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THE *EPISTEMOLOGY* OF SIMULATION, COMPUTATION AND DYNAMICS IN ECONOMICS[♣]

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[♣] Dedicated to Professor Björn Thalberg, whose own research (Thalberg, 1966), and lectures on macroeconomics, almost forty years ago, were the catalysts that encouraged me on a path of research that has always emphasized the power, necessity and versatility of simulational studies of nonlinear dynamical systems in economics. I am also in debt to the spirit, the practice, the philosophy and methodology of our - i.e., Thalberg's and my - 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. Joe McCauley's works in nonlinear dynamics and financial market dynamics -- in the latter, he was following the tradition of Maury Osborne (1977[1995])) -- have been important inspiring sources for my work and belief in the `synergy', in particular the significance he has increasingly come to attach to Poincaré's `recurrence theorem' (McCauley, 1993, chapter 3 and McCauley, 2009). 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 (Fermi, et. al., 1955 and Ford, 1992) and **The Genesis of Simulation in Dynamics** (Weissert, 1997). This `paradox' should be my 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. Finally, nothing reported in this paper would have been possible without the sterling intellectual support of my ASSRU colleagues and friends, Stefano Zambelli, Selda Kao and Ragu Ragupathy. They are NOT absolved from responsibility for remaining infelicities; all the others are.

Abstract

Computation and *Simulation* have always played a role in economics - whether it be pure economic theory or any variant of applied, especially policy oriented, macro and micro economics or what has increasingly come to be called empirical economics. This is a tradition that can, without too much difficulty, be traced to the spirit and vision of the founding father of Political Economy – as *Political Arithmetic* – by William Petty, whose finest exponent was, in my opinion, Richard Stone, in modern times a noble tradition whose living custodian is Lance Taylor. In this paper their spirit is the driving force, but it is given new theoretical foundations, mainly as a result of developments in the mathematics underpinnings of the tremendous developments in the potentials of computing, especially using digital technology. A running theme in this essay is the recognition – never neglected by Petty, Stone or Taylor – that, increasingly, the development of economic theory seems to go hand in hand with advances in the theory and practice of computing, which is, in turn, a catalyst for the move away from too much reliance on any kind of mathematics for the formalisation of economic entitites that is inconsistent with the mathematical, philosophical and epistemological foundations of the digital computer.

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

"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, [57], 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, [82], p.303; italics added.

"What must be achieved is in fact this: That every paralogism be recognised as an error of calculation, and that every sophism when expressed in this new kind of notation .... be corrected easily by the laws of this philosophical grammar .... . Once this is done, then when a controversy arises, disputation will no more be needed between two philosopers that between two computers. It will suffice that, pen in hand, they sit down .... and say to each other: Let us calculate."

Leibniz, [69], xiv; bold emphasis, added.[1]

[1] My friend Brian Hayes began his respected computing science column in the **American Scientist**, almost thirty years ago, with a reference to this 'Panglossian' optimism by Lebniz ([51]). My own interpretation of Leibnizian confidence in computation was expressed in [116], footnote 8, p.16.

1 Simulation, Dynamics and Computation - Epistemological Deficits

> "Ulam first used 'synergesis' in the context of the '*Computing machine as a heuristic aid' in mathematical research* ... . This was a new mode of working. ....
>
> From 'synergesis,' I formulated the *visiometrics* approach .. . Visiometrics is the process of producing cogent 2D and 3D images and parameter-scaled (normalized) graphs *for developing intuition and aiding in mathematical model formation.*
>
> Norman Zabusky ([128]). p. 10; italics added

The art of gleaning *epistemological lessons*, from *simulating a mathematical model* generating *dynamic behaviour*, on a (digital) computer, was elegantly outlined by Weissert ([124], p. 106; italics added), in the context of the famous Fermi-Pasta-Ulam problem ([39]), which was the setting for Zabusky's path from 'synergesis' to 'visiometrics':

> "The goal of *simulation in dynamics* is to learn about the true solution by simulating the trajectories of the associated differential equations. ... Whether or not the true solution of a model actually relates to physical reality depends upon the fundamental laws used and the *approximations* made to obtain the model. As the simulation teaches us about the true solution, we make decisions about the adequacy of the model. Simulation's function is to reveal the properties of the true solution and to aid our decisions about how well the model suits the physical system. As doubts arise about the model's adequacy, but we are reasonably confident of the simulation, we must turn our attention to the approximations made in the model. Several times .... a researcher indicates an implicit belief that *the simulation does tell us something about the physical reality.* Such an [indication] implies a *trust in a long chain of inferences and approximations* beginning from the adequacy of the fundamental theory itself, to the equivalency of an infinite series expansion of terms, then to the *approximations that are made to obtain a model*, and finally to *the simulation and what it might be telling us about the model.*"

Weissert's concise, yet elegant, encapsulation of a research program on *the epistemology of simulation* ***in*** *dynamics* leaves a few crucial questions un-broached. First of all, even if there is such a thing as **one** 'true solution of a model', the means – i.e., the methods of proof – used to demonstrate the existence of such a true solution of a model, in theory, may or may not provide any information on the 'adequacy of the model' and 'how well [it] suits the physical system' it aims to 'model'. Secondly, 'the approximations [to the physical system]' to obtain the model may not be definable uniquely. Thirdly, the methods of approximations may not be independent of the methods of proof used to demonstrate the existence of a true solution. Fourthly, unless the chosen methods of approximations are very carefully selected most models that are even reasonably faithful – in any one of many possible ways of 'defining' this tortuous term – cannot be forced to yield unique solutions. Finally, and most disturbingly, from the point of view of *simulation of the dynamics* of 'the trajectories of the differential equations' associated with the model, by machine computation – i.e., by, say, a digital computer – is the following question: can a dynamical system, whose *equilibrium existence* – i.e., 'true solution of a model' – is proved by non-finite means, be simulated by finite means to obtain epistemological answers to questions on the 'true solution of a model'?

Let me illustrate this last point more explicitly. Consider one possible elementary statement of the *Peano existence theorem* for the *initial value problem* (**IVP**) of odinary differential equations (ODEs), [60], pp. 364-365 (italics added):

Let the function $f : [t_0, t_0 + a] \times U \to \mathbb{R}^d$ $\left(\text{where: } U \sqsubseteq \mathbb{R}^d\right)$ be continuous in the cylinder:

$$S = \{(t, \mathbf{x})\} : t \in (t_0, t_0 + a), \mathbf{x} \in \mathbb{R}^d, \|x - y_0\| \leq b \tag{1}$$

Where: $a, b > 0$ and the vector norm $\|\cdot\|$ is given. Then, the ODE:

$$\mathbf{y}' = f(t, \mathbf{y}), t \in (t_0, t_0 + \alpha), \quad \mathbf{y}(t_0) = \mathbf{y}_0 \in \mathbb{R}^d \tag{2}$$

Where: $\alpha = \min\left\{a, \frac{b}{\mu}\right\}$ and $\mu = \sup_{(t,x) \in S} \|f(t, x)\|$ *posseses at least one solution*

However, it can be shown that $\exists f(t, y)$, satisfying the hypotheses of the Peano existence theorem, such that there is **no solution** to the **IVP** ([1]). Why is this so? This is because the existence of a solution violates a cardinal theorem of *computable calculus*: *the Unsovability of the Halting Problem for Turing Machines*. More specifically, there are a series of nonsolvable problems *by finite means*, in the computable calculus, some of which have to be made solvable *by non-finite* means for the Peano existence theorem to be satisfied. In the case of the Peano existence theorem, the relevant nonsolvable problems are:

Proposition 1 *It is undecidable (by finite means) whether,* $\forall a \in R, a$ *or* $\sim a$ *is* ***rational****.*

Proposition 2 *It is undecidable (by finite means) whether,* $\forall a \in R, a \geq 0$ *or* $a \leq 0$*.*

Thus, implicit in any standard proof of the Peano existence theorem there are appeals to non-finite means to decide (intrinsically undecidable) disjunctions. No amount of simulation, even by an 'ideal' computer – say, Turing Machine (or any of its equivalents, by the Church-Turing Thesis) – can contribute epistemologically to any question about the 'true solution of a model', which is analytically fully characterised, but using standard real analysis underpinned by set theory *plus* the axiom of choice.

However, it must be noted that *Peano went out of his way to avoid any appeal to the axiom of choice in his proof*. Furthermore, Peano dropped the assumption of the *Lipschitz condition* in his original paper of 1890 (thereby losing *uniqueness, [59],* p.66). Indeed, it was during the course of this proof that the ubiquity of this controversial axiom was *first recognised and avoided*[2].

This is, by the way, an instance of the ignorance of alternative mathematical traditions and careless understanding of the underpinning axioms which leads, in turn, to ignorant assertions on the interactions between simulation, mathematical formalisms, formal solutions, approximations, and so on[3].

The above conundrums of the formal, mathematical, ambiguities involved in understanding the deep structure of even a simple and ubiquitous theorem like Peano's existence theorem for the IVP problem, surely, is a case in point to invoke Jeremy Avigad's perceptive observation:

> "[The] adoption of the infinitary, nonconstructive, set theoretic, algebraic, and structural methods that are characteristic to modern mathematics [. . . .] were controversial, however. At issue was not just whether they are consistent, but, more pointedly, whether they are meaningful and appropriate to mathematics. After all, if one views mathematics as an essentially computational science, then arguments without computational content, whatever their heuristic value, are not properly mathematical. .. [At] the bare minimum, we wish to know that the universal assertions we derive in the system will not be contradicted by our experiences, and the existential predictions will be borne out by calculation. This is exactly what Hilbert's program was designed to do."
>
> Jeremy Avigad ([6]), pp.64-5; italics added.

Even if one does not 'view mathematics as an essentially computational science', appealing to non-finite means to demonstrate the existence of a solution to a dynamical system and, then, to study its dynamic behaviour by simulation on a (digital or even an analog) computer, to be able to characterise the nature of the solution – say, its basin of attraction – is, surely illegitimate from any epistemological point of view. This is easily illustrated by taking the example of Goodstein's theorem and the associated Goodstein sequence (cf. [42], [84]). They provide an 'elementary' example of *a finite combinatorial result* that cannot be proved without an appeal to *infinite sets, in the form of invoking the use of infinite ordinals*. An illuminative perspective on the Theorem - especially in view of the above example on Peano's existence theorem for the IVP problem for ODEs – is provided by the observation that it is *expressible in first-order*

[2] Long before Zermelo christened it the *axiom of choice*, fourteen years later.

[3] An egregious example of this kind of infelicty can be found in [15], especially pages 74 & 81.

arithmetic but cannot be given a proof in Peano Arithmetic (PA), which is generally considered to coincide with *finite arithmetic*[4].

The Goodstein Sequence itself can be interpreted as a *Dynamical System* of the type: $X_{t+1} = f(X_t, t)$ – i.e., discrete, deterministic and nonautonomous - from $\mathbb{N} \rightarrow \mathbb{N}$, with a very simple *Global Attractor*, but with extremely long transients and possessing the property of **SSDIC** – Super Sensitive Dependence on Initial Conditions. Most pertinently, it has not been possible, so far, to associate it with an ODE which is capable of the kind of rapid transient growth it exhibits.

But what is a computation? The lucid, elementary, answer to this question, entirely in terms of computability theory was given by Martin Davis in a masterly exposition almost three and a half decades ago ([24]). Here I am interested in an answer that links the triad of computation, simulation and dynamics in an epistemological way. As a path towards a tentative answer, it may be useful to recall the way Kant tackled his legendary question,: What is man? He sub-categorised it into the three following questions: What can I know? What must I do? What may I hope?

However, I shall assume the following 'claims', so that I can answer the question 'What is a computation' in an epistemologically meaningful sense.

Claim 3 *Every computation is a dynamical system*

Claim 4 *Every simulation of a dynamical system is a computation*

Analogously, then: What can we know, what must we do and what can we hope *from a computation*, which is, by the above claim, a dynamical system? This, in turn, means what can we know, what must we do and what can we hope from studying the behaviour of a dynamical process during a computation? Since, however, *not everything can be computed* it follows that not every question about a dynamical system can be answered unambiguously. But by the second of the above claims, I have expressed an 'identity' between a *simulation* and a *computation*, via the intermediation of a dynamical system, which implies that *not everything can be learned about the behaviour of a dynamical system by simulating it* on (even) an ideal device that can compute anything that is theoretically computable (i.e., a Turing Machine, assuming the Church-Turing Thesis). Above all, we cannot delineate, in any meaningful sense – i.e., in an algorithmically decidable sense – between what can be known or learned and that which lies 'beyond' this undecidable, indefinable, border, on one side of which we live our scientific lives.

Now, observe the following:

> "An argument deriving the truth of a universal arithmetical sentence from that of its numerical instances suggests that the truth

[4]This does not mean at all that I subscribe to anything like the *ultrafinitist* philosophy of mathematics, especially in view of the fact that even the first order validity for finite models is not recursively enumerable (due to Trakhtenbrot's Theorem, cf. [13], p. 198, problem 15.7).

> of the numerical instances has some kind of *epistemological priority* over the truth of the sentence itself: our knowledge of the truth of the sentence stems from the fact that we know all its numerical instances to be true. .. I shall show that it is just *the other way around*. ... [T]he source of *our knowledge* of the truth of the totality of its numerical instances is the truth of the sentence itself."
>
> György Serény, ([100]),p. 48; italics added.

Juxtapose this inversion of 'epistemological priority' between the 'truth of a universal arithmetical sentence' and 'the totality of its numerical instances' – remembering that the latter is obtained by computations – with Cohen's causal conjecture between 'the revolution in computing' and 'its inspiration in mathematics'.

> "Hilbert's vision of a universal algorithm to solve mathematical theorems[5] required a unification of Logic, Set Theory and Number Theory. This project was initiated by Frege, rerouted by Russell, repaired by Whitehead, derailed by Gödel, restored by Zermelo, Frankel, Bernays and von Neumann, shaken by Church and finally demolished by Turing. Hence, to say that the interest in algorithmic methods in mathematics or the progress in logic was engendered by the computer is wrong way around. For these subjects it is more correct to observe *the revolution in computing* that *was inspired by mathematics*."
>
> Daniel Cohen ([21]), p. 323; italics added.

There are *at least* five frontier research fields in economics, encompassing both micro and macro aspects of economic theory, where machine computation[6], in its digital mode, is claimed to play crucial roles in formal modelling exercises[7]:

1. Computable General Equilibrium *Theory* (CGE) (and its 'extensions': *Recursive Competitive Equilibrium* (*RCE*) & *Dynamic Stochastic General Equilibrium* (*DSGE*) theories) - *The Scarf Tradition*

[5] I suspect Cohen means 'solve mathematical problems', since 'solving mathematical theorems' seems a meaningless phrase.

[6] The claims on computation in standard advanced microeconomics (eg., [73], especially § 20.D) or general equilibrium theory (eg., [5], especially chapter 5 and §. 5 in Appendix C) are easily shown to be untenable from the point of view of any of the six mathematical frameworks for a model of computation, listed in the concluding section.

[7] The discerning reader would easily recognise that I disagree quite comprehensively with the discussion, characterisation and visions of 'computational economics' in [77] (especially in chapter 8, p.523 ff). There are also five varieties of game theory: (vN-M/Nash orthodox) *Game Theory*, *Algorithmic Game Theory*, *Constructive Game Theory*, *Arithmetic Games* and *Combinatorial Games*. Of these the second is really only an attempt at 'algorithmising' orhtodox game theory in the same sense in which computable general equailibrium theory tried to make computable sense of orthodox general equilbirum theory. The last three are intrinsically computational - with the third squarely in the constructive mathematics fold and the fourth (mostly) underpinned by classical recursion theory. Combinatorial game theory could be underpinned by either constructive or computability theory.

2. Computable General Equilibrium *Modelling - The Johansen-Stone Tradition* (cf., [61], [30])

3. Agent based computational economics (cf., [107], [34])[8]

4. Classical behavioural economics (CBE, as distinct from MBE: Modern behavioural economics, see [62])

5. Computable economics[9].

They broach, without resolving, many interesting methodological and epistemological issues in economic theorising in (alternative) mathematical modes.

That CGE in the Scarf Tradition is not uncerpinned by either computable or constructive mathematics is, by now, fairly well known (cf., [119]) But it is less well documented that the claims of computability of the Johansen-Stone tradition is equally untenable. Therefore some comments may be in order.

Somewhat surprisingly, the adherents and aficionados of Leif Johansen's classic work on **A Multi-Sectoral Study of Economic Growth** ([61]) claim that this was 'the first CGE model' ([30], p. 6). Their rationale for this claim is the following (p. 6; last two italics, added):

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

[8] The word 'constructive' in the title of the article by Testfatsion (*loc.cit*) has nothing whatsoever to do with any 'variety of constructive mathematics'. The claims, definitions and characterisations of constructive mathematical aspects of agent-based economics in [34] (chapter 1) are technically incorrect. In particular, the characterisation of nonconstructive existence proofs in terms of acceptance of the *tertium non datur*, implying that the contrary in the case in constructive existence proofs (*ibid*, pp. 11-12), is not accurate ([19]). This is because the author has not stated the kind of *tertiium non datur* unacceptable in constructive mathematics. Above all conflating computability theoretic statements, themselves imprecisely stated, with constructive ones makes the whole argument meaningless, from a mathematical point of view. In [121] these unwarranted claims are discussed in greater and more precise technical detail.

[9] Once again I am in profound disagreement with the views expressed in [77] on the origins and current status of this area of research. In particular, his technical discussions of the work of Alain Lewis (cf., [122] for many of the published classics by Lewis and the *Introduction, loc cit.,* for a summary of his contribution to the 'proto-history' of the subject) is technically incorrect and misleading in many instances. Moreover, the technical contents of the letters by Lewis to Debreu, quoted on pp. 431-2 and pp. 526-7, of [77] contain serious errors, which makes the implications drawn in the text meaningless. In particular, the notions of *combinatorial*, *finite* and *totally finite* models, *continuity* and *Peano Arithmetic* are used, and referred to,by Lewis in unfortunately imprecise, indeed incorrect, ways. The juxtaposition of combinatorial and recursive analysis, on the one hand, and combinatorial and totally finite models is simply incorrect, from a purely mathematical point of view.

This is an untenable claim, especially since it is very specific: that the Johansen model is of the *applied general equilibrium* variety which is also *computable* and, hence, *numerically* meaningful[10]. The Johansen model has no underpinning whatsoever in any formal model of computation, least of all a computable (or constructive) one.

The main claim in the paper, albeit stated only implicitly and expressed indirectly, is that there is a serious *epistemological deficit* in all of the approaches, but can be 'discovered' only in the last two, precisely because the latter are underpinned by computability and constructivity theories, in their strict mathematical senses, and the former are not (see the appendix to this section, below). However, this claim does not imply that classical behavioural economics and computable economics are 'complete' from an epistemological perspective, especially from the point of view of natural or intrinsic dynamics of formal models. The epistemological deficit and the epistemological incompleteness, it is suggested, can be resolved by a theory of simulation, itself based on recognising the double 'duality' between dynamical systems and numerical analysis, on the one hand, and that between computation and simulation, mediated by dynamical systems, both 'dualities' interpreted computably or constructively, leading to the core triad of computation, simulation and dynamics (because numerical analysis can be interpreted, equivalently, in terms of dynamical systems or computably). Hence, hopefully, paying heed to Turing's Precept: "the inadequacy of 'reason' unsupported by common sense."

Let me end this preliminary reflection on a computable approach to a discussion of the epistemological deficit intrinsic to the interlinked triad of computation,simulation and dynamics by invoking Feynman on my side:

> Computer science touches on a variety of deep issues. ... . It naturally encourages us to ask questions about *the limits of computability, about what we can and cannot know about the world around us*."
>
> Richard Feynman, [37], p. xiii; italics added.

The rest of this paper is organized as follows. In the next section I tell a contemporary story of simulational serendipities, just to set a light-hearted tone for what may appear to be too technically demanding paper making a case for underpinning the triad rigorously and, then, economic theorising on the triad. Section 3, I think, is where the full setting for computation, simulation and dynamics to interact fruitfully in an epistemological sense, along research frontiers encapsulated by what I call the *Zabusky Precept* at the end of the section

[10] My stance on this issue is reflected exactly by the view held by my friend, Lance Taylor. After attending the recent 50th anniversary celebrations of the Johansen Model, held in Oslo, Lance wrote as follows (E-mail, 27 August, 2010; italics added):

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

. Section 4 is on *Computation, Discretization, Proof and Other Mathematical Infelicities*. In the final section I 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' with reflections and hopes on what must be done to keep the bright shining light of computation, simulation and dynamics focused on economic theory and its formalisation.

1.A Visiometrics and the Graph Theorems

> "The problems of scientific computing often arise from the study of continuous processes, and questions of *computability* and complexity *over the reals* are of central importance in laying the foundations for the subject. *The first step is defining a suitable computational model for functions over the reals.*"
>
> Braverman & Cook ([17]), p.318; italics added.

Of the five frontier research fields in economics claiming computational underpinnings, mentioned above, only the first and the last two are explicitly based on 'a suitable computational model for functions over the reals'. Moreover, both classical behavioural economics and computable economics are either explicitly or implicitly defined over the computable, constructive or nonstandard reals. However, computable general equilibrium theory (in the Scarf mode), even though it is defined over the reals, does not develop its theory on a model of computation that is consistent with its basis on all of the standard reals (a fortiori for RCE and DSGE). Neither computable general equilibrium economics (in the Johansen-Stone mode), nor agent based computational economics (and finance), are based on any kind of formal (or informal) 'computational model over the reals'. None of the above five frontier research fields, except every kind of agent based computational economics (and finance) practice[11] rely overwhelmingly on an elementary – and undefined – kind of *Visiometrics*, appealing to computer graphics, without the slightest basis in the mathematics of the computer (of any variety). In this brief appendix I outline the kind of care needed to make sense of the excessive claims of the practitioners of agent based computational economics (and finance)[12], so that they can be brought into the fold of serious

[11] The field is a paradigmatic example of that classic approach Koopmans called 'measurement without theory'.

[12] An egregious example of such an excessive and completely unfounded – in any kind of practice or theory - claim is the one made by Leijonhufvud (In his chapter, titled *Agent-Based Macro* ([108], p. 1626; italics added):

> "Agent-based computational methods provide *the only way* in which the self-regulatory capabilities of complex dynamic models can be explored so as to advance our understanding of the adaptive dynamics of actual economies."

Quite apart from the many undefined and even formally (unambiguously) undefinable concepts in this statement, the extraordinary claim that 'agent-based computational methods provide *the only way*' to understand anything, let alone of the 'adaptive dynamics of actual economies' must rank with the most foolish claims, even in a field replete with an embarrassment of riches of this class of assertions (for example, see [26])

Visiometrics[13].

Zabusky's more discursive description – I shall not call it a formal definition – of what he means by *Visiometrics* is something like the following (loc.cit, p.12; italics in the original):

> "VISIOMETRICS is the process of: *Visualization, projection, identification and juxtaposition of evolving amorphous coherent structures and statistical backgrounds in massive multidimensional data sets.* The goal is to produce cogent images and specific, parameter-scaled (normalized) graphs for intuition building and mathematization."

The crucial terms are 'cogent images' and '*graphs* for intuition building and mathematization', especially in the case of *economic visiometrics*, where the 'massive multidimensional data sets' reside, at best, in the set of rational numbers.

Essentially, all of the above five frontier computational economic research areas theorise in the domain of the real numbers and real number functions (of arbitrary high dimensions), but agent based computational economics, in particular, seeks to extract patterns from the projected dynamics on the screens of digital computers (there are, of course, enlightened exceptions to this general rule). If the theorising is in terms of real analysis, then the 'graphs' of theory are subject to the classical graph theorem:

Theorem 5 *The 'Classical' Graph Theorem: A function (or mapping or transformation),*

$\psi : A \rightarrow B$ is any subset $\psi \subseteq (A \times B)$ such that $(\forall x \in A)\,(\exists! y \in B)$ & $[(x, y) \in \psi]$ and $(\forall x \in A)\,(y \in B)\,, [(x, y) \in \psi]\,\&\,[(x, y') \in \psi \Longrightarrow y = y']$

Where: A : Domain set; B : Range set: $!$: $\exists$ *exactly one*;

Theorem 6 *The Recursion Theoretic Graph Theorem: Let φ and ξ be, respectively, a partial and a total function. Then:*

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

2. ξ is *recursive* **iff** its *graph* is a *recursive set*;

To these theorems I would like to add, and invoke, what I call Brattka's Theses ([16]):

Criterion 7 *Brattka's Thesis I: A* function *$\zeta : \mathbb{R}^n \rightarrow \mathbb{R}$ is computable iff it can be evaluated on a physical computer with arbitrary given precision.*

Criterion 8 *Brattka's Thesis II: A closed subset $\Phi \subseteq \mathbb{R}^2$ is recursive iff it can be displayed by a physical computer for an arbitrary given resolution.*

[13] Zabusky's imaginative *Visiometric* approach was developed independently of Ralph Abraham's equally illuminating development of the *Vismath* vision for studying the geometry of dynamic behavior (see, for example, [3]).

Conjecture 9 *No function employed in any agent based computational model satisfies either of Brattka's Theses.*

Remark 10 *This is too strong a conjecture; I should be more specific about the particular agent based model. However, I want only to give a flavour of the kind of conditions that have to be satisfied for some of the enthusiastic, vague and imprecise claims to be valid.*

From these theorems and criteria any discernig reader can understand why I claim that classical behavioural economics and computable economics are eminently suitable for Visiometric explorations. Moreover, these theorems and criteria are also the reasons why I made '*the main claim*' of this paper, above.

2 Simulational Serendipities

The May-June, 2009, issue of the **American Scientist** (Vol. 97, No. 3) contained, *serendipitously*, four – possibly five[14] – articles on the fundamental role played by *simulations* - in its synergetic interactions with *theoretical analysis*, *experiment*, *computation*, *prediction* and *dynamics* – in macroeconomics ([52]), physics ([91]), engineering ([86][15]) and a 'revisit' to the *Limits to Growth* report ([48]). 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[16] simulated, without any recourse to approximations or discretizations, normally required in a *digital* computer - unless, of course, continuous data was available (which was not) to exploit its –

[14] This is because the article on *The Origin of Life* in this issue of the **American Scientist** ([111]) 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.

[15] 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!

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

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 [7], 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[17] 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[18]:

> "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 or even vaguely, 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 (even in more senses than in the famous Fermi-Pasta-Ulam exercise, to which I now turn, albeit 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 ([91]) 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 generalizations in the fifty-five years since the original 'experiment', are exhaustively discussed and dissected in the admirable monograph by Thomas

[17] A truncated version of which appears in [67], chapter 13, pp. 118-9. Copies of the full memoir and the covering letter to Nicholas Stern were sent by Goodwin to me; I will be happy to make them available to interested readers.

[18] 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.*"

Weissert[19] ([124]). As they perceptively and clearly[20] 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, which (see [129]) 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 personal conversations with me[21], the construction of a bridge is *less* about physics

[19] 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'.

[20] 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, [128]).

[21] The phrase used by Dr Allan McRobie to refer to the *Millennium Bridge* is the *Rainbow Bridge*– also referred to as the *Blade of Light* (cf. [105]) – during a personal conversation

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 studied 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. 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 I shall return in the sequel.

The fourth of the serendipitous articles is by Hall & Day ([48]), 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* ([75]). 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,

with me, in Cambridge, on 17th May, 2010. Dr Allan McRobie was instrumental in 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. I had gone to Cambridge to view a demonstration of the resurrected *Phillips Machine*, an analogue computing machine constructed by the 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. I 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 ([87]).

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[22]. 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 ([76]) – 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.

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[22] 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 ([41], 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'.

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 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.

3 Discovering Dynamics by Simulation in Macroeconomics

> "Progress in our understanding of the natural sciences has always depended upon the give-and-take between modelling (or theorizing), analysis, and experiment. With large-scale computers, we can process experimental data from a variety of sensors and juxtapose them readily with large-scale *simulations – numerical solutions of ordinary and partial differential equations*, etc. The insights gained from *attempting to bring these results into agreement* can synergize[23] the rate of improvement of *models*, *algorithms*, *analytical methods*, and *experimental procedures*."
>
> Norman Zabusky, [127], p. 236; italics added.

At least five kinds of standard dynamical systems have been routinely used in economic theory: Ordinary Differential Equations (Linear & Nonlinear; ODEs) – Standard & *Nonstandard*[24], Partial Differential Equations (Linear & Nonlin-

[23] Zabusky has almost from the outset of his sustained work on the Fermi-Pasta-Ulam problem used the word 'synergize' in the sense in which it was first used, in a similar context, by Ulam (cf., [114], chapter VIII, § 10):

> "I use the word 'synergetic' here to mean the enhancement in the rate and depth of mathematical understanding through the combined use of analysis and computer simulation." ([127])

May one be forgiven for wondering, in 'Nozick mode', *if not simulation by a computer, then simulation by what*?

[24] I am referring to formal nonstandard differential equations (cf., for example, [29], especially chapter 10 or [35]). To the best of my knowledge nonstandard stochastic differential equations have not been applied in any area of economics or finance theory. In finance theory, in particular, they should be the natural framework for modelling dynamics. A particularly lucid and pedagogical exposition of this virgin field can be found in [4].

ear; PDEs), Stochastic Differential Equations (Linear, Nonlinear and Partial; SDEs), Mixed Difference-Differential Equations, Difference Equations (Linear & Nonlinear; Maps).

Remark 11 *The above is a 'descriptive' delineation. I am able to 'reduce' them to two classes – perhaps even one – s.t. all the other forms can be dealt with as 'special cases'. But this makes the discussion somewhat un-illuminating!*

In economics, formal nonlinear dynamics, in terms of explicit and standard Rayleigh-van der Pol equations, were first introduced, within the context of an explicit Keynesian model of aggregate fluctuations, in the late 1940s ([44])[25]. From the outset simulation studies of variants of these equations explored their dynamic behaviour due to the difficulties of taming them with purely analytical means ([102], [103]) - but these were conducted on electro-analogue computers (or with 'pencil-and-paper' in classic geometric mode, using the auxiliary device of the *characteristic*, first described with almost exquisite pedagogical elegance in [66]).

It seems to me that it is possible to describe all nonlinear, endogenous (i.e., non-stochastic), dynamical equations that have so far been used in modelling aggregate fluctuations (and, often, also non-aggregate dynamics) are special cases of the following canonical mixed difference-differential equation:

$$\begin{aligned} &F[t, u(t), u(t-\omega_1), ..., u(t-\omega_m), u'(t), u'(t-\omega_1), ... \\ &......., u^{(n)}(t-\omega_1), u^{(n)}(t-\omega_m)] = 0 \quad .... \qquad (2) \end{aligned}$$

This is an n^{th} order difference-differential equation and it is a function of $1 + (m+1) + (n+1)$ variables where F and u are real functions and $\omega_i \in \mathbb{R}$ and $n \in \mathbb{Z}$.

In particular, the more well-known theories of aggregate fluctuations, modelled as nonlinear, endogenous, dynamical systems, have encapsualted the underpinning economic theories in terms of:

The van der Pol equation:

$$\ddot{x} - k(1-x^2)\dot{x} + x = 0 \tag{3}$$

Equations of the Liénard type:

$$\ddot{x} + f(x)\dot{x} + g(x) = 0 \tag{4}$$

studied in the Liénard Plane:

$$\dot{x} = y - F(x), \dot{y} = -g(x) \tag{5}$$

The generalized, forced, van der Pol equation:

$$\ddot{x} + f(x, \dot{x})\dot{x} + g(x) = p(t) \tag{6}$$

[25] Like all such attributes of origins, a great deal of caution should be exercised in interpreting this claim, as clearly outlined in my paper with Ragupathy ([93]).

The Rayleigh equation:

$$\ddot{x} + \eta\left(-\dot{x} + \frac{\dot{x}^3}{3}\right) + x = 0, \{0 < \eta < \infty\} \tag{7}$$

The Logistic Map:

$$x_{n+1} = \lambda x_n (1 - x_n) \tag{8}$$

The difference-differential equation:

$$\sum_{\mu=0}^{m}\sum_{\nu=0}^{n} a_{\mu\nu} y^{(\nu)}(x+\mu) = 0 \tag{9}$$

The second-order difference equation:

$$\begin{aligned} &y_{n+1} = F(y_n, y_{n-1}) \quad \forall n = 0, 1, 2, ... \\ &\text{where } F : \mathbb{R}^2 \to \mathbb{R} \ \& \text{ given initial conditions } y_0, y_{-1} \in \mathbb{R} \end{aligned} \tag{10}$$

The first five encapsulated the business cycle theories of Fisher, Keynes, Harrod, Schumpeter and Hawtrey - in the pioneering works of Kaldor, Hicks, Goodwin, Yasui, Ichimura, Morishima and others ([93]); the sixth, models of the 'cobweb' type, as in Leontief ([70]); the seventh, in various specialised forms, the business cycle theories of Tinbergen, Kalecki and Frisch; the last one, with variously specified functional forms for F, encapsulated variations on the dynamics of *Swedish Sequence Analysis* (although I do not fully subscribe to this interpretation of their 'dynamic method'), on the one hand, and the Hicks version of a Keynes-Harrod model of the trade cycle.

The example I shall discuss in this section is an appropriate version of any one of (4), (5), (6), (7) or (8), all of which are formally 'reducible' to one another, from particular economic, geometric or numerical points of view. It is an example encapsulating a noble tradition of computation in economics in every sense of this concept, to study a precisely specified mathematical system on both analogue and digital computers. It is, in a precise sense, also a substitute for an analytical study (because such a study is provably 'unlikely' to succeed in any meaningful way). Moreover, it can be viewed as an explicit example of an epistemological tool to interpret the results (most of which were unexpected) to gain insight into the link between a computing machine and *its theory* and the theory of nonlinear dynamical systems. The latter point is turning out to be the most significant from the point of view of the epistemology of computation, since the interaction can only be explored by representing the one system by the other – and, therefore, even an exploration into a new domain: studying the repertoire of digital machine behaviour with analogue computing machines, and vice versa.

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

$$\varepsilon \dot{y}(t) + (1-\alpha)\, y(t) = \phi\left[\dot{y}(t-\theta)\right] + \left[\beta(t) + l(t)\right] \tag{11}$$

Now, there are *at least* six different ways to investigate dynamic behaviour and solutions (or the basins of attraction) to this nonlinear difference-differential equation:

- In old fashioned analytical modes;
- Using *nonstandard analysis*;
- *Graphically*, i.e., in terms of the *geometry of dynamic behaviour*, as usually done in the qualitative theory of differential equations (cf.,[45]);
- By the method of equivalent linearization (cf., [14]);
- Using an electro-analogue computer (cf., [103]);
- Using digital computers (cf., [109], [129]);

Assuming, for example, $\beta(t) + l(t)$ a constant and reinterpreting $y(t)$ as a deviation from the *unstable equilibrium*, $\frac{\beta(t)+l(t)}{(1-\alpha)}$ of (12) one obtains a mixed nonlinear difference-differential equation:

$$\varepsilon \dot{y}(t+\theta) + (1-\alpha)\, y(t+\theta) = \phi\left[\dot{y}(t)\right] \tag{12}$$

Now, expanding this equation by a Taylor series approximation and retaining only the first two terms, one obtained the famous (unforced) Rayleigh (- van der Pol) - type equation:

$$\ddot{y} + \left[\frac{\chi(\dot{y})}{\dot{y}}\right]\dot{y} + y = 0 \tag{13}$$

With this approximated reformulation began an 'industry' in the endogenous theory of the business cycle, where the cardinal desideratum was the existence of a unique, stable, limit cycle, independent of initial conditions. For $\chi(\dot{y})$ displaying a 'cubic characteristic', such as: [Figure 1 here]

The unique, stable, limit cycle, independent of initital conditions, was shown to display planar dynamics such as: [Figure 2 here]

All four desiderata were violated when the approximations were more precise – in a purely technical sense – and the analysis proceeded via studies by means of *analogue and digital computing machines*. Using an electro-analog computer, it was found, in [103], that the approximation of (11) 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. Thus, for:

$$\epsilon\frac{\theta^4}{24}y'''''(t) + C_4 y''''(t) + C_3 y'''(t) + C_2 y''(t) + C_1 y'(t) - \phi\left[y'(t)\right] + (1-\alpha)\, y(t) = 0 \tag{14}$$

The displayed phase-plane dynamics were:[Figure 3 here]

Even more interestingly, the insights obtained from an analogue computing machine study provided hints in setting up a computing study of (12) by means of digital computing machines.

Next, coupling two equations of type (14), via the Phillips Electro-Mechanical-Hydraulic Analogue Computing Machine (*MONIAC*, [120]), Goodwin and Phillips were able to generate – unexpectedly – the quasi-periodic paradox ([2]). Neither Goodwin, nor Phillips, who did the coupled-dynamics computation on the Phillips Machine, had any clue – theoretical or otherwise – about interpreting and encapsulating this outcome in any economic theoretical formalization. The key point is that they were surprised by the outcome and did not know how to interpret it when it emerged. This is where the richness of the epistemology of computation manifests itself most dramatically. There was no macrodynamic theory to which they could relate the observed behaviour, which was contrary to expected behaviour.

Relaxation oscillations encompass two-phase dynamics in the sense that there is an interaction between slow and fast variables in the system, rather like one set of markets (financial?) clearing 'infinitely fast', and another set (real?) clearing in relatively slow mode. The problem, of course, is that '*infinitely fast*' is a meaningless concept in standard analysis, but an eminently sensible notion in *nonstandard analysis*; analogously, the '*infinitesimal*' is a fully viable concept in *nonstandard analysis*, but not so in standard analysis. What kind of dynamics can one expect for (12), when the mathematical underpinning of infinitely fast and infinetsimally slow variables are made to work in the nonstandard analytic phase-plane?

Consider, now, the following nonlinear equation, a variant of (12):

$$\epsilon \frac{d^2x}{dt^2} + \left(x + x^2\right) \frac{dx}{dt} + x + \alpha = 0 \tag{15}$$

This can be represented as:

$$\frac{dx}{dt} = \epsilon^{-1}(y - f(x)) \tag{16}$$

$$\frac{dy}{dt} = -(x + \alpha) \tag{17}$$

and the 'characteristic', $f(x)$, is given by:

$$f(x) = \frac{1}{3}x^3 + \frac{1}{2}x^2 \tag{18}$$

The phase-plane dynamics depicted in the diagrams below are for the following numerical values of α and ϵ (the red curves, in all cases, are the graphs of the 'cubic characteristic').

1. The values to get (Figure 2) were so chosen that the phase-plane dynamics would resemble, as closely as possible, that given in the pioneering nonlinear trade cycle model of Goodwin; here, $\alpha = .45; \epsilon = 10$;

2. Figure 4, almost similar in its geometry to the first one, has: $\alpha = .5; \epsilon = 1000$;
3. Figures 5 and 6 (the 'Duck Headed vdP' dynamics) were obtained for: $\alpha = .001012345; \epsilon = 1000$;
4. Finally, Figures 7 and 8 for the 'Unheaded Duck vdP's were obtained for: $\alpha = .00001025; \epsilon = 1000$;

[Figure 4, 5, 6, 7 & 8 here]

For $\alpha = 0$ and if $\frac{1}{2}x^2$ is replaced by x, the system reduces to the van der Pol equation on the Liénard plane[26].

It is clear that y is the 'slow' variable; i.e., it is finite for all finite points of the domain of the plane; x, then, is the 'fast' variable and takes infinitely large values for some finite values of its domain. If the trajectory of y is defined to be on that curve at which $\dot{x} = 0$, then its graph is given by $f(x)$.

As for the 'Duck' terminology, the idea should be obvious from a perusal of the diagrams (and a bit of imagination!).

The proof of existence of 'Unheaded Ducks', i.e., *counter-intuitive cycles* being *attracted to unstable manifolds*, for the van der Pol system is extremely simple - provided one learns a bit of nonstandard analysis - or, at least, non-standard terminology. Let me simply state it, in as heuristic and intuitive way as possible, to illustrate what we mean; the interested reader can get a clear idea from the exceptionally clear and detailed article by Zvonkin and Shubin. The only thing to keep in mind is that α *in an infinitesimal in the sense of nonstandard analysis*. Then[27] (referring to the last two phase-plane diagrams) and [130], §4.2:

Definition 12 *An admissible form for the characteristic, $f(x)$.*

$f(x)$ has an admissible form on a closed interval, say $[\beta_1, \beta_2]$, if:

(1). $f(x) \in [\beta_1, \beta_2]$ is standard and $\mathbb{C}^2$;

(2). $f(x) \in [\beta_1, \beta_2]$ has exactly two isolated extremum points, say a minimum at x_0, and a maximum at x_1, and $\beta_1 < x_1 < x_0 < \beta_2$, so that: $f'(x) > 0$ on $[\beta_1, x_1)$ and $(x_0, \beta_2]$ and $f'(x) < 0$ on (x_1, x_0);

(3). $f(\beta_1) < f(x_0)$ and $f(\beta_2) > f(x_1)$;

Theorem 13 *Existence of Duck Cycles in the van der Pol system [(2) or (3)–(5)].*

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[26] I chose the 'characteristic' with the $\frac{1}{2}x^2$ term, instead of the x term only because I had severe difficulties of precision to get the kind of phase-plane dynamics I could have got with a computer capable of more precise computations.

[27] This is only a sufficient condition and the 'admissible curve' is simply a formalization of the traditional 'cubic characteristic' for the van der Pol equation. I conjecture that 'Duck Cycles' can be shown to exist even without a 'cubic characteristic'; say, for example, with a 'characteristic' of the form: $\tau \left(e^{\dot{x}} - 2\right)$. Such a form would have only one isolated maximum or minimum.

Suppose $f(x)$ *has an admissible form on* $[\beta_1, \beta_2]$; *if* $x_\beta \in \Re$ *and* $x_\beta \in (x_1, x_0)$, *then* $\exists$ *value of the* infinitesimal α*, for which the van der Pol system has a Duck-Cycle such that* x_β *is the value on the x-coordinate corresponding to the 'beak' of the 'Duck'.*

The point of the exercise is that a knowledge of the possibilities for exploring a dynamical system with parameters and variables taking *infinitesimal* and *infinite* values is indispensable - not just for reasons of pure mathematical aesthetics; but also for eminent economic reasons, where financial market variables move 'infinitely' fast, at least relative to 'real' variables; and reactions in market sentiments to 'infinitesimal' variations in parameters is a non-negligible factor in turbulent markets. An economist, narrowly trained in standard mathematics will always have to resort to ad hockeries to handle the *infinitesimal* and the *infinity* - for example, in models capable of relaxation oscillations. Quite apart from aesthetics and pragmatics, it is also the case that the mathematics of nonstandard analysis is intuitively natural and much simpler, without all the artificial paraphernalia of the '$\epsilon - \delta'$ calisthenics.

But the purist may wonder whether the mathematics of the digital *computer*, on which the above *simulations* helped me 'discover' counter-intutive *dynamics*, is faithful to the underpinning logic of algorithmic mathematics. Without the space to go into details, it goes without saying that smooth or infinetisimal analysis (cf., for example, [8]) is entirely based on intuitionistc logic and its source is category theory. These two facts alone are sufficient to answer any sceptical purist.

Suppose, now, we are made aware of Ralph Abraham's conjectures on stability ([2], 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."

Simultaneously we, as economists, recall Leontief's characteristically perceptive observation ([46], 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 *a priori* reason for any of these kinds of dynamical systems to be stable for observational, simulational or experimental purposes ([89]) – especially also since it is easy to show that only dynamical systems incapable of being underpinned by any notion of maximization ([96], p. 12) are capable of *computation* *universality* and, hence, consistent with the standard assumption of rationality in economics[28].

Finally, the simulated dynamics, on only a 'laptop' computer, illustrate unambiguously, the precepts for the synergy enunciated by Norman Zabusky with which I started this section: 'The insights gained from *attempting to bring these results into agreement* can synergize the rate of improvement of *models*, *algorithms*, *analytical methods*, and *experimental procedures.'* The two most important, unexpected or surprising improvements were the generation, entirely by the simulation of an orthodox dynamical system, of the counterintuitive notion of a trajectory attracted to an unstable manifold, justified by an 'alternative' mathematical formalism. Economists, in particular, schooled for centuries on a particularly narrow vision of what Paul Samuelson quite perceptively referred to as the 'dogma of stability' ([97],p. 10) in business cycle theorising, and almost totally ignorant of alternative mathematical formalisms, have much to gain from what I should henceforth refer to as the *Zabusky Precept*[29].

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[28] A concise summary of the relevant results are:

Definition 14 ***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 15 *Dynamical systems characterizable in terms of limit points, limit cycles or 'chaotic' attractors, called 'elementary attractors', are not capable of universal computation.*

Theorem 16 *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*

Claim 17 ***Only dynamical systems capable of computation universality can generate behaviour that cannot be encapsulated in, or rationalised by, any notion of maximization.***

[29] I must hasten to add that what I have referred to here, as the 'counterintuitive notion of a trajectory attracted to an unstable manifold' is not an entirely new or fanciful notion for economics. It is a partial property of the famous Keynes-based Hicksian, piecewise linear, endogenous, model of the trade cycle (cf. [99]).

4 Computation, Discretization, Proof and Other Mathematical Infelicities[30]

> "[T]here is, strictly, no such thing as mathematical proof; that we can, in the last analysis, do nothing but *point*[31]; 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 the imagination of pupils. This is plainly not the whole truth, but there is a good deal in it."
>
> [49], p. 18; italics in the original

In their highly slippery discussion of the interaction between individual rational behaviour and equilibrium, L&K 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 I have to the claim in this statement can be divided into three sub-objections:

'Computers cannot model such an infinite regress';

[30] This section may appear to be motivated by the loose, dangerous and thoroughly uninformed opinions – not reasoned arguments - on simulation, proof and computation, made in [68], henceforth referred to as L&K, in this paper. Although it is motivated, only ostensibly, by the themes in a thoughtless – even mindless – and unreflective paper, replete with terrible inaccuracies of a conceptual, factual and technical nature, I have chosen a place for it in this paper partly because the substance of this section was written before I became aware of the absurdities in L&K. The fulcrum around which the content of the section may appear to be defined by the themes in L&K, but it was conceived and constructed independent of it.

[31] 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 orthodox mathematical economists

Computers, are 'based on[32] 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*[33] ones. There are eminently respectable constructive and computable fixed points that can be utilized to 'break' the infinite regress emerging from the potential indeterminacies of mutual expectations. In [117], §4, I have, 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 ([20]), which can be used to define the kind of equilibrium with the infinite regress of mutual expectations.

The caveat to these objections and my 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 or 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[34].

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[35].

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

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

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

[34] For a representative, but not exhaustive, sample of such work, see [122].

[35] See the eminently readable text by Nordström, Petersson & Smith ([81]) 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 [23], 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.

'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)[36].

Incidentally L&K seem to contrast – like many in economics – a '*digital proof*' with a so-called 'analytical proof'. If what is meant 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* ([9], [10] or [11])[37] is a *digital proof.* If by *proof* is meant, say, those sanctioned by *intuitionistic logic* only, then practically every so-called proof in almost any kind of formalised eocnomic theory fails to be acceptable. Even if not underpinned by intuitionistic logic, in many varieties of constructive analysis – for example in [9] – no appeal will be made to the *tertium non datur* in cases where infinitary instances have to be considered. Hence, any proof of a theorem in mathematical eocnomics or game theory, derived with appeal to the Bolzano-Weierstrass theorem, cannot and *will not be considered a valid proof existence.* By now many are aware that both the Nash equilibrium, as derived by John Nash, and the Arrow-Debreu equilibrium, are theorems whose proofs are based on the Brouwer[38] (or Kakutani) fixed point theorem, but few note that this is mainly because the so-called proofs invoked the Bolzano-Weierstrass theorem, which makes them unimplementable in a digital computer underpinned by computable, constructive or nonstandard mathematics.

On the other hand, every proof 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? I 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, Tymoczko's discussion of 'surveyability' of proofs ([113]) sug-

[36]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?

[37]See also the elegant and illuminative discussion on *Algorithm in Modern Mathematics and Computer Science* by Donald Knuth ([65]), 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."

[38]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 [20]).

gests (*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, [92], [116], especially chapter 3, and [119], 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 falls back on intuitive concepts such as 'rational agents', humanly surveyable', 'recognize', and so on. Suppose, however, Tymoczko 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. There are many other ways I can cast seriously rigorous doubts against Tymoczko's loose, allegedly philosophical, arguments against considering 'computer-assisted proofs' as 'mathematical proofs', but this must suffice for the moment[39].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'[40]. It is not as if there are no rigorous, formal, definitions, even in graduate textbooks with impeccable credentials ([25], comes to mind at once; see p. 536). However, a serious discussion of *program verification* requires much more depth and understanding of the issue, for example as in Platek's crystal clear, almost pedagogic, yet deep, article ([88]).

> " 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

[39] Incidentally, 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 ([36], 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.

[40] 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 [25]). But even if we grant them this 'definition', what is the scope of 'comparable'?

the tedium of eternal verification' of the validity of 'analytical proofs'![41] 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[42]. 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[43] (see, for example, [31][44], [85], chapter 3 and [58]).

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* ([112]). In 1985, no less an authority on dynamical systems theory than Morris Hirsch observed, for the Lorenz System[45] ([56], 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' ([2], p. 117; italics added):

[41] 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'!

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

[43] Incidentally, Écalle's proof of Dulac's conjecture was constructive (see [33]).

[44] I must confess that I have never actually read the original by Dulac, mainly because I am incapable of reading any intricate mathematical text in French. My 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, [126].

[45] 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*.

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 ([104]).

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 I have tried, over the years and in many of my writings; cf, for example, [116] and [115]), 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., [78]) 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 ([112], 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, [95], chapter 3). The parallels are even more than just the recourse to a digital computer to evaluate complex numerical calculations. In [112] (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[46]. 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, with *almost* absolute certainty that[47] ([95], p. 83; italics added):

[46]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.

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

"[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 ..."

However, in these kinds of hybrid proofs, where the analytic (usually nonconstructive) 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. 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 ([94], p. 100)[48]:

"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 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 ([98]), 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

[48]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'.

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[49]. 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 ([98]), 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. This kind of economist knows, perhaps, instinctively, that there is no point in simulating anything, using a non-constructive algorithm, to find an uncomputable 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?

[49] 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 ([20]) 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.

5 Reflections and Bright Hopes

> "The vital discovery which made possible *the analysis of a process of change, in properly economic terms, was the introduction of accounting procedure.* While economists were fumbling around to find a set of categories by which they could make a formal analysis of economic change, other people were doing the job in a professional manner. *In all its main forms, modern economic dynamics is an accounting theory.* It borrows its leading concepts from the work of which had previously been done by accountants (with singularly little help from economists); and it is in accordance with this that *social accounting* should be its main practical instrument of application."
>
> Sir John Hicks ([54]), p. 141; italics added.

It was fitting that Hicks, who considered himself *An Accountant Among Economists* ([64]), made these observations in the *Festschrift* for Lindahl, himself the founding father of Macroeconomics underpinned by *social accounting.* There is no better exponent of this particular vision of *modern economic dynamics* as *an accounting theory* and social accounting as its main instrument of application than Lance Taylor. It is in this particular sense that I believe Taylor carries on the noble tradition of Petty, the Swedes of the Period of Shackle's *High Theory* – in particular, Lindahl, Myrdal and Svennilsson – and the parallel work of Frisch in the same period, inherited by Leif Johansen, and the Keynes of **How to Pay for the War** ([63]), from which the *Political Arithmetic* of Stone emerged[50]. It was profoundly misleading for the computational economists who underpinned their economics in equilibrium and rationality to seek to claim they were working in the traditions of Leif Johansen, who was balancing accounts in a social accounting framework.

The numbers that make up a social accounting system – whether in formal matrix format or in any other scheme that allows the accounting balance, over time, of credits and debts, retrospectively or prospectively – are, at best, *rational numbers.* The founding father of Political Economy, Petty, laid the foudations for *Political Arithmetic*, in his opening statement in *Political Arithmetick*[51]

> The Method I take to [carefully examin[e] whatever tends to

[50] The third of the many books Sir John Hicks wrote ([53]) was titled **The Social Framework: An Introduction to Economics,** of which the two chapters of the last part, Part V, are entirely devoted to social accounting systems. Hicks began writing it around the time **How to Pay for the War** was completed and Stone began his work, under Keynes, in putting together the accounts to implement the Radical Plan for the Exchequer advocated in it. The Swedes had a head start on this – and Hicks was intimately aware of the Swedish work on social accounting as an underpinning for the emerging field of macroeconomics. I want to add here that Social Accounting is one thing; so-called Stock-Flow Consistent Modelling is quite another animal. Where the latter is non-trivial – which is a rare event – it is part of some meaningful Social Accounting System. Lindahl and Myrdal, in their work in the 1920s and 1930s, developing what later came to be called Macroeconomics, knew and worked within the discipline such awareness entailed.

[51] I do not know why, when and how the **k** in *Political Arithmetic***k** disappeared!

> lessen my hopes of the publick Welfare, is not yet very usual].. for instead of using only comparative and superlative Words, and intellectual Arguments, I have taken the course (as a Specimen of the Political Arithmetick I have long aimed at) to express my self in Terms of *Number, Weight, or Measure*; to use only Arguments of Sense, and to consider only such Causes, as have visible Foundations in Nature; leaving those that depend upon the mutable Minds, Opinions, Appetites, and Passions of particular Men, to the Consideration of others.
>
> William Petty, *Preface* to **Political Arithmetick** (3rd Ed., http://www.marxists.org/reference/subject/economics/petty/); italics added

The **natural data types** of the '*number, weights and measures*' in economics and finance are the the integers or rational numbers. But almost without exception all economic and finance theorising assumes that observable data – or even digitally generated data, from these two spheres – are real numbers (or, in moments of enlightened weakness, one or another kind of the non-standard reals). How the real numbers of theory are related to the numbers that a digital computer can process, is never specified - at least not in any of the standard advanced (or even elementary) textbooks in economics, finance, IO, game theory or whatever. Maury Osborne's warning to traders in the stock market, not to approximate by the continuous that which is intrinsically discrete[52], made over a quarter of a century ago, has never been heeded by the 'traders' but, even worse, not even those who sometimes provide the *hilfskonstruktion* of computer programs for their 'mechanised' responses to actual events, represented by the ostensibly patterned data on computer screens :

> As for the question of replacing rows of closely spaced dots by solid lines, you can do that too if you want to, and the governors of the exchange and the community of brokers and dealers who make markets will bless you. If *you* think in terms of solid lines while the *practice is in terms of dots and little steps up and down*, this *misbelief* on your part is worth, I would say conservatively, to the governors of the exchange, *at least eighty million dollars per year*."
>
> [83], p.34; italics added.

Brian Hayes ([50]) reminded us - at least those of us concerned with respecting the discrete and finite precision nature of digital computers - of the dangers of arbitrary approximations and routinised truncations of standard computations:

[52] I want to make it crystal clear that no where have I argued that all economic theorising should be in terms of so-called discrete models; nor have I ever supported the ridiculous suggestion that 'continuity' is irrelevant in economic theorising. Nothing in the kind of economics called computable economics, which I practice, has anything to do with these kinds of absurd claims. This caveat is added here to circumvent silly and aggressive assertions, as in [32], on the former, and even more ignorant observations by several of my senior colleagues in the department of economics at the University of Trento.

"On February 25, 1991, a Patriot missile battery assigned to protect a military installation at Dahrahn, Saudi Arabia, failed to intercept a Scud missile, and the malfunction was blamed on *an error in computer arithmetic*. ... In combination with other peculiarities of the control software, the inaccuracy caused *a miscalculation of almost 700 meters* in the predicted position of the incoming missile. *Twenty-eight soldiers died*."
ibid, p.484; italics added.

What was this tragic 'error in computer arithmetic'? It is simply due to the fact the binary fraction for the decimal fraction $10^{-1} = 0.1$ is *not terminating*:

$$10^{-1} = (0.1)_{10} = (.0001100110011...)_2 = (0\ 0011\ 0011\ 0011\ ....)_2 \tag{19}$$

In other words, the decimal fraction, in its binary notation, cycles and is non-terminating and will have to be truncated with unpredictable consequences, unless a serious approximation analysis is included in the software which truncates automatically for some predetermined instruction. But there is another alternative, in the case of pitfalls due to the discrete and finite nature of the digital computer and its arithmetic. This alternative would be to use *Interval Analysis*, where an '*interval of real numbers is treated as a new kind of number, represented by a pair of real numbers, namely its right and left end points'* ([78], p.vii; italics added). Had such numbers been used in the software that was built into the operation of the control software referred to above, the error would have been eliminated.

But there are, in fact, (*at least*) six ways of mathematical theorising where a computational basis is naturally embedded:

- Varieties of *Constructive Mathematics* and *Constructive Analysis* ([18])
- *Computability Theory* and *Varieties of Computable Analysis* ([22], [123])
- *Interval Analysis* (*Russian Constructivism,* cf., [1],[78], [79])
- *Real Computation* (the 'Smale School', cf., [12])
- Smooth (Infinitesimal) Analysis (cf., [8])
- *Numerical Analysis* (cf., [12], [104])

I have, in a series of writings over the past decade (see, in particular, the *Introduction* to [122], many of the essays in [119] and [118]), and more, reflected on the nuanced differences in these mathematical theories, and their applications in economics. The interested reader is referred to them for more detailed information on these mathematical theories, their alternative logical foundations, the nature of their algorithmic underpinnings, and much else – for example, in particular, on the role (or non-role, as the case may be) of the Church-Turing Thesis in the different theories. If economic theorising eschews an underpinning in one or another of these mathematical theories, the computational claims and, hence, the feasibility and meaning of *simulation* by machine computation to explore their non-analytical aspects borders on the absurd (in the noble sense of **The Theatre of the Absurd** - *pace* Esslin).

I have argued that economic relationships, whether micro or macro, whether in game theoretic or IO modes, whether in finance or interindustrial analysis, should be formulated as Diophantine equations. Such equations, or systems of equations, are rarely algorithmically solvable, *in general*, without extensive simulational experiments which lead to new theretical insights, encapsualted in innovative conceptual analysis. Essentially, the case I am putting forward is that the framework developed for studying and resolving *Hilbert's Tenth Problem* should be the paradigmatic one for any research field in economics, whose theoretical basis is underpinned by digital machine computation Consider, for example, the following three apparently 'simple' Diophantine equations ([90], p. 344), and ask the seemingly straightforward question whether they have solutions in integers:

$$X^3 + Y^3 + Z^3 = 29 \tag{20}$$

$$X^3 + Y^3 + Z^3 = 30 \tag{21}$$

$$X^3 + Y^3 + Z^3 = 33 \tag{22}$$

Todate, we know that the answer is in the affirmative for (21), and for (22), that the answer is 'yes' is known only since 1999 (the solution is: -283059965, -2218888517, 22204229320). However, (23) is still to be 'cracked'! Of course, in a strictly economic context, interpreting the right hand side as the available 'budget' for spending and the left hand side as the allocation of spending on three items – without bringing into play the motivational base for the alternative choices – it is clear one seeks solutions that are not only integer valued, but also non-negative. One must ask, in this context, why the mathematisation of eocnomics, which claimed to go beyond the 'Walrasian' method of simply (sic!) counting equations and variables to ensure consistency and 'meaningful' solutions, did not go further than considering non-negative real values for economic variables? I have my own answers to this quasi-hypothetical question, but this is not the place to express and attempt to justify it.

In any case, continuing the theme of advocating a Diophantine approach to economic theorising and modelling, it may well be apposite, at this point – and given many of the themes discussed above, particularly in section 2, I might as well state the Diophantine conundrums of dynamics, from the point of view of computability, and hence, obviously, the importance of simulation to help resolve some, at least of the 'conundrums'. Indeed, to take up the subject discussed above, the Peano Existence Theorem for the IVP of ODE's, one of the frontier research results in applied recursion theory is the following, (see, [74], chapter 9) :

Theorem 18 *There is no* effective method *for determining, for an* arbitrary *system of differential equations of the form,*

$$\begin{aligned} P_1\left(x, \Xi_1\left(x\right), ..., \Xi_k\left(x\right), \Xi_1'\left(x\right)\right) &= 0 \\ &.... \\ &.... \\ P_k\left(x, \Xi_1\left(x\right), ..., \Xi_k\left(x\right), \Xi_k'\left(x\right)\right) &= 0 \end{aligned} \tag{23}$$

where $P_1, ..., P_k$ *are polynomials with integer coefficients, whether the system has a solution on the interval* $[0, 1]$.

This is just one representative result, in an important applied domain, derived using a uniform method of proof.

Essentially this means one has to resort to what I have called the Zabusky Precept to breach the algorithmic walls and make conceptual advances in economics in the Diophantine mode. Nothing less than simulation by machine computation of Diophantine dynamics should be the research program of economics.

Richard Stone was prescient, as he always had the penchant to be, when dealing with issues that had to do with computations, simulations, numerical collation of statistical data and quantitative modelling:

> "Our approach is quantitative because economic life is largely concerned with quantities. We use computers because they are the best means that exist for answering the questions we ask. *It is our responsibility to* formulate the questions and *get together the data which the computer needs to answer them.*"
>
> Richard Stone, *Foreword* to [101], p. viii; italics added.

If we supplment this admonishment to the empirically oriented economic theorist, with David Deutsch's equally important – and eminently rigorous – insight, keeping in mind the Zabusky Precept, we – as economists – are well on the way to an empirically implementable algorithmic research program encompassing the triad of computation, simulation and dynamics indissolubly:

> "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 19 *"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."*

Much of my discussion above has had as a backdrop the precise theory of computation, which is underpinned by the Church-Turing thesis. But I have also consciously adopted – although, I hasten to add, only *pro tempore*[53] – Deutsch's important extensions, all of which seem to be consistent with the stand taken also by Gandy ([40]), on the physical principles that will have to be the basis on which the Church-Turing thesis is interpreted in computability theory. Even though I worked with an informal – but, hopefully, precise – definition of 'simulation', my arguments above have been made with the precise definitions of Deutsch in mind. It is, therefore, just as well, I state them precisely, at this concluding stage.

It is my 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 – with category theory in the wings to help us dispense with set theory altogether.

From my 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'.

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;

[53] This is partly because I am *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 I 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 [110].

most of the models emanating from work in economic theory belong to *The Museum of Unworkable Devices*[54] – 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.

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.

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. I have never found any advanced undergraduate or graduate student of reasonable maturity to have had any difficulty whatsoever with understanding the case I 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*' ([106]) – 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 essay, *a paen* (I hope) to the triad of computation, simulation and dynamics in their epistemological settings, remembering the thoughts of two of the giants of 20th century mathematics and philosophy, David Hilbert and Ludwig Wittgenstein:

> Hilbert, [55], (p. 191): 'No one shall drive us out of the paradise which Cantor has created for us.'
>
> Wittgenstein, [125], (p.103): 'I would say, "I wouldn't dream

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

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." '

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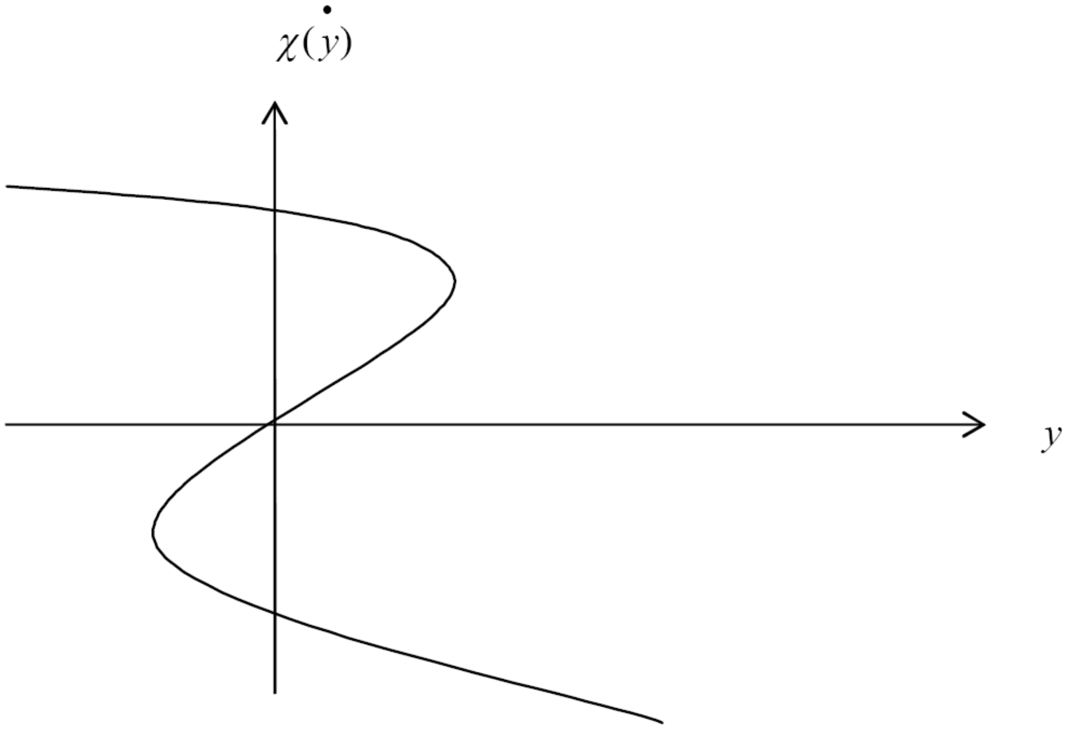


Figure 1: Cubic 'Characteristic'

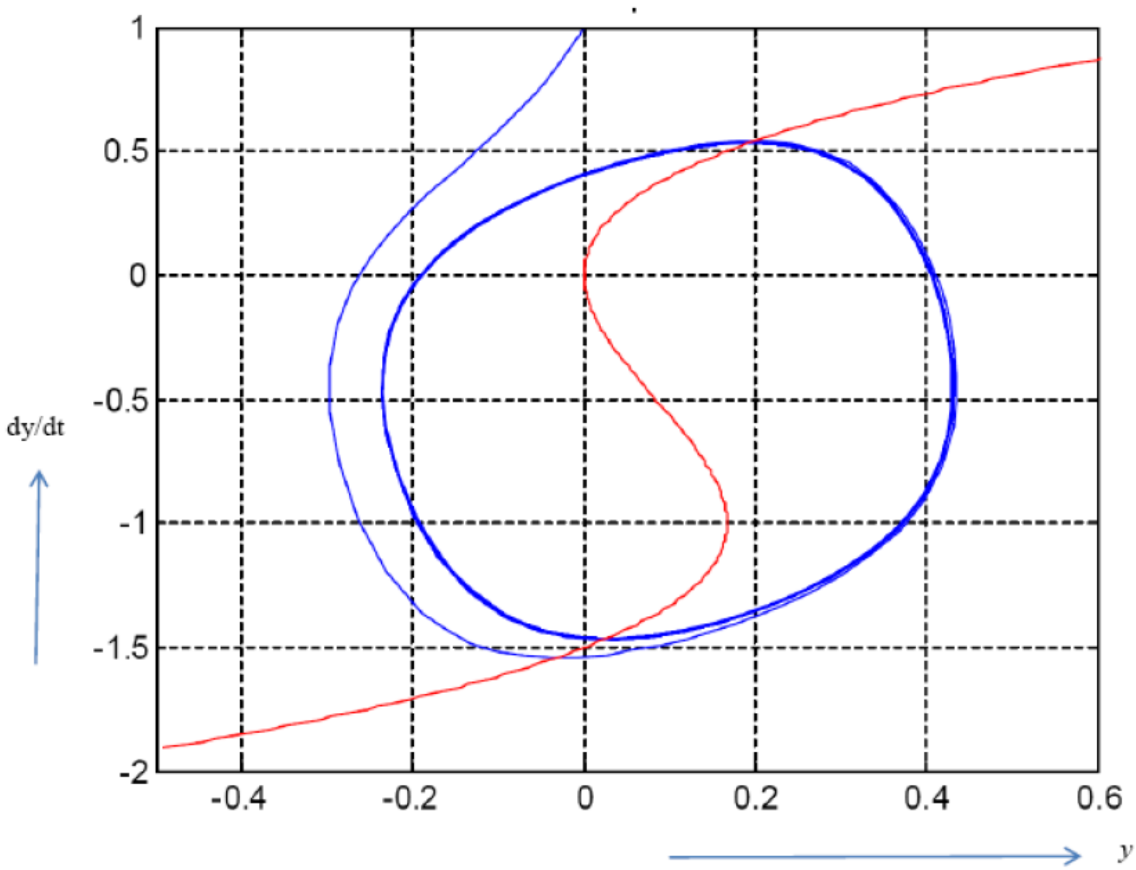


Figure 2

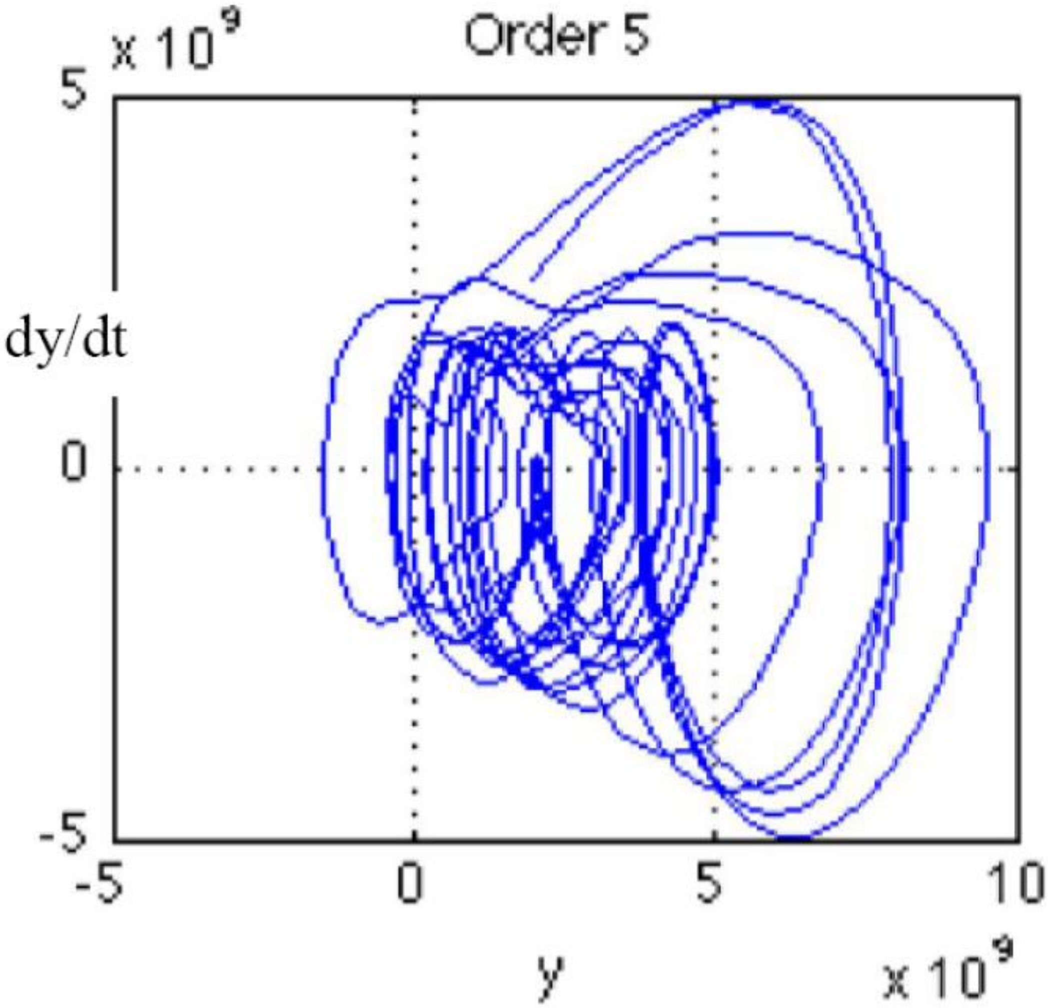
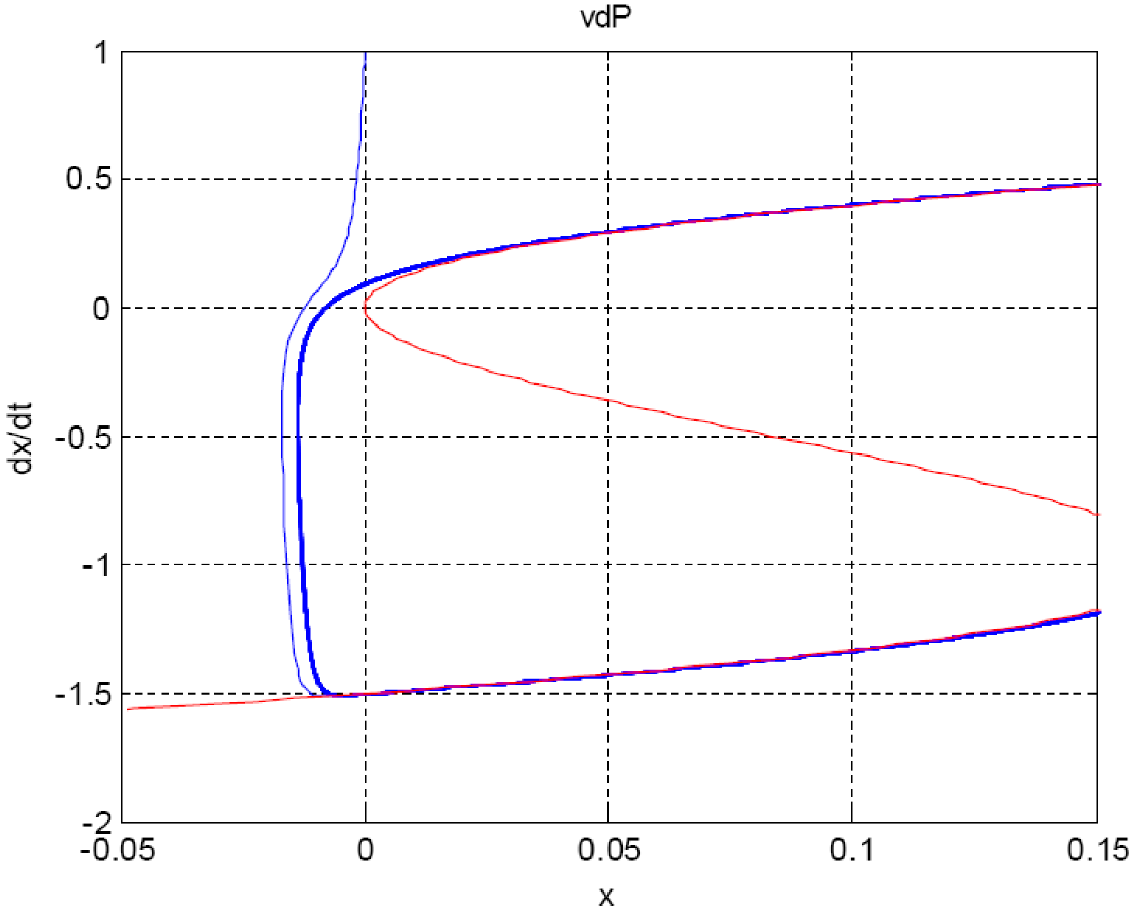


Figure 3



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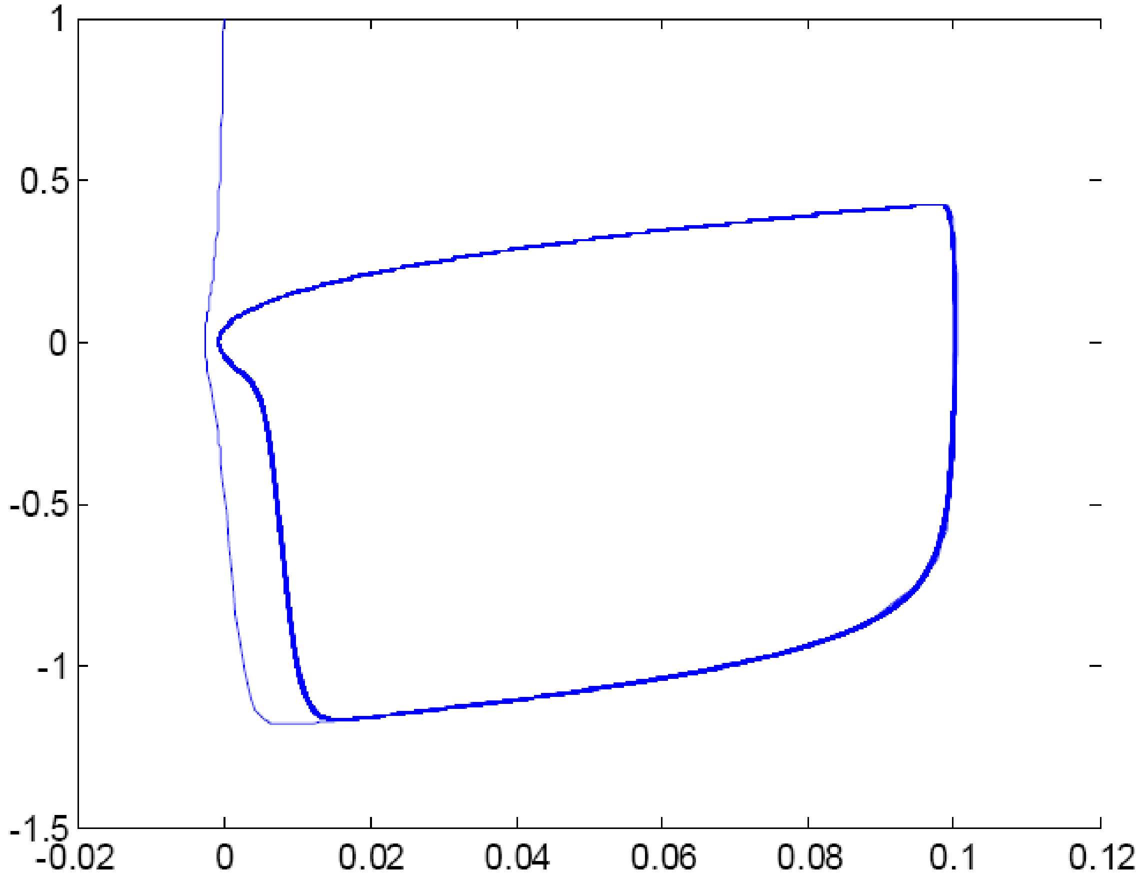


Figure 5

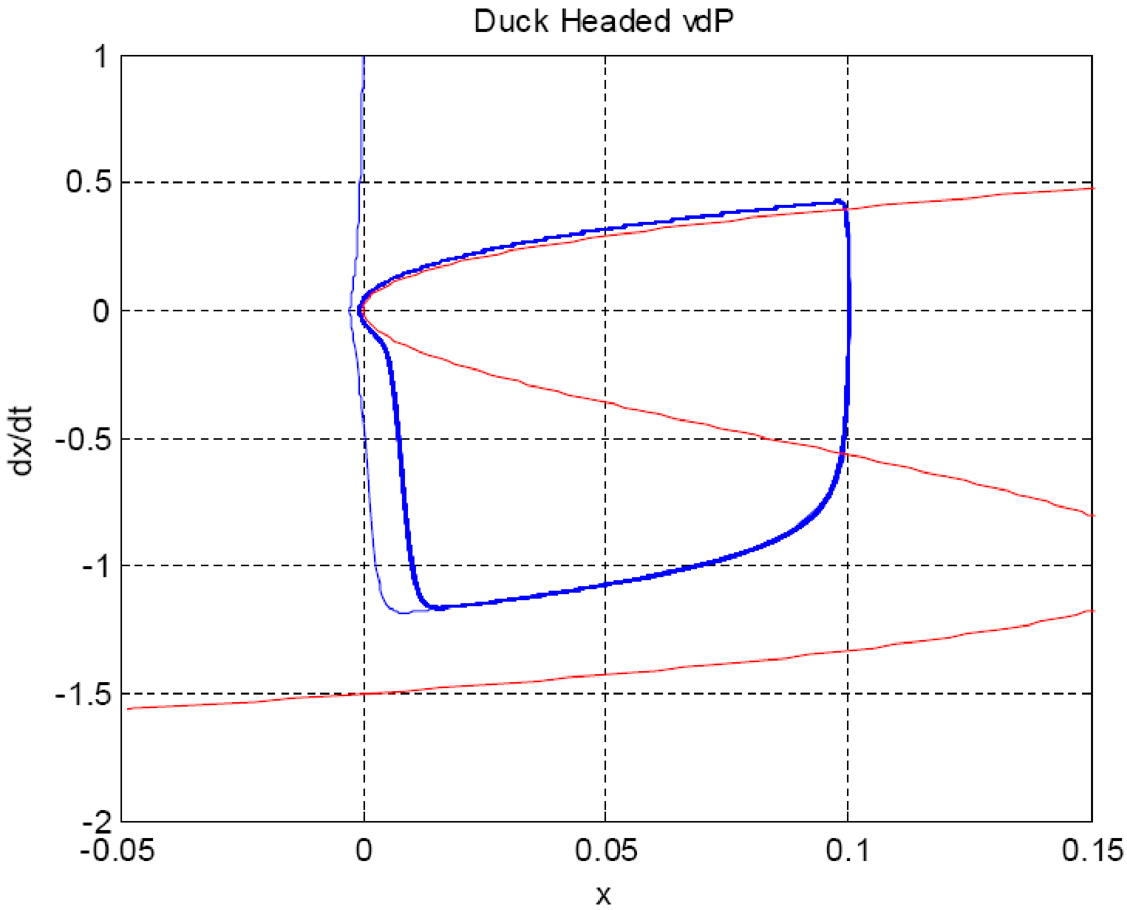
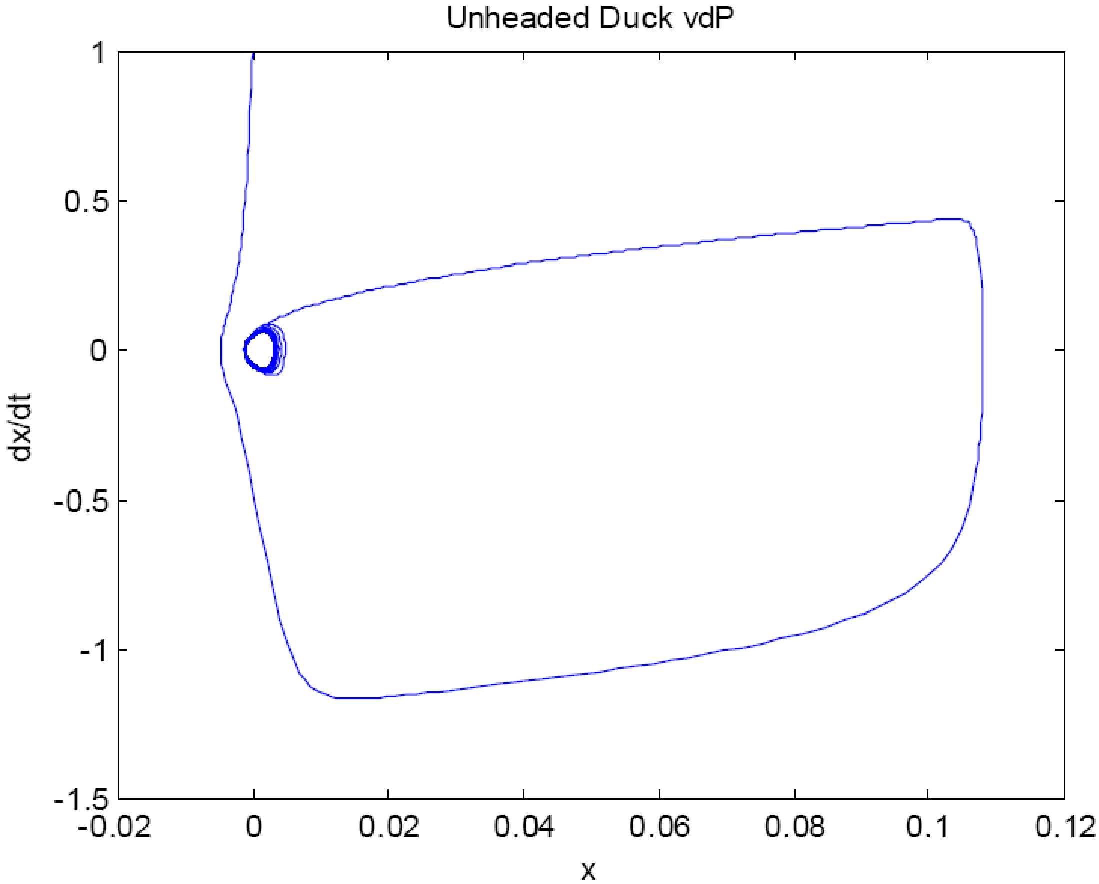


Figure 6


Unheaded Duck vdP
dx/dt
x
1
0.5
0
-0.5
-1
-1.5
-0.02
0
0.02
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0.1
0.12

Figure 7

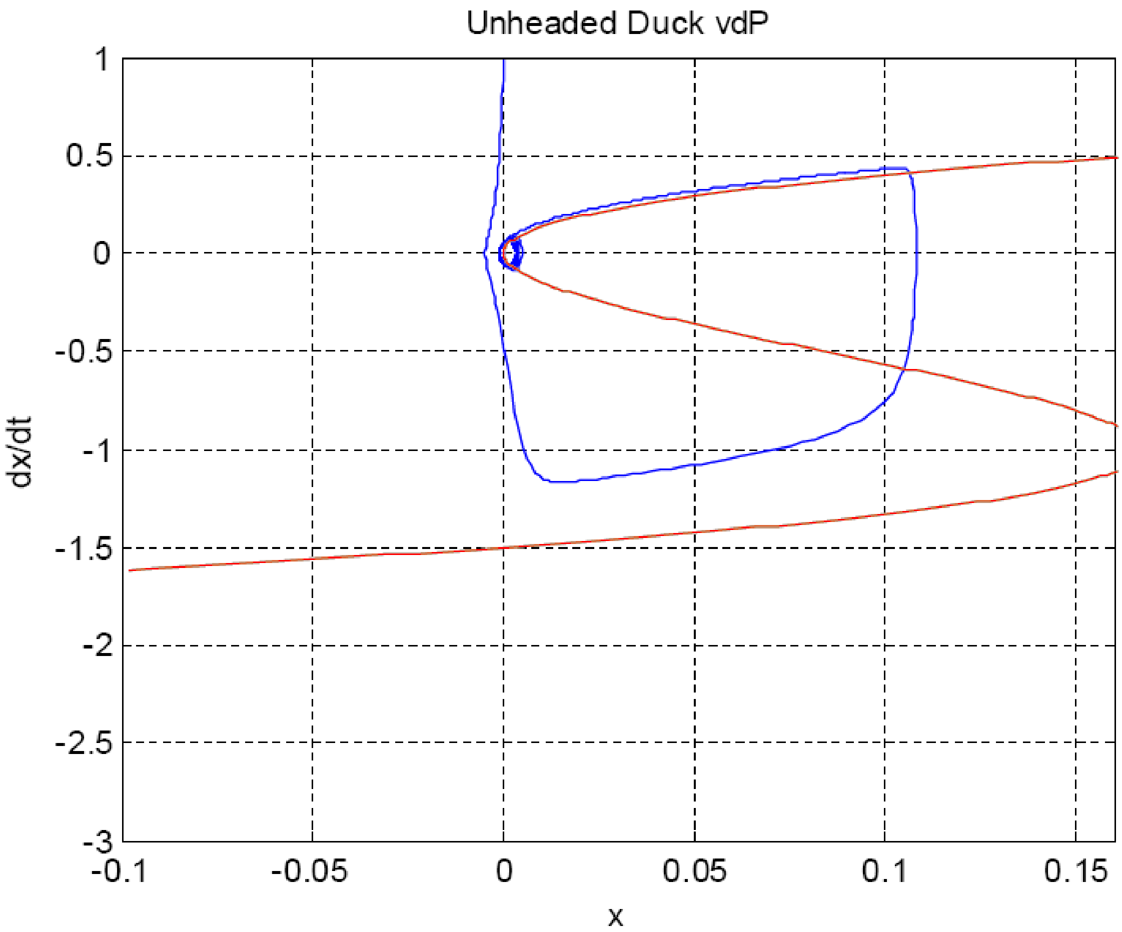

Figure 8