Ragnar Frisch, Erik Lundberg, Richard Goodwin, Edward Chamberlin, Bill Phillips, Robert Strotz, Wassily Leontief, Tjalling Koopmans, Herbert Simon, James Meade, Richard Nelson, Sidney Winter, Richard Day, Richard Stone, Vernon Smith and scores of other only slightly lesser mortals. It is interesting to recall that even in this partial list there are seven Nobel Laureates – some of whom were bastions of the neowalrasian code, indeed even helped in devising the code, in the first place.

Just for elementary starters, Herbert Simon devoted a great part of his fertile professional life towards investigating and elucidating 'empirical knowledge of causal mechanisms', by means of structured, theoretically underpinned, simulations. This particular aspect of Simon's monumental contributions to the interplay between economics, psychology, computer & administrative sciences and evolutionary theory, was based on the notion of causality defined, analytically, by Goodwin ([45]) to study, numerically and by structured simulations, the dynamics of coupled markets. But this is a topic for detailed consideration in the next section. We mention this here simply to emphasize the point that L&K stand and stamp on quicksand, with their 'factual claim', aims and definitions, and will get nowhere with them, except deeper down into a hole that will naturally be formed by the soil in which they stand.

L&K attribute to 'analytical economists' – by now one supposes that this class refers to those who are adherents of the neowalrasian code, which is itself an alias for the 'perfect model' – the preference to be 'exactly wrong rather than vaguely right' (p. 315). Neither of us are adherents of the neowalrasian code; however, we are serious students of the neowalrasian code, for, if not, how can one criticize them? Neither of us are aware of any serious adherent of the neowalrasian code of ever subscribing, explicitly, to this kind of sophistry. Once again, we believe this to be a gratuitous, unscholarly, throwaway remark, without substance or the possibility of substantiation. Indeed, it is also, once again, due to deplorable ignorance of the history of economic thought, in addition to less than sure grounding in economic theory – of any sort. The original version of this misquoted and unattributed 'aphorism' was an allusion to Marshall's method and attitude towards the construction of economic theory by Gerald Shove in his masterly essay on the role of Marshall's 'Principles' in the development of economic theory, during the first few decades of the last century, particularly in Anglo-Saxon academic circles. Shove's exact (sic!) allusion was as follows ([120], p.323; first set of italics added):

> "Partly no doubt this [i.e., the tendency within the theoretical com-

partment (of economics) for mechanical concepts and analogies to regain their primacy] has been due to the itch for precise results: not all of us are content to act on the late Prof. Wilden Carr's admirable motto (which might well have been Marshall's), '*It is better to be vaguely right than precisely wrong.*' ... In those parts of economics with which the *Principles* was concerned, there has been a distinct reversion to Ricardo's method and away from the Marshallian blend of realism and abstraction: a return to the mechanical as against the biological approach."

Where simulation interacts fruitfully with theory, as in the works of the many economists we have listed above, the preference if clearly for being 'vaguely right than precisely wrong', simply because it would be the first step in an iterative process that leads to a refinement of theory, feeding back next to a more refined simulation model, and so on. This was the way the FPU problem and its paradoxes have been tackled – although still without a complete resolution – and this was also the way Simon, Stone, Goodwin and Nelson-Winter, in particular, proceeded in their imaginative and outstanding theory construction. Of course, being 'exactly wrong' has its place in scientific practice within a Popperian scheme of things, if there is no other alternative. But the Popperian scheme of things is, in our opinion, less preferable to the Duhem-Quine world[27] in economics ([22]), outside the neowalrasian cloisters.

Finally, L&K display utter confusion, if not total lack of familiarity, with the meaning and scope of CGE[28] modelling, not to mention the theoretical bases that underpin varieties of *Computable General Equilibrium* modelling traditions, the kinds of fields designated Computational Economics and, therefore, end up by equating this area of many successes – and some failures – with the trivial work done by the adherents of the neowalrasian code, i.e., theorists and practitioners of the DSGE[29] approach. Computable General Equilibrium, in its deep mathematical and algorithmic origins, was the research program initiated by Herbert Scarf ([118]) to devise procedures to compute the Arrow-Debreu (and Nash) equilibria. This became a feasible research program[30] only after Uzawa proved ([150]an equivalence result between Brouwer's fixed point theorem and

[27] Which should be coupled to *Interval Analysis* ([88]) in the specification of computer codes to minimize the side-effects – sometimes catastrophic even in terms of, costing directly, human lives – due to the rounding errors of the digital computer's internal mechanism of floating point representation of reals ([59]). The significance of interval arithmetic in computer-aided proof will be mentioned later.

[28] L&K refer to CGE as Computational General Equilibrium, which is an elementary and trivial indication of their lack of familiarity of the rich and deep work initiated by Scarf to devise algorithms to compute the Arrow-Debreu General Equilibrium (GE). The path to DSGE was that from GE to AGE (Applied General Equilibrium) to RCE (recursive Competitive Equilibrium) and,then, to its current dominant position in macroeconomics as DSGE (Dynamic Stochastic General Equilibrium). We will have more to discuss on these topics in section 4, below.

[29] **D**ynamic **S**tochastic **G**eneral **E**quilibrium – **DSGE** – but also referred to, sometimes, by transposing the first two words and using **SDGE** as the acronym.

[30] At least in the opinion of the practitioners of AGE (cf. [121], [119] [85] and [68]).

the Walrasian equilibrium existence theorem (cf. [154] and [157] for full details of these issues). These were not 'models to conduct computerized *macroeconomic* thought experiments' (L&K, p. 309; italics added). They were, if anything, the original and theoretically absolutely solid – though *computably* dubious – agent-based models (not just thought experiments) which were set in their empirical paces in computerized simulation policy experiments.

As for computational general equilibrium models, this, too, in one noble tradition, was – and remains – a well-defined field of research with a clearly attributable origin: in the remarkable work by the Norwegian economist, Leif Johansen ([67]). Had L&K done their homework and read also the first volume of the two volume series, the second of which contains the article by Leigh Tesfatsion ([140]) to which they refer[31], they would have read the lead article by Dixon & Parmenter ([30]) and could have avoided the *non sequiturs* on CGE. In fact it is the 'other' tradition of computational economics, that of which the agent-based modelling approaches are a leading *genre* to which the L&K comment that the field is 'not a very clearly defined umbrella term for the various computational approaches' can be applied (although not even L&K would be foolish enough to claim that agent-based modelling arose 'from certain branches of the theories of general equilibrium and real business cycles')[32].

A result of this lack of familiarity with the serious fields of CGE, AGE, RCE, DSGE and the Leif Johansen tradition of computational economics, and the peculiar definition of *simulations as approximations to a perfect mode*l, L&K are forced to make the untenable assertions on 'simulations' and its role being confined to 'computing the equilibrium paths of macro-variables', in the formal game played by the inhabitants of the neowalrasian cloisters (p. 309).

4 Computation, Discretization, Proof and Other Mathematical Infelicities

> "[T]here is, strictly, no such thing as mathematical proof; that we can, in the last analysis, do nothing but *point*[33]; that proofs are what Littlewood and I call *gas*, rhetorical flourishes designed to affect psychology, pictures on the board in the lecture, devices to stimulate

[31] Incidentally, the word 'constructive' in this article by Tesfatsion has nothing to do with constructive mathematics.

[32] There is a quite different tradition of Computable General Equilibrium, pioneered by Lance Taylor ([136]), where the foundation is *not* the neowalrasion code, but Stock-Flow Consistent Social Accounting Matrices, a tradition to which also the distinguished macroeconomic equilibrium growth theoretic work of the Nobel Laureate Richard Stone belongs ([130]). But this is a topic for the next section.

[33] But 'point' at what? At selecting, in the face of an *undecidable disjunction*, a particular subsequence from a closed and bounded sequence of which it is a member? Or does one point at the *choice* – i.e., a 'selection' – of an element from an uncountable infinity of sets, appealing to the *axiom of choice*? These are the kinds of selections and choices that are routinely appealed to, and claimed as feasible, in the proofs of theorems by the members of the neowalrasian cloisters.

> the imagination of pupils. This is plainly not the whole truth, but there is a good deal in it."
>
> [58], p. 18; italics in the original

It may not be a particularly generous or enlightening strategy of exposition to begin a section with what we think is an egregious error, from economic, mathematical and computational points of view. But the example with which we begin is, in a sense, paradigmatic of the confusion in L&K's own exposition and sorting it out may help the unsuspecting reader from propagating these errors into posterity.

In their highly slippery discussion of the interaction between individual rational behaviour and equilibrium, they point out that 'the equilibrium that is used to solve an analytical problem is based on mutual expectations' which, in turn, requires a resolution of an infinite regress in mutual expectations (pp. 310-1). They go on (p. 311):

> "The role of equilibrium is that of breaking this and thus enabling the derivation of a definite solution. In equilibrium, none of the agents has a unilateral incentive to change behaviour, and hence the equilibrium 'determines' how the agents will act. Computers cannot model such an infinite regress of expectations because, being based on constructive mathematics, they cannot handle it."

The *non sequiturs* in this paragraph are embarrassingly many, and covering one of these by enclosing 'determines' within quotation marks does not hide the obfuscation. Essentially, L&K are groping towards a definition of a Nash equilibrium, but the economic environment and basis of agents' behaviour is not fully and well specified for one to be very certain of this; the whole set up could easily be for some kind of more dynamic game theoretic set up, but let that pass. The key objection we have to the claim in this statement can be divided into three sub-objections:

'Computers cannot model such an infinite regress';

Computers, are 'based on[34] constructive mathematics';

Constructive mathematics 'cannot handle' infinite regress;

We will take these claims in the above order. First of all, it is plainly incorrect that 'computers cannot handle an infinite regress', particularly the kind of infinite regress they define (admittedly in a very loose way). They fail to point out that the infinite regress in expectation they refer to, in economics and game theory, are 'broken' by the utilization of one or another fix point theorem, usually *non-constructive* and *uncomputable*[35] ones. There are eminently

[34] Presumably, they mean 'computer behaviour', i.e., the underlying *program* on the basis of which the computer processes data, whether numerical or not.

[35] It is not clear that L&K understand the difference between recursion theory – i.e., computability theory – and constructive mathematics.

respectable constructive and computable fixed points that can be utilized to 'break' the infinite regress emerging from the potential indeterminacies of mutual expectations. One of us (see [155], §4) has, in fact, devised and derived a perfectly well-defined rational expectations equilibrium using the standard mathematics of the computer – i.e., 'recursion theory'. Moreover, there is an eminently rigorous fixed point theorem in constructive mathematics, derived and proved constructively, on the basis of intuitionistc logic, by no less an authority than Brouwer ([14]), which can be used to define the kind of equilibrium with the infinite regress of mutual expectations L&K seem to want to define.

The caveat to these objections and our counter-claims are, of course, the implication that one must go back to the proverbial 'drawing board' and formalize the basic closures of economic theory – especially preferences and endowments, but technology, too, eventually – either in recursion theoretic terms of constructively. Both of these enterprises are feasible, have been achieved successfully and are, then, entirely consistent with using either a computer running on recursion theoretic principles or on constructive mathematics foundations[36].

Secondly, it is simply not true that computers – i.e., any standard, working, computer, particularly those that are accessed by any and every economist – and their associated workings are 'based on constructive mathematics'. In every standard, working, computer, the mathematical basis is recursion theory (computability theory), if anyone cares to think deep enough about it. It is not as if one cannot make a working computer on the basis of constructive mathematics, or even make the standard Turing Machine realization implement programs written in a language adapting some version of constructive mathematics[37].

Thirdly, it is absolutely false that constructive mathematics 'cannot handle infinite regress especially the kind needed in the L&K framework of individual behaviour. Even apart from this, it is entirely feasible to handle varieties of 'infinite regress' within constructive mathematics, except that the kind of 'infinities' are more carefully defined and invoked and, therefore, the nature of economics in a constructive mode would be very different from the orthodox theory of individual rational behaviour, equilibria – whether game theoretic or not – and, above all, the associated solutions (particularly via existence proofs, typically of equilibria)[38].

Obviously, the authors are blissfully innocent of any knowledge of the vast

[36] For a representative, but not exhaustive, sample of such work, see [164].

[37] See the eminently readable text by Nordström, Petersson & Smith ([96]) for an elegant and accessible introduction to Martin-Löf's *type theory*, developed 'with the aim of being a clarification of constructive mathematics' (ibid, p. 1). several of the essays in [21], particularly chapters 1 & 6, are equally illuminative on the kind of approach to programming practicable computers with program languages developed for the specific purposes of encapsulating constructive mathematics.

[38] Unfortunately, L&K go on to compound the above infelicities with a further absurdity when they continue (ibid, p. 311):

> "However, they [i.e., the computers] can be programmed to check for each possible strategy combination whether it constitutes an equilibrium."

How does a computer, whether based on recursion theory or constructive mathematics, 'check' for an equilibrium which is provably uncomputable and non-constructive?

and continuing research – *without* complete resolution – on the issue of *structural stability* in *dynamical systems*. Otherwise, they would not claim (p. 317)[39]:

> "[T]here is no straightforward procedure for testing for robustness with respect to small changes in parameter values in analytical models."

However, even if they knew anything about the deep results (and non-results) in structural stability for dynamical systems, it would not be comfortable for them to be told that ([1], pp. 120-1; underlined emphasis in the original):

> "The ubiquity of structurally unstable motions .... suggests that structural stability is not an appropriate concept for *experimental systems*. .... Here we may hazard a conjecture: all natural systems are dynamically stable. In fact, we will probably evolve the definition of stability until this conjecture becomes true."

L&K, in particular, especially because they never really distinguish between static and dynamic stability, and the kinds of dynamical systems in which stability plays any role, and the subtle differences in the kind of *correspondence principle* ([115], pp. 5 & 258, ff) – a component of which is comparative statics – one can invoke, they would not understand how to make sense of Leontief's characteristically perceptive observation ([50], p. 68; italics added):

> "Professor Leontief does not accept [that instability is an unrealistic hypothesis] and maintains that we may utilize *dynamical systems that are unstable* throughout and cites capitalism as an example."

The question, then, is: whether dynamic economics (aggregative or not), modelled as a (nonlinear) dynamical system, is a *natural system* (Abraham), an *experimental system* (Abraham) or an *empirical system* (Leontief)? There is no *apriori* reason for any of these kinds of dynamical systems to be stable for observational, simulational or experimental purposes ([104]) – especially also since it is easy to show that only dynamical systems incapable of being underpinned by any notion of maximization ([117], p. 12) are capable of *computation universality* and, hence, consistent with the standard assumption of rationality in economics. These are among some of the reasons why the L&K discussions of comparative statics and robustness are both less than useful and highly misleading.

[39] Their whole discussion of 'robustness', 'sensitivity' and 'comparative statics', in economics, is replete with the same kind of embarrassing infelicities as the ones we have catalogued and discussed above. We refrain from going into further details on these particular issues because we have just realized – after the first draft of this paper was completed – that the authors, with an additional co-author ([69]), seem to have recently embarked on an adventure in *Economics as Robustness Analysis*. We will have to reserve our critique of the issue of robustness analysis, and related concepts, as presented by these authors, for a different exercise. Suffice it to say that – as in the case of the embarrassing ignorance of structural stability - the many and varied allusions to 'comparative statics' in this paper are mostly without substance. The one attempted 'formalization' of 'robustness', on pp. 316-7, is a tissue of formal confusion.

The analysis, discussions and explanations in section 4 of L&K, pp. 318-321, are particularly obscure, when not outright incorrect. To being with, what is a '*digital proof*'[40] being contrasted with? The context of their discussion suggests that the alternative is a so-called 'analytical proof'. If they mean by a 'digital proof', those theorems that are provable by programming a digital computer, then every *analytical* proof, say in standard textbooks on constructive *analysis* ([7], [8] or [9])[41] is a *digital proof.* If by *proof* they mean, say, those sanctioned by *intuitionistic logic* only, then every reference to 'proof' in their paper, and all of the 'proofs' of theorems derived within the *neowalrasian code,* fail to be acceptable. Even if not underpinned by intuitionistic logic, in many varieties of constructive analysis – for example in [7] – no appeal will be made to the *tertium non datur* in cases where infinitary instances have to be considered. Hence, any proof of a neowalrasian theorem, derived with appeal to the Bolzano-Weierstrass theorem, cannot and *will not be considered a valid proof existence*. Surely, the authors must know that both the Nash equilibrium, as derived by John Nash, and the Arrow-Debreu equilibrium, are theorems whose proofs are based on the Brouwer[42] (or Kakutani) fixed point theorem, whose proof invoked the Bolzano-Weierstrass theorem.

So, what do L&K mean by 'proof'. L&K do point out that (p. 319; italics added):

> "[A] computer program could be seen as a kind of logico-mathematical *argument*, albeit a particularly *long and tedious* one."

We will have to assume that proof, according to L&K, is a 'logico-mathematical argument'. Now, every valid computer program is a 'kind of logico-mathematical argument', but what kind of *logic* and which branch of *mathematics* underpins a computer program? We have, in earlier paragraphs, belaboured this point with mention of constructive mathematics, recursion theory and intuitionistic logic. In this precise sense, therefore, every computer program is a proof in a strict mathematical sense. However, they go on to 'spoil' this useful observation – of

[40] The title of section 4 is: 'What is Wrong with Digital Proofs?' This is the only occurrence of the phrase 'digital proof' in L&K. Hence we have to infer what is meant from the context, by interpreting, appropriately, phrases such as (p. 319):

> ".. consider the implications of the possible differences between analytical and computerized proofs."

Note, however, that 'a computerized proof' can be perfectly analytic, depending on what one means by 'analytic'.

[41] See also the elegant and illuminative discussion on *Algorithm in Modern Mathematics and Computer Science* by Donald Knuth ([70]), where he states unambiguously (p. 94):

> "The interesting thing about [Bishop's Constructive mathematics] is that it reads essentially like ordinary mathematics, yet it is entirely algorithmic in nature if you look between the lines."

[42] Of course Brouwer did derive, forty years after he first derived the non-constructive version of the fixed point theorem that bears his name, a fixed point theorem based on intuitionistic logic, that had the express aim of avoiding any reliance on the Bolzano-Weierstrass theorem (see the first footnote in [14]).

'a computer program could be seen as a .. logico-mathematical argument', by a stunted appeal to Tymoczko's discussion of 'surveyability' of proofs ([149]). Tymoczko suggests (*ibid,* p. 59) three characteristics of proof: that they should be *convincing*, *surveyable* and *formalizable*. He, then goes on to claim that surveyability and formalizability 'are the deep features [of a proof]', and that (p. 61-62; italics added):

> "It is because proofs are surveyable and formalizable that they are convincing to *rational agents*.
>
> Surveyability and formalizability can be seen as two sides of the same coin. .... Can there be surveyable proofs that are not formalizable or formal proofs that cannot be surveyed? Are all surveyable proofs formalizable? .... Given any sufficiently rich theory, we can find a surveyable proof of a statement of that theory which has no formal proof. ...
>
> Are all formalizable proofs surveyable? ... Here the answer is an easy no.
>
> However, if we stop to think about this situation, it appears unlikely that this logical possibility can ever be realized. ....
>
> In summary, although formal proofs outrun surveyable proofs, it is *not at all obvious* that mathematicians could come across formal proofs and *recognize them* as such without being able to survey them."

However, one cannot let these interesting remarks pass unchallenged! First of all, who or what is a 'rational agent'? It is entirely conceivable – and formally demonstrable (see, for example, [106], [152], especially chapter 3, and [160], especially parts II & IV) – that an effective characterization of the behaviour of a rational agent in the sense of economic theory is formally equivalent to the computing activity of a Turing Machine. Next, Tymoczko is admirably clear in defining the concept of formalizability and formal proof – both by appealing to results in model and proof theory and to Gödel numberings of formal proofs considered as mathematical objects – but does not define or characterize the meaning – formal or not – of surveyability! In the case of surveyability he – and L&K – fallback on intuitive concepts such as 'rational agents', humanly surveyable', 'recognize', and so on. Suppose, however, Tymoczko (and L&K) did formally define or characterize formally the notion of surveyability, in the same sense in which the intuitive notion of effective calculability was encapsulated in the formal notion of a Turing Machine or the λ-calculus, or partial recursive functions – all formally equivalent to each other by the Church-Turing *Thesis*. Then, it will be possible to show that a *rational agent* will not be able to *recognize* as *surveyable* the proof of some theorems by appealing to the *Halting Problem for Turing Machines*. Tymoczko's admirable and informal discussion is valid – formally, of course – only on the basis of an invalid asymmetry between his way of defining, implicitly, the notion of formalizability and formal proofs, but leaving to the intuitive domain the characterization of surveyability. This makes the rest of his 'philosophical' arguments against accepting the Appel-Haken proof of the four-colour theorem much less than formally convincing. *A fortiori*, therefore, L&K's appeal to them for their loose case against considering 'computer-assisted proofs' as 'mathematical proofs'. There are many other ways we can cast seriously rigorous doubts against Tymoczko's and L&K's loose, allegedly philosophical, arguments against considering 'computer-assisted proofs' as 'mathematical proofs', but this must suffice for the moment[43].Within this context of the discussion of computer-aided proofs, L&K have thoughts on 'program verification'but fail to state exactly what they mean by 'program verification'[44]. It is not as if there are no rigorous, formal, definitions, even in graduate textbooks with impeccable credentials ([24], comes to mind at once; see p. 536). However, a serious discussion of *program verification*, to substantiate the kind of loose assertion in L&K, requires much more depth and understanding of the issue, for example as in Platek's crystal clear, almost pedagogic, yet deep, article ([103]).

They claim (p. 319; italics added) that the:

> "[C]onsensus view concerning program verification seems to be that it is, in principle, possible to check any program for errors, but that it may be prohibitively arduous or even *humanly impossible* to do so. .... It is thus possible to construct logical proofs of program correctness. In practice, such proofs are seldom presented ... because they are complex, boring and usually their presentation

[43] Almost everything that L&K write on 'program verification', in this context, never rises above the trivial, and even the banal. They are, very clearly, without any grounding in the rich and burgeoning literature on 'program verification' and their inherent formal undecidabilities. Here, too, they get away with loose claims, referring to even looser references, simply because they do not bother to state clearly – i.e., formally – exactly what they mean by 'program verification'. On the other hand, every time they do try to define any concept 'rigorously', they trip over them and on them and make a mess of their claims, anyway. For example, it is all very well to refer to 'De Millo, Lipton and Perlis, 1979' ([27]), on p. 319, when suggesting that 'program verification' is an academically and financially unrewarding exercise. Quite apart from this being untrue – the financial and academic rewards for usable results on 'program verification' are considerable, given their importance in cryptology, patent codification, and other similar security related fields – one would have expected L&K to refer also to the counter-argument to [27], given with pungency and clarity by Fetzer ([33], p.1062; italics added):"The fact that one or more persons of *saintly disposition* might sacrifice themselves to the tedium of eternal verification of tens of millions of lines of code for the benefit of the human race is beside the point. The limitations involved here are *not merely practical.* ..... In maintaining that program verification cannot succeed as a generally applicable and completely reliable method of guaranteeing the performance of a program, De Millo, Lipton and Perlis thus *arrived at the right conclusion for the wrong reasons."* Actually, however, the nuanced discussion in [27] is far richer and more persuasive than the caricature of the message in it summarized by L&K.

[44] Except to state that (p.319):

> "It is also, in principle, possible to check computer codes for errors because from the *syntactic* perspective the code is comparable to mathematical symbolism."

They forget – or, more likely, do not know - that program verification is a part of denotational *semantics* (see [24]). But even if we grant them this 'definition', what is the scope of 'comparable'?

> does not provide the author with much in terms of academic prestige or *financial gain*." "

Apart from this being a false claim, what exactly do they mean by '*humanly possible*', '*logical proofs'* or *'complex'*? Suppose the logic in question is intuitionistic logic and the mathematics in which a proof is devised in constructive, then the validity of the proof is equivalent to the validity of the program. And, would they include among allowable humanly possible processes, in this context, those of the 'six-and-thirty of the (forty) lads, employed by a Professor at the Grand Academy of Lagado, described with wit and venom by Swift, in Gulliver's *Voyage to Laputa* ([135], pp. 213-4):

> "To read the several lines softly as they appeared upon the frame; and where they found three or four words together that might make part of a sentence, they dictated to the four remaining boys, who were scribes. This work was repeated three or four times; and at every turn, the engine was so contrived that the words shifted into new places, as the square bots of wood moved upside down. Six hours a day the young students were employed in this labour; and the professor showed me several volumes in large folio already collected of broken sentences, which he intended to piece together, and out of these rich materials to give the world a complete body of all arts and sciences; which, however, might be still improved and much expedited, *if the public would raise a fund for making and employing five hundred such frames in Lagado, and oblige the managers to contribute in common their several collections.*"

And, would they – L&K – please also specify the time horizon of 'humanly possible'! After all, the purveyors of the perfect model of economics, those in the neowalrasian cloisters, have no compunction about employing rational agents who decide over infinite horizons, in an 'as if' world, of course. But that is part of living in Cantor's Paradise, in which the neowalrasian cloisters are situated.

Thus we disagree quite profoundly with the utterly unsubstantiated claim by L&K, that (p. 320):

> " It goes without saying that program verification is more difficult in practice than verifying an analytical proof: there are simply more factors that can go humanly wrong."

Apart from this repeated appeal to something undefined called 'humanly' repeatedly, we – in our human, intellectual, capacity – would be very happy to provide, for every 'difficult in practice' computer-aided proof (or 'digital proof', if L&K give a formal definition of this term, as one about non-constructive 'analytical proofs'), an equally difficult 'analytical proof', constructed by ordinary human beings, that has gone wrong, quite seriously. Here, too, then, we must rely on 'one or more persons of saintly dispositions' to 'sacrifice themselves to the tedium of eternal verification' of the validity of 'analytical proofs'![45] Just off the cuff, we have in mind something that is of concern for us in our own research on economic dynamics: the (in)famous example of *Dulac's Theorem*, claiming to have 'proved' a theorem contributing to the resolution of the second part of the 16th of Hilbert's famous 23 Problems[46]. It was published by Dulac in 1923; it was only more than half a century later, that the errors in the original proofs by Dulac were corrected, by Yulij Ilyashenko, and, independently, by J. P. Écalle[47] (see, for example, [31][48], [100], chapter 3 and [65]).

More pertinently, we would like to provide two examples of 'computer-aided' proofs, both executed with full cognizance of the difficulty of program verification but, at the same time, with rigorous and transparent criteria explicitly made, to make sure that any 'factors' that 'can go humanly wrong' can be detected and corrected, if anyone wishes to do so. But more importantly, the first example shows the intimate way *mathematical theory*, *experimental simulation* and deep *numerical analysis* was brought to bear to resolve a long-standing paradox. The first is the very recent proof of the existence of the *Lorenz Attractor* ([146]). In 1985, no less an authority on dynamical systems theory than Morris Hirsch observed, for the Lorenz System[49] ([62], p. 191; second set of italics added):

> "[C]haotic behaviour has not been *proved*. As far as I am aware, practically nothing has been proved about this particular system. ... It is of no particular importance to answer this question; but the lack of an answer is a sharp challenge to dynamicists, and considering all the attention paid to this system, *it is something of a scandal*."

In the same volume in which Hirsch's article appeared, another distinguished dynamical system theorist, Ralph Abraham, added his nuanced opinion – in softer phrases – to this 'scandal' ([1], p. 117; italics added):

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[45] Witness the brouhaha surrounding the recent claim by Vinay Deolalikar – in no less a medium that the World Wide Web! – that he had solved one of the *Clay Millennium Problems*, that of resolving the $P \stackrel{?}{=} NP$ conundrum. One supposes that his claim was motivated entirely by a sense of intellectual achievement; but the hundreds who seem to have engaged themselves in 'verifying' the validity of the proofs are obviously of a 'Saintly Disposition'!

[46] The second part of Hilbert's 16th Problem remains unsolved, to this day.

[47] Incidentally, Écalle's proof of Dulac's conjecture was constructive (see [32]).

[48] We must confess that we have never actually read the original by Dulac, mainly because neither of us are capable of reading any intricate mathematical text in French. Our own favourite text on this problem, which also gives a complete report on the rich Chinese tradition of research in this area, is the monograph by Ye Yan-Qian and his many collaborators, [168].

[49] The Lorenz System is as follows:

$$\frac{dx}{dt} = -10x + 10y$$
$$\frac{dy}{dt} = 28x - y - xz$$
$$\frac{dz}{dt} = -\frac{8z}{3} + xy$$

> "The chaotic attractor of *mathematical theory* began with Birkhoff in 1916. The chaotic attractor of *simulation experiment* arrived with Lorenz in 1962. .. The identification of these two objects has not yet succeeded, despite many attempts during the past twenty years. Of course, everyone (including myself) expects this to happen soon .. ."

However, Abraham's own take on the 'scandal' was expressed in another way, a little further down (p.118; underlined phrase in the original, italics added):

> "However, most of the time *experimentalists* observe not braids (rationally related frequencies) but quasi-periodic motions (apparently irrationally related frequencies). That is the quasi-periodic paradox. *More than one scientists has lost faith in mathematics because of the ubiquity of this illegal motion in the natural world*."

The most interesting point here is that the 'scientist lost faith in mathematics' because *it* was not able to make sense of the *simulation experimentalists observation*. L&K, would claim, justifiably, we think, in this case, that the economists in the neowalrasian cloisters – their *perfect model economists* – would 'lose faith in the simulation experimentalists observation'!

Now, the Lorenz system is the paradigmatic repository of the property that almost characterizes so-called chaotic dynamical systems: *sensitive dependence on initial conditions* (SDIC). In such a system, then, what can L&K mean with (p.320; italics added):

> "[I]n discretizations it is necessary to check that the computer model is presented *in exactly the same way* as the analytical model upon which it is based."

Since L&K require the 'presentation' to be to – presumably – a *digital computer*, 'discretization' presupposes that the 'analytical model upon which it is based' is *continuous* in some rigorous, well-defined, sense. However, what does '*exactly the same way*' mean? Do they mean the 'presentation' of the 'analytical model' is to be in its original continuous form? For example, in the above case of the Lorenz system, are they expecting the analyst, experimenter, simulator or whoever, to be able to use the digital computer to faithfully replicate the dynamics of the continuous time-space Lorenz system's nonlinear dynamics – *SDIC and all* – 'exactly the same way'? But no serious experimenter, simulator, numerical analyst, or even a mathematically competent dynamical system theorist, would forget that *the digital computer has its own way of truncating floating point representation of real numbers, depending on its internal, built-in, precision.* A system of nonlinear equations, such as Lorenz's, susceptible to the problems of SDIC, cannot, therefore, almost by definition be represented 'exactly the same way', if we interpret the phrase in its obvious, intuitive, way (for lack of a formal definition).

On the other hand, suppose we interpret 'exactly the same way' to mean that the numerical method that is implemented on the digital computer to simulate, experiment with, or analyse, the Lorenz system, should be mathematically equivalent to it, then we must ask what '*mathematical equivalence*' entails. This is one kind of frontier research in the interface between nonlinear dynamics and theoretical numerical analysis, elegantly summarised in ([133]).

Another way to make sense of this thorny issue of 'exactly the same way' would be to construct a Turing Machine equivalent of, in this case, the Lorenz system. Then, of course, the question becomes: What is the meaning of 'Turing Machine equivalent? Again, a precise answer can be given (as one of us has tried, over the years and in many of his writings; cf, for example, [152] and [151]), so that one circumvents the pitfalls of discretizations and the rounding errors due to the computer's internal floating point representations and truncations.

Finally, there is the fairly straightforward alternative of using *Interval Analysis* (cf., [88]) for the numerical method that is implemented in the digital computer to analyse, experiment or simulate the continuous time-space system, in this case, of course, the Lorenz system. It is this alternative that is chosen in Tucker's computer-aided proof of the existence of the Lorenz attractor ([146], especially pp. 1200-11).

But, surely, the existence of the Lorenz Attractor should be a classic *analytical proof* – perhaps utilizing one or another (non-constructive) fix point theorem? Why, then, this preoccupation with 'discretizations' and 'presentations of exactly the same model' to a digital computer? For the same reasons that the proof of the four-colour theorem was achieved by Appel and Haken with the aid of a digital computer (see, for accessible, but quite complete details, [114], chapter 3). The parallels are even more than just the recourse to a digital computer to evaluate complex numerical calculations. In [146] (p. 1199), he begins with a classic mathematical method of an *Ansatz*, an intuitive hunch, which will, hopefully, be confirmed by the results of the complete analysis and necessary evaluations. The intuitive hunch is not a frivolous guess; it is an educated guess of the right starting point, based on a thorough knowledge of all possible aspects of an unsolved problem - in this case that of finding a correspondence between a mathematical object and an experimentally discovered one. In Tucker's *Ansatz*, normal form theory is combined with rigorously implemented digital computations are brought to bear on getting the desired final result. In deriving the normal form, an analytic change of coordinates leads to a classic small divisor problem, which to complete a necessary element of the analytic proof requires the numerical evaluation of 19,386 low-order divisors[50]. It is here that the 'computer-aided' part of the proof acts as a 'scratch pad'.

Correspondingly, it is possible to identify the *Ansatz* in the Appel-Haken proof: it is a particularly well-informed probabilistic argument establishing,

[50]The knowledgeable reader would immediately recognize the similarity with the origins of what eventually became the celebrated *Kolmogorov-Arnold-Moser* (*KAM*) theorem in dynamical systems theory. Small divisors, quasi-periodic orbits, perturbations (of Hamiltonians) – all issues we have had to mention in our various discussions, above – play significant parts in the motivation and the eventual formalization Kolmogorov's original conjecture.

with *almost* absolute certainty that[51] ([114], p. 83; italics added):

> "[T]here must exist some *discharging procedure* producing *an unavoidable set all of whose configurations are reducible*. That is, they showed that the computer-assisted reducibility proof was overwhelmingly likely to succeed ..."

And proceeded to do just that! Of course, they, too, could have emulated the Professor in the Academy at Lagado, in case the purists preferred a dozen Ramanujams or a few thousand ordinary computing geniuses to do the computations for producing the 'unavoidable set all of whose configurations are reducible.'

However, in these kinds of hybrid proofs, where the analytic (usually non-constructive) and the numerical or combinatorial elements are brought to bear upon a procedure or a thought-experiment, the dividing line between the domain of the two has to be carefully distinguished. In Tucker's case, (1199) 'a change of variables ... in a small cube centered at the origin, transforms the Lorenz equations ... into a carefully selected normal form... Inside the cube, we can then estimate the evolution of trajectories *analytically*, and *thereby we avoid the problem of having to use the computers in regions where the flow times are unbounded.'* The construction of the small cube, via the change of variables, entails 'an analytic change of coordinates', which ..introduces a small divisor problem', all 19, 386 of them, which then necessitates recourse to a digital computer and to interval analysis to compute, numerically, their estimates.

Of course, Tucker, also, could have emulated the Professor at Lagado and hired a few thousand 'rational agents' with exceptional computing abilities – perhaps a few dozen Ramanujams would have been sufficient – to dispense with the dreaded, program-unverifiable, digital computer. Mercifully, the versatile Tucker wrote a 'small C-program, SMALLDIV.C' (ibid, p. 1200) to estimate these small divisors.

An illustration of this point is made in Ruelle's report of one of Oscar Landford's computer-aided proofs ([112], p. 100)[52]:

> "My colleague Oscar Landford reported once on a theorem [whose] proofs [was] computer aided, which means that it consists of some mathematical preliminaries and then a computer program. The program (or code) uses *interval arithmetic to check various inequalities*; if these are found to be correct, the theorem is proved. The complications of the problem forced Landford to write a relatively long program, about 200 pages. .... Oscar Landford is a very careful

[51] Having first produced 1936 *reducible configurations*, at least one of which had to occur in any *planar triangulation*.

[52] In the spirit of complete honesty and candour with which we have written this paper, in fairness to the sceptics of the mathematical purity of computer-aided proofs, we must inform the reader the following fact. The continuation of the above quoted paragraph by Ruelle may be 'rather disheartening' to people like us, who believe that such proofs are on an equal footing, mathematically, to so-called 'analytical proofs'.

> person, and he took pains to check that, when the code is fed into the computer, the computer does exactly what it is supposed to do. In this manner – after the computer has agreed with the inequalities in the code – the proof of the theorem is complete."

Scarf's elegant, clear and complete exposition of the genesis of the CGE research program ([118]), admirable though it is – and resides as the core fountainhead of the genesis of the core of current orthodoxy in the neowalrasian cloisters, the Real Business Cycle (RBC) model's Recursive Competitive Equilibrium (RCE) – simply does not confront the conflict between the analytical and the constructive or the computable domains. The interplay between the analytical and the numerical was bridged by the *Uzawa equivalence theorem* and the parallels with discharging procedures, unavoidable sets and reducibility can be identified with the *construction* of a specific sequence of primitive sets, replacement operations, labelling, etc. In fact, a study of the precise nature of the computer-aided nature of the establishment of *Scarf's Theorem* (*ibid*, p. 45, Theorem 2.5.1) and its utilization in demonstrating the original Brouwer fixed point theorem would be the starting point for a way to reduce *the remaining indeterminacy* in this research program (p. 51): the constructive or computable determination of 'a convergent subsequence of subsimplices... which tend in the limit to a single vector x^*.' The missing link is an *imaginative* Ansatz[53]. In its absence, the CGE program, followed by its uncritical application by the AGE practitioners and, then, taken up even more uncritically by the RBC theorists, remains unfinished because ([118]), p. 52:

> "The passage to the limit is the nonconstructive aspect of Brouwer's theorem, and we have no assurance that the subsimplices determined by a fine grid of vectors on [the price simplex] *contains* or *is even close to a true fixed point* of the mapping."

Yet the whole program has been accepted as having been successful in determining constructive and computable methods to locate Walrasian equilibria, proved to exist by Arrow and Debreu, of course, non-constructively. This magic transformation of a non-constructively derived uncomputable equilibrium, via an algorithm that appeals to an undecidable disjunction during its execution, is uncritically accepted by the inhabitants of the neowalrasian cloisters and is taken to define – implicitly, of course – the 'perfect model' of the economist. No wonder, then, that this kind of economist 'shuns computation'; he or she knows, perhaps, instinctively, that there is no point in simulating anything, using a non-constructive algorithm, to find an uncomputable equilibrium

[53] The *Ansatz* will have to find a way either to avoid any appeal to the Bolzano-Weierstrass theorem or to work directly with constructive mathematics without undecidable disjunctions. In fact, Brouwer's Intuitionistically corrected proof of his original theorem ([14]) is the solution - but only if the foundations of the theory developed in the neowalrasian cloisters is redone in terms of constructive mathematics. Our own intuition is that ordinary economic theory, formalized on $\mathbb{N}$, $\mathbb{Q}$, or $\mathbb{Z}$, would avoid reliance on fix point theorems for the proof of equilibrium and, hence, would be amenable to a fruitful interaction of the analytic and the combinatorial to prove, with the aid of the digital computer, the existence of an equilibrium.

Finally, it may well be apposite to remind L&K another aspect of computer-aided proofs – the candour and care with which those who appeal to the computer, at any particular stage of a proof, make available the codes and the kind of *Ansatz* that may have forced them to seek the aid of the computer, so that any interested person could repeat, check or whatever, the procedures adopted in the interface and by the computer. How many analytical proofs are made transparent in this way – particularly in the neowalrasian cloisters? How many years has it taken – and continues to take – to 'prove' Ramanujam's results?

We learn nothing from section 4 of L&K, from any point of view: economics, computer-aided proofs, program verification, digital proofs, discretizations, analytical proof, 'humanly' this or 'humanly' that, computerized proofs, or robustness, all concepts freely, enthusiastically and frivolously thrown around, with no anchoring anything, especially in anything like any kind of theory, experiment or simulation.

5 The Noble Tradition of Simulation in Economics

> "My guess is that *the age of* theorems may be passing and that of *simulation is approaching*. Of course there will always be logical matters to sort out, and our present expertise will not be totally obsolete. But the task we set ourselves after the last war, *to deduce all that was required from a number of axioms*, has almost been completed, and while not worthless has only made a small contribution to our understanding."
>
> [54], p. 258;italics added.

It is gratifying to note one of the high priests of the neowalrasian code[54] heralding the 'age of simulations'. But the claim that it is possible to '*deduce all that was required from a number of axioms*', whether it was the 'task' that the primitives of the neowalrasian cloisters set themselves as explicit aims or not, is impossible to substantiate with any kind of rigour. Above all, it is impossible to do so without also specifying clearly what 'deduce' means; i.e., which deductive methods are allowable, mathematically and from the point of view of whichever kind of mathematical logic is chosen to underpin the deductive method. Moreover, who knows – or can ever know, epistemologically – whether the chosen 'number of axioms' was sensible, meaningful or relevant from any economic vantage point. Surely, a strong case can be made that many of the chosen axioms were done so for purely mathematical – of a particular variety – reasons. In any case, what kind of 'understanding' does Hahn mean- or, rather, the practitioners of the neowalrasian code mean? This latter question, indirectly, is addressed, obviously, to L&K, whose norm of a perfect model of economics is nothing other than the neowalrasian code.

[54]Clower's defining works of the neowalrasian code included, apart from the classics of Arrow-Debreu, the books by Debreu ([26]) and Arrow & Hahn ([3]).

However, in an 'unguarded address', in Clower's classic phrase ([16], p. 821), Hahn also reflected, in his Econometric Society Presidential Address of 1968, ruefully (we hope), [53], pp. 1-2 (italics added):

> "We all know the endless variety of adjustment models, not uncongenial to commonsense, one is capable of constructing. *No unifying principle, such as maximization, seems available*; ..... The achievements of economic theory in the last two decades are both impressive and in many ways beautiful. But it cannot be denied that *there is something scandalous in the spectacle of so many people refining the analyses of economic states which they give no reason to suppose will ever, or have ever, come about. It probably is also dangerous.*"

Here is the crux of the matter as to why simulation is actually shunned by the neowalrasian cloisters: simulation models, unless they are underpinned by equilibrium models that are, in turn, built on *maximization principles* – whether in competitive markets or not is besides the point; whether in game theoretic situations or not is irrelevant – can only be utilized to compute equilibria in the precise sense in which it is used in RBC models (cf. [18], especially §4 & §5), and at least in this L&K are right, but for the wrong reasons. But what if one can *prove* – *analytically*, no less – that only dynamical systems *incapable* of being consistent with any maximum principle can be underpinned by a rigorous computable formulation of bounded rationality and satisficing in the strict sense in which Simon defined them? What if, moreover, there *is* a unifying principle, underpinned by the conjunction between computability theory and dynamical systems theory that can, in turn, generate dynamical systems incapable of being made consistent by any maximization principle[55]? Indeed, there does exist such a unifying principle, far more general, for dynamical systems, yet rigorously underpinned by computability theory: *computation universality*. In other words, dynamical systems capable of computation universality are incapable of being made consistent with any maximization principle, yet they are consistent with Simonian bounded rationality and satisficing. These systems, in general, can *only* be explored, investigated and analyzed for their dynamic behaviour, in anything beyond the ultra-short-term, by simulation models that are computably based. It is not possible to use the kind of mathematics to which the neowalrasian code adheres, separating and employing one kind for their analytical part and another for the simulation part. Thus, there cannot be and there will be no need for the dichotomy between an analytical part of a proof of a property of a system and a computer-aided part. Some of the relevant results can be found in our very recent research results, although built upon a series of investigations going back to the founding of the field of *Computable Economics*, about twenty years ago (cf. [161] and [173]).

This is the reason why in our discussion of the noble tradition of simulation in economics we concentrate exclusively on varieties of disequilibrium approaches

[55] See, above, for the impeccable source for this insight in economics, as usual, made first by Paul Samuelson, in his *Nobel Memorial Prize Lecture* ([117], pp. 12-13).

to economic dynamics, the latter, however, interpreted in a very particular sense: **the concentration on understanding the properties of a path towards an equilibrium, whether such an end state is definable or reachable or not**. In the next three subsections we summarize the noble tradition of simulation in economics via the epistemological and methodological roles played by simulation and computing in: *indeterminate nonlinear dynamics*, *coupled linear dynamics*, *evolutionary dynamics* and *behavioural dynamics*.

Before we end the 'prologue' to this section it is necessary to mention yet another red herring thrown out by L&K. They claim, ostensibly correctly, that:

> "The dearth of simulations models is most conspicuous in the most widely respected journal that publish papers on *economic theory*."
>
> L&K, p. 305; italics added.

Why is this surprising? After all, it is precisely in *economic theory* – where the '*perfect model*' rules in the form of the *neowalrasian code* – that there is no need for simulation, as argued by L&K, themselves! This kind of pointless pointing out comes about because, we surmise, L&K do not understand exactly what a simulation is – theoretically, above all, but also epistemologically, although their paper is liberal in invoking epistemics all over the place – and, even worse, what exactly is the meaning of economic theory and how a variety of closures determine exactly what foundations for which economics dominates the vision of orthodoxy in any one epoch. It is not as if the current orthodoxy – particularly in macroeconomics – has been the ruling paradigm, even of the recent past.

They continue the above sentence as follows (italics in the original):

> "A quick search for papers with 'simulation' in the title yielded a total of 47 hits in JSTOR and 112 hits in the Web of Knowledge for the five journals commonly considered the most prestigious: *American Economic Review*, *Journal of Political Economy*, *Econometrica*, *Quarterly Journal of Economics*, and *Review of Economic Studies*."

'Commonly considered most prestigious' by economic theorists, experimental economists and applied economists, which would appear to exhaust the category of economists employed at the leading Universities, some of the better known International Institutions – for example the World Bank, IMF, OECD, the various UN affiliates – and even Central Banks and the treasury-linked Ministries of Finance, etc. But this is a jaundiced vision for a very simple reason: many faculties of economics contain departments of management[56], business and operations research, in short the *decision sciences*, broadly interpreted. Conversely,

[56] We are both members of the Department of Economics of the Faculty of Economics at the University of Trento. The Faculty, itself, is made up of our department and the department of management and operations research. In fact, we were 'inspired' to embark on this essay because a colleague in the sister department asked us whether we were aware of L&K, knowing our own adherence to a belief in, and respect for, the role of simulation intrinsic to computable economics. Unfortunately, this colleague – to the best of our knowledge – had no better command of economics, mathematics or epistemology than that which has been demonstrated in L&K.

many distinguished economists belong to Schools, Faculties and Departments of Business. Just off the cuff, the Stern School of Business at NYU, the Anderson Graduate School of Management at UCLA, the Graduate School of Business at the University of Chicago, the London Business School, the Judge Business School at Cambridge University,and the Haas School of Business at UC Berkeley, come to mind - not to mention the great tradition of decision sciences nurtured and fostered by what is now the Carnegie Mellon University, Herbert Simon's home base for most of his academic life. For members of these kinds of schools, faculties and departments, it is not clear the above five journals would be 'considered' unambiguously 'the most prestigious'. In particular, journals in management, business, operations research and statistics would be equally - or, most likely, more – prestigious, in terms of promotion and tenure for young academics. Again an off the cuff check on "hits in JSTOR" for "papers with 'simulation' in the title" of Journals that would count as prestigious for economists with a business, finance, OR and related decision science orientation, gave us between 800 and 840 hits, depending on the permutations and combinations of the Journals chosen[57]. In this particular context, given that we have included the *Proceedings of the National Academy of Sciences of the USA* in our 'hits list search', it may be useful and apposite to point out to L&K that three of Debreu's fundamental papers that led to the famous Arrow-Debreu classic and, then, to Debreu's own defining book, **Theory of Value**, [26], were published in this journal; i.e., the defining framework and foundations for the neowalrasian code came to see their first public light of day in the *Proceedings of the National Academy of Sciences of the USA*. A similar story could be told for the defining tools of the dominant school of macroeconomics – the Newclassicals – that were fashioned by Bellman, Kalman and Wald[58]. Ditto for the classics of mathematical programming[59].

If a lesson is to be learned from this kind of past, it is that the 'commonly

[57]We checked the following: The Journal of the Operations Research Society, Operations Research, The Journal of Business, Proceedings of the National Academy of Sciences of the US, Science, Management Science, mathematics of Operations Research, SIAM Journal on Applied Mathematics, SIAM Review, SIAM Journal on Numerical Analysis, Journal of Business & Economic Statistics, Journal of the Royal Statistical Society, Series A & Series B.

[58]Newclassical macroeconomics is often referred to – especially by its core adherents and practitioners – as *Recursive* Macroeconomics ([77]). The reason for it being 'recursive' is, ostensibly, due to the mathematical tools underpinning its formalization: *Dynamic Programming* (Bellman), *Kalman Filtering* (Kalman) and *Markov Decision Processes* (Wald). Naturally, as practitioners of *Computable* Macroeconomics we find the use of 'recursive' by the Newclassicals somewhat unfortunate. The foundations of computability theory, from its inception in the late 1930s, lies in *recursion theory* in its mathematically rigorous sense.

[59]Indeed, this is particularly true of some of the classics by Richard Bellman on dynamic programming. For example what is now routinely used and taught by economists (mostly, but not exclusively of neowalrasian code persuasions) as the '*principle of optimality*', had one of its incarnations, as the '*principle of invariant imbedding*' in the *Proceedings of the National Academy of Sciences of the USA* in 1956. In passing, it should also be mentioned that many classics of what defined the neowalrasian code were also the product of a fertile period of activity at RAND, and one of the classics was precisely on simulation. We have in mind the 1972 RAND monograph by Shubik and Brewer on *Models, Simulation and Games - A Survey* ([122]).

considered most prestigious' Journals, are good at consolidating what has become orthodoxy; but they are, almost by definition and necessity, oblivious to challenges to orthodoxy, in the nascent period of the challenges.

Instead of prowling around title 'hits' in so-called 'prestigious' Journals, they should have studied the way the subject of economics is pushing its frontiers with new tools and concepts – some of which are being used by agent-based modellers, again first emerging at the hands, from the brains, of Turing, von Neumann, Ulam and Conway – in obvious synergies between computation, simulation and dynamics.

A series of articles, from the last decade of the previous century, through to the mid-point of the present decade, have been celebrating, and reflecting on, various anniversaries pertaining to OR and Simulation (a representative sample would include, for example, [110], [91], [137], and [99]). There are many references to citation indexes and the rich vein of information that can be garnered from them, in a primitive inductive mode, about the way simulation modelling has determined the evolution of many of the decision sciences that underpin modern economic theory.

To claim that simulation has been shunned by economists only because it has been ignored by precisely *the kind of economic theory that has no need for it* is a *non sequitur* of the highest form of absurdity, in a paper replete with many vintage absurdities.

In any case, this story, that economists never shunned simulation, can be told in many different ways, even emphasizing and invoking traditions that we have chosen to neglect. However, we have endeavoured very explicitly to avoid any reliance on the *Whig approach* to the telling of this story.

5.1 Economist's *Never* 'Shunned Simulation'

> "Lundberg's basic contribution to the field [of macroeconomic dynamics] does not make any use of the now commonplace mathematical tools that sometimes offer general solutions to dynamic relationships; instead, it relies on *numerical time sequences* generated with the aid of particular illustrative values of the parameters of his dynamic equations. ... Indeed, it will be pointed out that the ostensible superiority of the formal approaches in terms of the greater generality of their results is to a considerable degree an illusion, because the mathematical techniques are fully effective in providing *analytic solutions* for dynamic models only when those models take the most elementary (linear) forms. ... There are even significant recent contributions[60] that have reverted directly to *the Lundbergian*

[60] By these Baumol refers (ibid, p. 193) to the *evolutionary growth theoretic tradition* pioneered by Nelson and Winter (op.cit), built on Schumpeterian foundations, and the problem of *traverse in the Neo-Austrian tradition*, revived by John Hicks and imaginatively cast into a simulation model by Mario Amendola and Jean-Luc Gaffard ([2]). In both cases the emphasis is on the path between equilibria, not the equilibria themselves. See §5.2, below.

techniques, thereby demonstrating their power to provide profound *insights into complex and important issues.*"

[6], 1991, pp. 185-6; italics added.

This is just for starters, on the methodology advocated, and practised, by a distinguished group of macroeconomists whose work was definitive in defining the subject [158].

One can easily make a fairly good case that some of our classical and neoclassical founding fathers were equally adept at relying on simulation to obtain educated guesses for evolving patterns in both aggregative and disaggregated economic variables, within equilibrium and non-equilibrium models. Malthus, Jevons, Marshall, the Austrian capital theorists, even Irving Fisher, are names that come to mind. Jevons, could be considered the successor of Babbage and the precursor of Turing, in building and putting to use a primitive digital computing machine to implement deductive processes and investigate empirical hypotheses – even of inferring, despite his penetrating critiques of Mill's reliance on induction (see chapter 15 in [160]). Fisher is, arguably, the first eminent economist to build and use an analogue computing machine to investigate and infer values for microeconomic parameters, by a structured simulation model and method (see [11]), to determine *equilibrium* prices and quantities. In Fisher's case, even though he could considered the precursor of Phillips, in that he constructed a special purpose hydraulic analogue computing machine for studying, in a simulated environment, a theoretically informed model, he should also be considered a precursor of the CGE school. In that sense, simulation was not for the epistemological reason of learning about the analytical model. As Borges reminded his readers: '*The fact is that every writer creates his own precursors.*'

It is not clear, however, that these distinguished precursors, and their Borgesian precursors, could be considered the founding fathers of simulation modelling in the epistemological and methodological sense that we have suggested above, at the end of the previous section. For that we have really to wait for the various 'revolutions' – macro, micro, interindustrial economics and game theory – of the 1930s. Above all, in macroeconomics, at the hands of the pioneering Swedes and Frisch; in interindustrial economics – ironically as 'an empirical application of equilibrium analysis' – and in varieties of competitive and non-competitive microeconomics, and also in dynamic game theory. But in the latter three cases – microeconomics, interindustrial economics and game theory – simulation played the role of the handmaiden to the analytical solution, as a method and means to obtain conjectured, analytically predicted, solutions. This is exemplified by the classic works by Leontief ([74]), Chamberlin ([15]), Brown ([12]) and even Flood ([35]). They may be – and, in some circles, are – considered the Borgesian precursors of today's experimental economics. But in macroeconomics there was, from the beginning, a clear difference between simulation and its epistemologies as a source for the development of macrodynamic theory and macroeconometrics, considered the experimental wing of the subject, above all in the works of Ragnar Frisch[61] (see [38], [39] & [40]). This is, of course, clas-

[61] It is possible to include within Frisch's contribution to simulation modelling, in the sense

sically the case with Frisch's Cassel Festschrift contribution, sadly – and quite undeservedly and incorrectly – considered the fountainhead for the methodology of the current dominant orthodoxy in macroeconomics. In this case one of us – Zambelli, [172] – has demonstrated, replicating exactly, the Frisch simulation exercise, that the claims of orthodoxy are untenable. But that does not mean the this classic can, in many senses, be considered the pioneering quantitative contribution to simulation modelling in the sense in which we mean: studies of disequilibrium paths by simulation exercises to learn and construct an appropriate mathematical model of the macroeconomy, rather than the reverse process of simply computing and confirming an analytical equilibrium and the formal model for which it is a solution.

But we had to wait for the Stockholm School macroeconomists to initiate the epistemological role for simulation in the construction of macroeconomic *theory* as a study of the disequilibrium paths of aggregative variables in their dynamics towards what may or may not be an equilibrium – even underpinned by rational expectations and intertemporal accounting discipline.

Knut Wicksell's 'second generation' pioneer followers, and the founding fathers of the Stockholm School approach to macroeconomic dynamics, primarily Erik Lindahl, Gunnar Myrdal, Dag Hammarskjöld and Erik Lundberg, refused to 'simplify' their dynamic formulations in the interests of analytical solutions. Instead, they opted to study copious *numerical sequences*[62], based on 'realistic' parameter value ranges, which were, in their turn, assumed to be time-varying, in general. Theirs was a disequilibrium dynamic approach to monetary macroeconomics[63], with impeccable economic theoretical foundations, but investigated – from an epistemological point of view - for their dynamic properties by *primitive simulation methods.* Their refusal to adopt simplifying assumptions in the interest of obtaining unambiguous analytical solutions was one of the main causes of their ultimate demise from frontier macrodynamic research. Others, more in tune with the *zeitgeist*, had no qualms about making the necessary simplifying assumptions to obtain ostensibly general analytical solutions. This hastened the demise also of their methodology for studying, from an epistemic

we are using the term, also his work on determining the parameters of political preference functions ([41]). Although one of us has worked fairly intensively in this area (see [113]), we are not sure that it satisfies the unwritten rules of epistemology in the kind of simulation modelling we have in mind. In other words, it was not quite a simulation study of a disequilibrium path.

[62] The almost final formal outline of this approach was summarized in chapter IX of Lundberg's classic ([79]) and titled: *The Construction of Model Sequences.*

[63] Contrary to many popular statements and claims that Lindahl was wedded to a temporary equilibrium approach, by the time he – Lindahl – came to present his mature vision of economic dynamics as a *General Process Analysis*, he had dropped the earlier adherence to equilibrium dynamics. That many commentators of Lindahl persisted in attributing to him an adherence to equilibrium dynamics was due to an unfortunate historical accident. Myrdal's trenchant criticism of Lindahl's method of temporary equilibrium, in the Swedish version of *Monetary Equilibrium* ([90]) was excised in the English version of the book. This critique was fully accepted by Lindhahl and he, then, completely revised his equilibrium methodology. The immediate outcomes of these interactions were clearly evident first in Hammarskjöld's doctoral dissertation of 1933 and, finally, in the above mentioned work by Lundberg and the first part (written last) of Lindahl's 1939 monograph (see, respectively, [89], particularly, f.n., 4, p. 205 & chapter III, § 22, [56], [79] and [76]).

point of view, model sequences simulationally[64]. Had the computing capabilities that are routinely available now been at their disposal, the pioneers of the Stockholm School approach to macrodyamics could have stopped the slide into the neoclassical synthesis and, thereby, heralded the advent of the DSGE model and slide, of inherently indeterminate macroeconomics, into the neowalrasian cloisters. This is a counterfactual claim. Whatever that may be, the fact is that the rich simulation-strengthened macrodynamics of the Stockholm School disappeared at the dawn of the computer era! Therefore, we will summarize our story of the noble tradition of simulation in macroeconomics from the way digital and analogue computing methods were harnessed to study the complex indeterminacies of the nonlinear disequilibrium dynamics of Keynesian aggregative economics and the equally complex and equally indeterminate linear disequilibrium dynamics of *coupled* economies, the latter leads to the quasi-periodic paradoxes in the economic domain, as well.

5.1.1 The Place of Simulation and its Epistemology in the Keynesian Macrodynamic Tradition

> "No doubt there are many Kalecki Effects walking about the world, some of them dressed up in such grand mathematical clothes that they are not likely to come my way. But I want to tell you about my meeting, a year or two ago, with one of them who seemed a fairly plain-spoken and approachable person."
>
> Robertson, [109], p.188

There are *many Keynesian traditions* of disequilibrium macrodynamics 'walking about the world', some even at the frontiers of economic theory, happily coexisting with the rational expectations hypothesis and the shared equilibrium norm of a long-run configurations devised by the magicians and high priests in the neowalrasian cloisters. We concentrate, instead, on variants of the original multiplier-accelerator macrodynamics for a very simple reason: it is one of the few dynamical systems, once widely used in economics, that is incapable of being reconciled with any maximization disciplining rule. This makes it imperative that one studies the properties of its 'transition' path, towards an equilibrium, whether it ever reaches it or not, whether the equilibrium can ever be characterized or not. Indeed, even whether there is an equilibrium towards which it is a transition path. One just studies the dynamics of a path – full stop.

The example we have chosen here encapsulates a noble tradition of *simulation* in every sense of this concept: as a numerical method to study a precisely specified mathematical system to be studied on a digital computer; as a substitute for an analytical study (because such a study is provably 'unlikely' to succeed in any meaningful way); as an epistemological tool to interpret the results (most of which were unexpected); and, above all, to gain insight into the link between a computing machine and *its* theory and the theory of nonlinear

[64] The classic example of such a transmogrification was the linearization of Lundberg's piecewise linear – i.e., nonlinear – model of inventory fluctuations by Lloyd Metzler (cf. [153]).

dynamical systems. The latter point is turning out to be the most significant from the point of view of the epistemology of simulation, since the interaction can only be explored by representing the one system by the other.

Consider the following equation, representing a classical Keynesian nonlinear multiplier-accelerator model:

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

Where:
$y(t)$: national income in real terms;
ϵ : a constant with the dimension of years;
α : a dimensionless constant;
t : time in years;
θ : a time lag in years;
ϕ : the flexible accelerator (either a smooth, elongated, s-shaped or a piecewise linear, elongated, z-shaped, curve);
$l(t)$: autonomous investment;
$\beta(t)$: autonomous component of consumption expenditure;

Now, there are *at least* four different ways solutions to this equation has been investigated:

- Graphically, i.e., in terms of the geometry of dynamic behaviour, as usually done in the qualitative theory of differential equations (see[48]);
- By the method of equivalent linearization (see [10]);
- Using an electro-analog[65] computer (see [132]);
- Using digital computers (see [173]);

Assuming, for example, $\beta(t) + l(t)$ a constant 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:

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

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:

$$\ddot{y} + \left[\chi\left(\frac{\dot{x}}{\dot{x}}\right)\right]\dot{x} + x = 0 \tag{3}$$

[65] In parallel, but slightly earlier, work of a related nature, [131], 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."

With this approximated reformulation Goodwin, geometrically, and Yasui ([167]) analytically, began an 'industry' in the endogenous theory of the business cycle! For the past six decades we have had variations on themes based on (3), extending, generalizing, proving and simulating (digitally, of course), this homogeneous equation in all sorts of ways. In all this activity all and sundry seem to have forgotten the basic final form (1), of which (3) is a very special approximation.

On the other hand, using an electro-analog computer, it was found, in [132], that the approximation of (1) retaining the first four terms of a Taylor series expansion, 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. A similar result was obtained in [163], this time using non-standard analysis to study the dynamics of (3) – i.e., loss of independence of initial conditions in the generated cycle(s) and the existence of many cycles.

The next 'assault' on this obdurate equation was via coupled analog computers, where the coupling was between two economies. The coupling was of two equations of type (3), via the Phillips Electro-Mechanical-Hydraulic Analogue Computing Machine ([51]), which was shown to generate the quasi-periodic paradox (cf., [1], above). Neither Goodwin, nor Phillips, who did the coupled-dynamics simulation on the Phillips Machine had any clue – theoretical or otherwise – about interpreting and encapsulating this outcome in any formalization. The key point is that they were surprised by the outcome, which they did not expect, and did not know how to interpret it when it emerged – exactly as in the original FPU experiment. The analogies (sic!) are uncannily similar – especially from the point of view of the epistemology of simulation. There was no macrodynamic theory to which they could relate the observed behaviour, which was contrary to expected behaviour. In the case of the FPU paradox, serendipitously, Kolmogorov's conjecture of what was to become *KAM theory* was announced that same year – the *annus mirabillis*, as we may now call it, since the Goodwin-Phillips simulation exercise was also conducted that year – 1954[66].

Finally, one of us – Zambelli ([173] – repeated the simulation in [132], but this time on a digital computer. Our aim was also to test the conjectures in [45], regarding quasi-periodicity in coupled markets, but doing the simulation for nonlinearly coupled economies, varying coupling strengths systematically. Our results came as much of a surprise to us as every kind of FPU experiments have been to their modellers: although we can confirm the results in [132], the outcomes are richer and more varied and we would have no idea which way to proceed, if we are wedded to an equilibrium norm to which the results have to

[66] As a matter of fact, it was also the year Simon finalised the paper that is now considered the fountainhead of classical behavioural economics (replete with simulation results), [123].

conform. Instead, for the moment, we accept George Temple's wisdom as our practical precept ([139], p.233):

> "The closely guarded secret of [the study of differential equations] is that it has not yet attained the status and dignity of a science, but still enjoys the freedom and freshness of such a pre-scientific study as natural history compared with botany. The student of differential equations – significantly he has no name or title to rank with the geometer or analyst – is still living at the stage where his main tasks are to collect specimens, to describe them with loving care, and to cultivate them for study under laboratory conditions. The work of classification and systematization has hardly begun."

This is exactly what we are doing, 'collecting specimens, describing them with loving care and cultivating them for study under laboratory conditions', where to 'study under laboratory conditions' means investigations by means of simulations.

More than a lifetime ago, the prescient pioneer of the above Keynesian nonlinear dynamics, suggested that the study of coupled economic systems would require '*the prolonged services of yet unborn calculating machines.*' ([45], p. 204). We think Richard Goodwin, who for the geometric *analysis* of equation (3) utilized '*paper and pencil simulation*' ([25], p. 16; italics added), had the intuition of a geometer to realise that coupled nonlinear dynamical systems would prove to be impervious to formal analysis. He was the undisputed pioneer, also, of 'collecting specimens, to describe them with loving care, and to cultivate them for study under laboratory conditions.''We are simply standing on his shoulders, and following in his footsteps. With his 'paper and pencil simulations' he did discover dynamical systems, inspired by economic intuitions, that the theorist of nonlinear differential equations had not envisaged (see [47], and [156] for a brief history of the discovery by means of 'paper and pencil simulation' by Goodwin).

This is also the right place to point out that simulations are implementable in very many different ways, not just by means of classical digital and analogue (or hybrid) computing machines.

In summary, the lesson we have learned from this tradition and our own attempts at enriching it, is exactly similar to the one reported by Weissert's text on **The Genesis of Simulation in Dynamics: Pursuing the Fermi-Pasta-Ulam Problem**, [165], particularly chapter 5: *Steps to an Epistemology of Simulation*. For sixty-three years, macroeconomists outside the neowalrasian cloisters have been investigating coupled dynamics in nonlinear systems, to the best of their ability, using analogue and digital machines and discovering, via simulations, dynamic behaviour that cannot be encapsulated by any economic theory underpinned by any kind of maximization framework. Every experience and every exercise in the traditions we have noted above is a challenge to the absurd characterization by L&K: (p. 324)

> "Simulations do not advance economic understanding since they cannot correspond to argumentation patterns (perfect models) that con-

stitute understanding."

Nothing done outside the neowalrasian cloisters – i.e., perfect models – can 'correspond to understanding', despite evidence to the contrary. No wonder they – L&K – think we, i.e, economists, have 'shunned simulations'!

5.1.2 Varieties of Growth Theoretic Traditions

> "Let me conclude ... with a brief allusion to two recent developments in economic dynamics that can be taken to have some of their roots in Lundbergian analysis. The first of these .... is the evolutionary economics associated with the work of Nelson and Winter ([93]). ... In their valuable work, Nelson and Winter have proposed that a fruitful alternative [to equilibrium analysis] can be found in dynamic models, much of whose interest lies in their ability to provide *rigorous description of process of transition* toward *an equilibrium target that may in fact never actually be attained*. If that target continues to shift, t*he nature of the transition path may encompass most of what is really of interest in the formal analysis*."
>
> [6], p. 193; italics added.

The second of the 'two recent developments in economic dynamics' to which Baumol refers to in the above quote, is the Neo-Austrian analysis of *traverse dynamics* via simulation models as in, for example, [2]. But there is a third growth theoretic tradition, emerging from endogenous growth theory, grounded squarely in recursion theory and analysable only by means of simulation! We have called this the *Computable Growth Theoretic Tradition* (see [170] & [171]). In each of these exercises in growth, the focus is on the path of growth, and not on its (conceivable) end state. The approaches, and the stimulating and inevitable epistemological role of simulation in their implementations, are adequately – indeed in very great detail – presented in the four references given above. Therefore, we shall not try to summarise or highlight any aspect of evolutionary growth theory, traverse analysis or computable growth theory.

However, there are two issues that need to be pointed out: the first relates to a characteristically uninformed point made by L&K, in the context of evolutionary models and their place in simulation in the sense in which *we* mean it (p. 312):

> "The recent popularity of evolutionary game theory shows that economists do not shun simulation simply because it provides a way of studying less-than-fully-rational decision-making rules."

But they fail to point out that this kind of evolutionary theory is as wedded to equilibrium methodology as anything else in the neowalrasian cloister. Moreover, if they want to give a proper example of an evolutionary theory, fully independent of any and every rule of the neowalrasian code, they needed only to refer to evolutionary growth theory, where, moreover, there is an explicit epistemological role for simulation. There is no evidence whatsoever that L&K are even remotely aware of the existence of a flourishing field of evolutionary growth theory – and has existed for over forty years!

The second point is more formal. Recall that our concern is the tradition of simulation in economics whereby the focus is on the dynamics of a path, independent of underpinning in orthodox rationality and equilibrium states. In other words we are focusing on those traditions in economics that study, by simulation models, the path and its properties, rather than states of equilibria or the foundational rationality hypotheses. Now, consider the following definition and the ensuing proposition and theorem:

Definition 1 ***Dynamical Systems capable of Computation Universality:***

A dynamical system capable of computation universality is one whose defining initial conditions can be used to program and simulate the actions of any arbitrary Turing Machine, in particular that of a Universal Turing Machine.

Proposition 2 *Dynamical systems characterizable in terms of limit points, limit cycles or 'chaotic' attractors, called 'elementary attractors', are not capable of universal computation.*

Proof. *Essentially because the basin of attraction of such dynamical systems are recursive.* ■

Theorem 3 *There is no effective procedure to decide whether a given observable trajectory is in the basin of attraction of a dynamical system capable of computation universality*

Proof. The first step in the proof is to show that the basin of attraction *of a dynamical system capable of universal computation is recursively enumerable but not recursive. The second step, then, is to apply Rice's theorem to* the problem of membership decidability in such a set.

First of all, note that the basin of attraction of a dynamical system capable of universal computation is recursively enumerable. This is so since trajectories belonging to such a dynamical system can be effectively listed simply by trying out, systematically, sets of appropriate initial conditions.

On the other hand, such a basin of attraction is not recursive. For, suppose a basin of attraction of a dynamical system capable of universal computation is recursive. Then, given arbitrary initial conditions, the Turing Machine corresponding to the dynamical system capable of universal computation would be able to answer whether (or not) it will halt at the particular configuration characterizing the relevant observed trajectory. This contradicts the unsolvability of the Halting problem for Turing Machines.

Therefore, by Rice's theorem, *there is no effective procedure to decided whether any given arbitrary observed trajectory is in the basin of attraction of such recursively enumerable but not recursive basin of attraction.* Only dynamical systems whose basins of attraction are poised on the boundaries of elementary attractors are capable ■

Claim 4 ***Only dynamical systems capable of computation universality can generate behaviour that cannot be encapsulated in, or rationalised by, any notion of maximization.***

We believe we can prove this claim, but only by recourse to non-constructive means. However, this claim shows exactly what is going on in an evolutionary growth path, a traverse and in a computable growth path.

Next, consider the definition of the Kolmogorov complexity of a finite object (Kolmogorov, 1968[[71]], p. 465) :

$$K_{\phi}\left(y|x\right) = \{ \begin{array}{c} \min_{\phi(p,x)=y} l\left(p\right) \\ \infty \nexists\, p \text{ s.t } \phi\left(p,x\right) = y \end{array} \tag{4}$$

Where: $\phi\left(p,x\right) = y$: a partial recursive function or, equivalently (by the Church-Turing Thesis), a Turing Machine – the 'method of programming' – associating a (finite) object y with a *program* p and a (finite) object x; the minimum is taken over all programs capable of generating y, on input x, to the partial recursive function,p. Consider the above (minimal) universal dynamical system as canonical for any question about membership in attracting sets, A. What is the complexity of $K_U\left(p|x\right)$? By definition it should be:

$$K_{\mathbb{U}}\left(y|x\right) = \{ \begin{array}{c} \min_{\mathbb{U}(p,x)=y} l\left(p\right) \\ \infty \nexists\, p \text{ s.t } \phi\left(p,x\right) = y \end{array}$$

The meaning, of course, is: the minimum over all programs, p, implemented on $\mathbb{U}$, with the given initial condition, x, which will stop at the halting configuration, y.

Unfortunately, however, $K_{\phi}\left(y|x\right)$ is a *non-recursive real number*!

How can we decide, algorithmically, whether any observed trajectory is generated by the dynamics of a system capable of computation universality?

Consider the observable set of the dynamical system, $y \in A$; given the UTM, say $\mathbb{U}$, corresponding to $\wp$.

The question is: for what set of initial conditions, say x, is y the halting state of $\mathbb{U}$. Naturally, by the theorem of the *unsolvability of the Halting problem*, this is an *undecidable* question.

Remark 5 *Why is it important to show the existence of the minimal program? Because, if the observed y corresponds to the minimal program of the dynamical system, i.e., of* $\mathbb{U}$*, then it is capable of computation universality; if there is no minimal program, the dynamical system is not interesting! A monotone decreasing set of programs that can be shown to converge to the minimal program is analogous to a series of increasingly complex finite automata converging to a TM. What we have to show is that there are programs converging to the minimal program from above and below, to the border between two basins of attractions.*

Shortly after Kolmogorov's above paper was published, Zvonkin and Levin, [**?**], p.92, Theorem 1.5, b, provided the result and proof that rationalises the basic principle of the *computable approximation* to the uncomputable $K_{\phi}\left(y|x\right)$.The significant relevant result is:

Theorem 6 *Zvonkin-Levin*

$\exists$ a general recursive function $H(t,x)$, monotonically decreasing in t, s.t :

$$\lim_{t\to\infty} H(t,x) = K_\phi(y|x) \tag{5}$$

Remark 7 *This result guarantees, the existence of 'arbitrarily good upper estimates' for $K_\phi(y|x)$, even although $K_\phi(y|x)$ is uncomputable*[67].

But formally, at least, we can obviate the above result on the algorithmic impossibility of *inferring*, *from observable trajectories*, whether they have been generated by a dynamical system capable of computation universality. Thus, we can try to approximate to the undecidable by a monotone computable process; i.e., we can approximate to the dynamical system capable of computation universality by a sequence of observation on simpler dynamical systems. Unfortunately, however, the melancholy fact noted in the last footnote may haunt the empiricist forever!

The point of these formalisms is the following: the exercises in evolutionary growth theory, traverse analysis and computable growth theory are about dynamical systems that are capable of computation universality. Such systems possess strong and intuitive undecidability properties. Therefore, the scope of analytical results are limited, at least with respect to traditional steady state desiderata. Only simulation studies can give hints on possibilities for extracting rules from observational behaviour – i.e., by traditional inductive methods. However, they cannot be definitive – as claimed by the plethora of agent-based simulations, without a basis in computability theory.

5.2 Classical Behavioural Economics[68]

> "The theory proclaims man to be an information processing system, at least when he is solving problems. ......
>
> An information processing theory is dynamic. ..

[67] Our view on this is further strengthened by some of the remarks in [20], particularly, p.163, where one reads (italics added):

> "The shortest program is not computable, although as more and more programs are shown to produce the string, the estimates from above of the Kolmogorov complexity *converge to the true Kolmogorov complexity*, (the problem, of course, is that *one may have found the shortest program and never know that no shorter program exists*).

These remarks border on the metaphysical! How can one *algorithmically* approximate to a true value that which cannot be known *algorithmically* – by definition?

[68] We have distinguished between classical and modern behavioural economics for many years on the basis of a simple criterion: all of the behavioural assumptions made by Simon and his closely associated pioneers of behavioural economics at its founding, were grounded in a computational context. In the case of Simon, even more specifically on computable grounds. None of the behavioural assumptions of the modern variety, sometimes said to originate in [142], are so grounded. The pioneers of classical behavioural economics are, in addition to Herbert Simon, James March, Richard Nelson, Richard Day and Sidney Winter.

> The natural formalism of the theory is the program, which plays a role directly analogous to systems of differential equations in theories with continuous state spaces ... .
>
> All dynamic theories pose problems of similar sorts for the theorist. Fundamentally, he wants to infer the behavior of the system over long periods of time, given only the differential laws of motion. Several strategies of analysis are used, in the scientific work on dynamic theory. The most basic is taking a completely specific initial state and tracing out the time course of the system by applying iteratively the given laws that say what happens in the next instant of time. *This is often*, but not always, *called simulation,* and is one of the chief uses of computers throughout engineering and science. *It is also the mainstay of the present work.*"
>
> Newell & Simon, [94], pp. 9-12; italics added.

We think there is common agreement in the economics profession that the foundational, founding, works of behavioural economics are *A Behavioral Model of Rational Choice*, [123], and its immediately succeeding companion piece, [124], by Herbert Simon. The terminology, its computational underpinnings, its concern with computational complexity, its setting in the framework of decision problems – in contrast to the orthodox setting in a maximizing framework – and, eventually, the incorporation of the satisficing metaphor, were all seeded in that classic QJE piece. That Simon was working with underpinnings in computability is easy to substantiate, if only one would take the time, and make the effort, to read carefully his writings from about 1954 till right to the end of his life. It can be inferred quite clearly and easily in two famous *homages* to Alan Turing, one in *Models of Man*, [125], setting the scene for his monumental work with Newell ([94]) on **Human Problem Solving**[69], and the other in one of his later important works, [126]. The first, in [125], was:

> "But all of these efforts [cybernetics and robot building] were rather separate from simulation on the computer, which tended not toward activating mechanical beasts but toward programming game playing and other symbolic activities. ... And Alan Turing ([147]), in a justly famous discussion, 'Computing Machinery and Intelligence,' had put the problem of *simulation* in a highly sophisticated form .... ."

[69] In very probably his last published paper before his tragic death in June, 1954, Turing tackled, in a truly brilliant essay, the issue of Solvable and Unsolvable Problems, [148]. This extraordinary essay, presaging the monumental work by Newell and Simon on Human Problem Solving, was published in February, 1954 – yet another of the serendipitous events of that momentous year, 1954: first there was FPU, then, KAM, third, Bounded Rationality; fourth, the coupled oscillator experiment on the Phillips Machine by Goodwin and Phillips, finally, this classic by Turing. In our forthcoming compendium on Computable Economics, [164], pride of place is given to the computable underpinning of (human) problem solving by the inclusion, in lead places, the above classic by Turing and [128]. Alas, all these serendipities are blotted by the one undeniable tragedy of that year: the death, under tragic circumstances, of Alan Turing.

In the context of bounded rationality, satisficing, and their underpinnings for the architecture of human thinking, it was the path broached by Turing that guided Simon's path-breaking contributions. In a volume celebrating '*The Legacy of Turing*' ([126], p.81 & p.101), Simon's essay, *Machine as Mind*, began and ended as follows:

> "The title of my talk is broad enough to cover nearly anything that might be relevant to a collection memorializing A.M. Turing. ... If we hurry, we can catch up to Turing on the path he pointed out to us so many years ago."

In every field to which he contributed, economics, psychology, computer science, philosophy of science and management science, *simulation by digital machines* played a fundamental *epistemological role* – in discovering laws, in implementing retroduction, in making induction scientifically respectable. There is no need for us to list the separate special areas of research, within the five disciplines just listed, in which he used, imaginatively and systematically, *simulation as an epistemological tool*. All of this is amply documented in the primary and secondary literature by and on Simon[70]. Even on the vast and impressive canvas in which he sketched, developed and pioneered varieties of theories of: human decision making, organizations and their evolving structures, evolution, models of discovery, human problem solving, administrative behaviour and causality, all of them investigated with imaginative ways of using simulation by digital computers as *experimental devices*[71] and conceptual tools, it is, in our opinion, in the special contributions to human decision making, human problem solving and devising laws of discovery, that simulation tools made their lasting and most significant impact.

However, given the meaning we have given to simulation, in this work, and following the brief formalization in the previous section, we will end this section with a similar exercise, to substantiate our case for the role of simulation in *adaptive behaviour*, adaptive dynamics and adjustment processes in disequilibria, the fulcrum around which all of classical behavioural economics was developed, all underpinned by boundedly rational agents, searching for satisfying solution in the context of decision problems (in the formal sense).

First of all, consistently with the framework of the previous section, we can state a preliminary theorem:

Theorem 8 *There is no effective procedure to decide, given any subset of an iteration by a Dynamical System Capable of Computation Universality, whether it is an equilibrating process.*

[70] Our own contribution to this literature is detailed in the forthcoming monograph on Simon, by Velupillai ([162]), Preliminary essays, leading up to this monograph, are, [159] [161] and the chapters in Part IV of [160].

[71] Scholars who quote Simon rarely point out that he emphasized that his approach to human problem solving, human behaviour and decision making, in particular, and to economics, in general, was '*empirical, not experimental*'. This, for Simon, simulation was a tool in the domain of an empirical science, to be wielded the way a 'biochemist or archeologist' would, not in the way 'the agricultural experimenter' would use it (see [94], pp. 12-13 and [127]).

This does not necessarily mean that every 'time series', generated by a Turing Machine, if it is to be consistent with the rationality postulates of economic theory, must be interpreted as a disequilibrium configuration. We can allow for a third alternative – neither an equilibrium, nor a disequilibrium, process.

Now, let us assume the following (all of these can be formalized rigorously, as we have done in related writings, in recent and not-so-recent papers and books):

(a) *Adaptive processes* will be assumed to be *dynamical systems* (in the strict technical sense of the term);

(b) *Dynamical systems capable of computational universality can be constructed from Turing Machines* (TM);

(c) *Rational behaviour* by an economic agent is equivalent to *the computational behaviour of a Turing Machine*;

Then, we can state (and prove, if necessary), the following two theorems:

Theorem 9 *Only adaptive processes capable of computation universality are consistent with rationality 'in the sense that economist's use that term'.*

Theorem 10 *There is no effective procedure to decide whether given classes of decision rules are 'steady states of (some) adaptive process'.*

To these we can add, first of all, that no dynamical system used in the neowalrasian cloisters are capable of computation universality. Secondly, also the following theorem:

Theorem 11 *Boundedly rational choice by an information processing agent within the framework of a decision problem is capable of computation universality.*

Proof. See [161] ■

These formal results were derived by us to understand and substantiate Simon's lifelong adherence to simulation in its epistemological senses, and as a tool in methodological modes.

The very idea that economists have shunned simulation can only crop up in the heads of non-economists or those in the neowalrasian cloisters or those who think – and there are surprisingly many – Simon was not an economist. And that about a fellow of the Econometric Society and an economics Nobel Laureate! What will philosophers think up next, to accuse economists of shunning – perhaps computation, especially in its computable mode?

6 Melancholy Reflections, Bright Hopes

> "Every finitely realizable physical system can be perfectly simulated by a universal model computing machine operating by finite means."
>
> *The Turing Principle* - enunciated by David Deutsch ([28], p. 99).

Deutsch enunciated the Turing Principle on the basis of a searching analysis of the meaning of the Church-Turing thesis. He came to the conclusion that underpinning the Church-Turing thesis there was a *physical principle*, which he enunciated as the Turing Principle. Naturally, the Turing Principle, as given above, requires a precise statement of what is to be meant by 'perfectly simulated'. Deutsch, being the serious scientist he is, did not forget to add a definition of 'perfect simulation' (ibid, p. 99; italics added):

Definition 12 *"A computing machine M is capable of perfectly simulating a physical system S, under a given labelling of their inputs and outputs, if there exists a program $\Pi(S)$ for M that renders M computationally equivalent to S under the labelling. In other words, $\Pi(S)$ converts M into a 'black box' functionally indistinguishable from S."*

We wonder whether a deeper knowledge of the frontiers of computability theory – as well as a little of its history – may have prevented L&K from making the many incorrect claims, from being slightly more precise about computation, 'perfect' and 'black box', and much else[72].

Much of our discussion and critique of the claims in L&K has had as a backdrop the precise theory of computation, which is underpinned by the Church-Turing thesis. But we have also consciously adopted – although, we hasten to add, only *pro tempore*[73] – Deutsch's important extensions, all of which seem to be consistent with the stand taken also by Gandy ([42]), on the physical principles that will have to be the basis on which the Church-Turing thesis is interpreted in computability theory. Even though we worked with an informal – but, hopefully, precise – definition of 'simulation' and side-stepped the bland references to 'black-boxes' in L&K, our arguments above have been made with the precise definitions of Deutsch in mind. It is, therefore, just as well, we state

[72] Although it is becoming tiresome to catalogue the series of infelicities in L&K, we are forced to add one further example, in view of the context of this concluding section. On p. 311, L&K 'provide an example of the kind of research that [they] think could be more common in economics: an agent-based simulation of a simple financial market'. They, then, proceed to summarize the structure, assumptions and results of the paper to which they refer. Unfortunately, they trip over the formal, analytic, part right at the beginning, by confusing assumption with results - and worse. Moreover, the paper to which they refer is, essentially, a finite automaton model of computation, which makes the interpretation of the results, given in the original paper and by L&K, formally dubious. But correcting mistakes in references used by L&K is not the one of the purposes of this paper. For, if so, we would have begun with the absurd claim, in the first paragraph of the article by Hughes, on the contents of *The New Physics* ([23]). Hughes states that the book contains only 'one entry on the topics of computers and computer simulation'. Obviously he did not read chapter 6 by Malcolm Langair, which is, in a way, a paean for 'computers and computer simulation' in Astrophysics. Moreover, this chapter is 115 pages long, about a fifth of the book's total!

[73] This is partly because we are *not* completely convinced that the particular physical principle Deutsch derives and states as the Turing Principle encapsulates entirely, for example, *Gandy's Principles for Mechanisms*. It is the latter that we have usually worked with and have refereed to it as *Gandy's Principles for Mechanisms*. Of course, this also requires a precise definition of simulation, but which turns out to be slightly more complicated and lengthy to formulate than the admirably succinct definition derived by Deutsch. A deep and persuasive critique of Deutsch's Turing Principle can be found in [143].

them precisely, at this concluding stage. Interested readers, even – hopefully – inspired ones, may now want to go back and re-read our critique with these precise definitions in mind.

It is our belief, in this time and age, that economist with aims and ambitions to construct models and theorize with mathematical tools, should be exposed to the availability of a variety of mathematics and, correspondingly, different logical bases for them. To be taught mathematical economics as if real analysis and set theory are the be all and end all is absurd, especially when the next step is to use the mathematical models built on such foundations for computation by a digital computer, which is based on wholly different mathematical and logical principles: constructive mathematics and proof theory, on the one hand; or the theory of computation and recursion theory, on the other.

From our own experiences in teaching and interaction with colleagues, we are painfully aware that economists are, in general, blissfully ignorant of any notion of limits to computation, even with ideal machines. But even worse is the equally blissful ignorance on the intrinsic limits to the results obtained with real analysis, underpinned by set theory plus the axiom of choice, let alone the impossibility of adapting such results, from such domains, for computation on machines built on a wholly different mathematics – even with the most rigorous and careful notion of 'approximation' (not the utterly loose and dangerously irrelevant notion used in L&K).

Economists have never shunned simulation. However, they may have misused it, perhaps due to a misunderstanding of the notion, nature and limits of computation, even by an ideal machine. Engineers do not attempt to design perpetual motion machines that violate the laws of thermodynamics or mechanics, although cranks, over the centuries, have claimed to have done so; most of the models emanating from work in economic theory belong to *The Museum of Unworkable Devices*[74] – at least when viewed from the vantage point of constructive mathematics or recursion theory, i.e., from the point of view of computation. How those in the neowalrasian cloisters make their unworkable devices perform the tasks that need to be done, just for survival, is beyond our commonsense comprehension. They must, together with L&K, live in illusions.

Surely, a strong case can be made for making economists, at least at the level of graduate pedagogy, aware of *The Museum of Unworkable Devices* and *The Association for the Study of Failure* (*Shippai Gakkai*)! An imposing catalogue of unworkable devices and their failures can easily be composed, entirely out of the products coming out of the neowalrasian cloisters, even without any mediation from constructive mathematics or recursion theory.

More seriously, what are the main lessons to be learned from the infelicities propagated by L&K? At the ground level, economists should be taught at least the following, in conceptual and mathematical ways:

[74] See the illuminating website dedicated to *The Museum of Unworkable Devices*: **http://www.lhup.edu/~dsimanek/museum/unwork.htm**

The Uzawa Equivalence theorem

The Sonnenschein-Mantel-Debreu theorem

The Ubiquity of Non-Maximum Dynamical Systems

The Church-Turing Thesis/Diagonalization

The Turing Principle/Gandy's Principles for Mechanisms

The Turing Machine/Halting Problem

Computation Universality/Hilbert's Tenth Problem

Kolmogorov Complexity/Algorithmic Randomness

Constructive Lawless Sequences

Decision Problems[75] *vs. Optimization Problems*

Only common sense of a universal variety and the ability to think logically – preferably, but not necessarily, along the natural lines outlined by the Brouwerian Intuitionists - are required to understand, and work with, the above concepts, all of which are elementary in a deep mathematical sense. We have never found any advanced undergraduate or graduate student of reasonable maturity to have had any difficulty whatsoever with understanding the case we make for an economic theory framed in a mathematics that can handle these concepts. Since an economic theory encapsulating the possibility of, say, a computationally universal dynamical system, can only be explored by *actual simulation* – '*to collect specimens, to describe them with loving care, and to cultivate them for study under laboratory conditions*' – of the relevant system, it is natural for such students to realize that there is a wholly different world of economics than the one peddled by the purveyors of the ideas and tools emanating from the neowalrasian cloisters.

No one equipped with the above concepts and their mathematical and epistemological underpinnings would dream of thinking that bounded rationality is some special subset of the economist's notion of rationality – the quintessential 'unworkable device'. No one who understands the ubiquity of non-maximum dynamical systems and understands the notion of computation universality would try to anchor a norm in equilibrium dynamics. No student of economics, equipped with these concepts, even at the level of nodding acquaintance, would feel comfortable in the neowalrasian cloisters, itself located in *Cantor's Paradise*. It may well be apposite to end this long and critical essay, remembering the thoughts of two of the giants of 20th century mathematics and philosophy, David Hilbert and Ludwig Wittgenstein:

[75] In the strict sense in which this is meant in metamathematics, computability and computational complexity theories (cf. [159]).

> Hilbert, [61], (p. 191): 'No one shall drive us out of the paradise which Cantor has created for us.'
>
> Wittgenstein, [166], (p.103): 'I would say, "I wouldn't dream of trying to drive anyone out of this paradise." I would try to do something quite different: I would try to show you that it is not a paradise – so that you'll leave of your own accord. I would say, You're welcome to this; just look about you." '

We are doubtful, however, whether L&K have 'looked about them' with sufficiently critical minds and adequate knowledge of the history, theory and practice of economics.

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