UNIVERSITA' DEGLI STUDI DI TRENTO - DIPARTIMENTO DI ECONOMIA

ESSAYS ON COMPUTABLE ECONOMICS, METHODOLOGY AND THE PHILOSOPHY OF SCIENCE

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Essays on Computable Economics, Methodology and the Philosophy of Science

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Dedicated to my teachers: Ryoichiro Kawai, Björn Thalberg and the late Richard Goodwin.

The author is greatly indebted to Tom Boylan, Bob Clower, Duncan Foley, Nico Garrido and John McCall for advice, inspiration, instruction and help in preparing these essays. The remaining errors, infelicities and omissions are, alas, his own responsibility.

Abstract. These essays tackle standard problems in mathematical economics, macroeconomics, methodology and a particular aspect of Popperian Philosophy of Science from the point of view of classical recursion theory and constructive mathematics.

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Preface

The first two and the fourth and fifth essays were written in an intensive period of about four or five weeks, this Summer; the last one, conceived last Autumn and a first draft presented at the Popper Centennial Conference held at NUI Galway in September 2002, was completed this Spring. A first draft of the third essay was completed in the late Spring of last year. The revised version was completed in the past few days. These essays were not written with the intention of collecting them as one unit, within the same covers. Departmental exigencies and logistical priorities and rules, have forced me to adopt this format. Perhaps they are better presented this way, in any case, although this is not the format I would have chosen. I would have preferred to have them released as individual discussion papers. However, there is, of course, an underlying theme that unites them. On the other hand, I have not attempted to revise any of them to eliminate minor duplications or insert coherent cross-references and other stylisitc devices that mark a well collated and collected set of essays

The unifying, underlying, theme in them is the recursion theoretic and constructive analytic backdrop. Close, now, to almost forty years ago, I was taught, by example and patience, by Professor Ryoichiro Kawai, the meaning of *proof* in a mathematical setting. It was during special lectures on Linear Algebra, during my undergraduate days at Kyoto University, in 1966. As an undergraduate in the Faculty of Engineering, but specialising in Precision Mechanics, I am not sure whether by accident or predilection, I ended up by choosing most of the applied mathematics options. The mathematical training from that background was, mostly, about *methods of solutions* rather than proof of propositions in an axiomatic framework. However, Professor Kawai, voluntarily, gave me some special lectures and tuition in the more 'rigorous' aspects of pure mathematics and some of the precepts I learned from him must have remained etched in my memory cells.

After graduation at Kyoto I entered the University of Lund to continue postgraduate studies in economics and was fortunate to come under the gentle and wise influence of Professor Björn Thalberg. The kind of macrodynamics he was working on at that time, stabilisation policies in the Phillips-Goodwin tradition that had even been popularised in Roy Allen's textbooks of the time, emphasised the kind of applied mathematics I had learned at Kyoto. After obtaining my Master's degree in economics at Lund, in January 1973, I arrived at Cambridge in October of that year to work under Richard Goodwin for a Ph.D[1]. The wise and enlightened way he supervised me emphasised the same applied mathematics tradition in which I had been trained at Kyoto and which Thalberg reinforced in Lund.

During my first months in Cambridge I attended some of Frank Hahn's advanced lectures and as part of the background tried to read whatever I could of 'Arrow-Hahn'.

[1]My 'official' supervisor for the Michaelmas and Lent terms of 1973 was Nicky Kaldor, but I betgan seeing Richard Goodwin almost from the outset. Björn Thalberg, during the year he had himself spent in Cambridge, at the DAE, had considered himself a pupil of Goodwin and had, in fact, written his own doctoral dissertation on *A Trade Cycle Analysis: Extensions of the Goodwin Model*. Thus my journey and odyssey in Cambridge was destined to be Goodwinian rather that Kaldorian!

I almost did not get beyond p.ix of the Preface to that book because I encountered the following phrase which stumped me quite completely: '...our methods of proof are in a number of instances quite different [from those in Debreu's *Theory of Value*]'. I did not have the slightest idea that there were 'different methods of proof' and had never been taught that such things were relevant in economics. In complete innocence I bought a copy of Gaisi Takeuti's book on 'Proof Theory' to try to educate myself on such things but, of course, got nowhere, at that time.

In 1974 Axel Leijonhufvud came to Cambridge to give the *Marshall Lectures* and opened my Cambridge blinkered eyes to the fascinating world of Herbert Simon's behavioural economics. It was not that Leijonhufvud's *Marshall Lecture* claimed to be in the tradition of Simon's kind of behavioural economics, but the underlying theme of adaptive economic behaviour and the few allusions to Simon's work during the delivered lecture[2] was sufficient to send a Goodwin-inspired student in search of the relevant references. The nonlinear dynamics I was learning at Goodwin's feet seemed entirely appropriate for the adaptive behaviour underlying the Marshallian themes in Leijonhufvud's *Marshall Lecture*. The behavioural economics that Simon had broached and developed, and the problem-solving context in which such economics was embedded, opened my eyes to a new world of mathematical economics and applied mathematics.

These new visions were reinforced when, immediately after my student years at Cambridge, I was fortunate to obtain a research post in the Department of Computing and Control at Imperial College where I met and worked with Berc Rustem. He educated me in a different kind of mathematical economics, that which emphasised computation, computational complexity, control and filtering. Although we were, for all official purposes, colleagues, in actual fact it was more a teacher-pupil relation where I was, of course, the pupil.

These were the origins of the journeys that took me, first to the philosophy of mathematics, then to the foundations of mathematics and metamathematics and, in between, to constructive, computable and non-standard mathematics and their relevances in economic theory. Thirty years have elapsed since that encounter with the phrase in 'Arrow-Hahn'. I feel, now, finally, I am ready to put down in writing the ruminations and reflections of three decades of work and, eventually, to try to forge an alternative Mathematical Economics, one that is more faithful to the classical quantitative traditions of a subject that began as 'Political Arithmetic'. But the mind is not as agile as it was some years ago and the body is even less robust and I am not sure I have sufficient stamina left to complete the task. Hence the intensive activities of the past few weeks and months to try to put together some of the ideas in a coherent and unified format.

These loosely collected essays are a first step in a path which, I hope, will see some of the final aims realised. The forced format of a loosely collected set of essays

[2] I did not read the manuscript of that lecture till about a decade and a half later, in 1987, when entirely due to Leijonhufvud's efforts I was invited to be a Visiting Professor at UCLA.

has, on the other hand, forced me to rethink and reflect on my work in the three areas that are indicated in the somewhat contrived title for this collection. As a result, I expect, this will be the 'first edition' of an attempt, with hindsight, to collect, collate and present my work in the general areas of computable economics, methodology and the philosophy of science, in a more organised way, in the near future.

CHAPTER 1

Analogue Computing – Reviving the Icarus Tradition in Economic Dynamics

1. Introduction

"The [hydraulic] mechanism just described is the physical analogue of the ideal economic market. The elements which contribute to the determination of prices are represented each with its appropriate rôle and open to the scrutiny of the eye. We are thus enabled not only to obtain a clear and analytical *picture* of the interdependence of the many elements in the causation of prices, but also to employ the mechanism as an instrument of investigation and by it, study some complicated variations which could scarcely be successfully followed without its aid."

([**7**], p.44, italics in original)

"Economic phenomena may often be found to be representable by *analogous* electrical circuits. Electrical *analogue computing techniques* may therefore have broad applications in the study of economic problems. This paper .... presents a brief survey of suitable problems iin theoretical ecoonomics, and discusses electrical representations in terms of present and imminent developments in *analogue computing techniques.*"

([**28**],p.557, italics added)

Analogue computing techniques in economics had the proverbial still birth. There was a flurry of activities in the late 40s and early 50s, at the hands of A.W.H. (Bill) Phillips, Richard .M.Goodwin, Herbert.A. Simon, Robert H.Strotz, Otto Smith, Arnold Tustin, Roy Allen, Oscar Lange and a few others. Phillips built his famous *Moniac*[1] hydraulic national income machine at the end of the 40s and it was used at many Universities - and even at the Central Bank of Guatemala - for teaching purposes and even as late as the early 70s Richard Goodwin, at Cambridge University, taught us elementary principles of coupled market dynamics using such a machine. Strotz and

[1]I have not succeeded in finding out whether this word is an acronym for something or whether it was chosen to rhyme *ENIAC* (**E**lectronic **N**umerical **I**ntegrator *a*nd **C**omputer), itself a name linking the embryonic digital computer with the existing functions of analogue computing devices of the time.

his associates, at Northwestern University, built electro-analogue machines to study inventory dynamics and nonlinear business cycle theories of the Hicks-Goodwin varieties. Otto Smith and R.M. Saunders, at the University of California at Berkeley, built an electro-analogue machine to study and simulate a Kalecki-type business cycle model. Roy Allen's successful textbooks on Macroeconomics and Mathematical Economics of the 50s - extending into the late 60s - contained pedagogical circuit devices modelling business cycle theories (cf:[**1**] especially chapter 9; and [**2**], especially chapter 18). Arnold Tustin's highly imaginative, but failed textbook attempt to familiarise economists with the use of servomechanism theory to model economic dynamics ([**32**]) and Oscar Lange's attractive,elementary, expository book with a similar purpose ([**16**]).

My purpose here is not to try to study the causes and consequences of the brief flurry of activities on analogue computing metaphors and attempts in economic dynamics[2]. I want to focus on a more restricted but also more diffused question: *why* is economic dynamics - particularly growth and cycle theories - modelled as continuous time ODEs[3] or as *Maps*, in short as *dynamical systems*, often *nonlinear*? In the former case, *how* are discretizations, numerical approximations, computations, simulations etc., exercises made consistent with the original continuous time analysis and results? To make the questions less diffused, I shall narrow the question to the following: given that economic theorists resort to continuous time modelling of dynamic issues in economics, particularly in macroeconomics but not exclusively so, how best can one facilitate the computing underpinnings of such models. At a minimum, such models, if they are to be implemented in a modern digital computer, need to be *discretized effectively* and *approximated numerically.* In the latter case, taking also into account the constraints imposed by the digital computer's own internal precision factors, both software-based and hardware-based. So far as I am aware, there is hardly any serious discussion of such issues in formal economic theorising, even where simulations are serious tools of analysis and not just number-crunching adventures. I do not know of a single study, in the context of economic dynamics, of trying to devise a theory of simulations. Moreover, I think I am correct in stating, even categorically, that there is not a single study of any continuous time model of economic dynamics, at any level, where its numerical investigation has gone *pari passu* with a study of the numerical method itself as a dynamical system so that a consistency between the two dynamics is achieved and spurious solution paths are formally and systematically studied and avoided[4].

[2] At the hands of the ever imaginative Irving Fisher also economic statics, as far back as the early 1890s. One can, with definitive substantiation, also include Jevons, Walras, Edgeworth and Pareto among these pioneers of the analogue computing metaphor for and in economics. However, historical priorities and delineation of a narrative history is not the focus of this paper.

[3] *O*rdinary *D*ifferential *E*quations.

[4] I am sure there will be isolated exceptions. As always, there is the notable exception of Paul Samuelson (cf., [**24**] in particular, pp. 45-6). Also, the fashions of the times that brought chaos to the forefront, even in economics, did focus on issues of numerical sensitivity in approximations,

Serious discussion of these issues, in formal and theoretical ways, requires a simultaneous study of dynamical system theory, numerical analysis and recursion theory. Economic theorists and economic curricula are too busy and overloaded with so much else that is of 'burning' importance, particularly of a short-term nature, that it is too much to expect any immediate reorientation of interests in incorporating such a puritanical study of dynamical systems and their solutions, simulations and experimentations consistently, by underpinning them in theories of numerical analysis and recursion theory, simultaneously[5]. I do not expect, therefore, much progress in this direction, at least not in the near future, even given the ubiquity of the digital computer as a tool and a concept in economic theory, applied economics and experimental economics.

Therefore, I aim to capitulate! I shall ask, and try to answer, in this paper, a more direct question, taking as given the fact that economic practice relies on continuous time modelling and theorising and this practice is untempered by numerical analytic and recursion theoretic foundations. The question, therefore, is the following: what kind of computers can avoid these issues and, hence, take the economic theorist's almost reckless need to solve formally unsolvable systems of nonlinear equations - i.e., study by simulations - and experiment with them? Clearly, one needs to avoid the use of the digital computer - an advice that will not be taken seriously even by the most good-natured and the well-disposed, to put it mildly. But suppose we can role back that eternal villain, time, and go back to an age when the digital and the analogue computers were almost on par, say the late 40s, and wonder, counter-factually, whether it would have been possible for both alternative paradigms to have evolved parallely Had they done so, what would have been the minimal necessary conditions for the anlogue devices to have kept pace with the spectacular developments in the use and dissemination of the digital computer? Obviously, hardware questions would dominate the answers: the severe difficulties of precision mechanics involved in the construction of accurate analogue devies contrasted with the developments in electrical and, more importantly, electronic engineering which were almost decisive in the evental demise of analogue computing devices. But this is not a complete story. One could envisage a possible world where the analogue devices were also based on the burgeoing electronic basics of the basic architecture.

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discretizations, the precision arithmetic underpinning the computer and its software and related issues, particularly because of a defining characteristic of such nonlinear models: *sensitive dependence on initial conditions.*

[5]Recently I received an ironic note from an old friend of mine, Pierre Malgrange, with the following remark: "By the way, may I take the liberty to [inform] you about the next conference of the Society for Computational Economics (*a society computing everything even what is uncomputable*) in June, 27-29 2002 in Aix en Provence, France, of which I am in charge together with [another of] our old friend[s]. This famous 'Malgrangian irony' masks a profound truth: economic computation has hardly ever paid serious attention to recursion theory, except in so far as computational complexity theory has played important roles in optiimization and programming models in economics.

Another part of the story is, of course, the fact that the practical, engineering, development of the digital computer was preceded by theoretical developments in its mathematical foundations. Theories of what is and what is not computable by a digital computer; theories of the complexity of computations by a digital computer; theories of the architecture of the digital computer; theories of the language of the digital computer; and so on, often preceded the actual and spectacular engineering development of the hardware repositories of these theories. Indeed, the mathematics of the computer, as recursion theory, came into being as a result of debates on the foundations and philosophy of mathematics, prior to its development as a physically realized device, at least in its modern versions. Almost none of this happened in the case of the analogue computer.

However, there has been a resurgence of interest in the mathematical foundations of the analogue computer. Part of this resurgence has to do with the problem of computation over the *reals* - the paradigatic case of continuous time computation involved in the study of dynamical systems Various theories of real computation have been appearing, but not all of them related to the analogue computer as such. Some of these theories are about developing a mathematical theory of computation over the reals, of one sort or another, but not abandoning the paradigm of the digital computer for their numerical realisations. Keeping in mind the fact that there is a noble, if shrt-lived and narrow, tradition of analogue computing in economics (as mentioned and referred to above), my focus in this paper will be on discussing the mathematical foundations of analogue computing. In other words, suppose the economist insists, even consciously, of ignoring the mathematics of the digital computer and the numerical anlysis that is a handmaiden to it, as she has done forever, and simply wants to use a computer, for whatever purpose, what are the mathematical limitations and implications of such an attitude? What exactly is computable by an analogue device? How complex are computations by analogue devices? Indeed, is there a mathematical theory of the analogue computer paralleling the recursion theory of the digital computer and, if there is such a thing, why and how much should be known to an economist?

To answer these questions in a very general way, within the limiited scope and space of this paper, I shall structure the discussion in the following way. In the next section I shall present a few examples of the problems inherent in ignoring the interaction between dynamical systems, numerical analysis and recursion theory - simply as a formal stage from which to justify a by-passing of these issues by the use of an analogue computing device. Then, in §3, I define, formally, the kind of analogue computing device for which a mathenatical foundation will be sought. In defining it I shall keep in mind the tradition of economic dynamics and computation as well as the Turing tradition of recursion theory. The mathematical underpinnings of the defined device will be investigated, more or less along the lines of classical recursion theory: computaibility, Church-Turing thesis, Universal Turing Machines and Universal computations, fix point theory, etc, but, of course, over the reals rather than over the integers, rationals or the natural numbers. Some parallels emerge and, perhaps, there is a glimpse of an emerging unified theory, but much remains to be done. Finally, in a concluding §4, I suggest that there can, in fact, be an Ariadne's Thread, or the wings of Daedalus, guiding the perplexed through the Labyriinth that is analogue computing, towards an exit where the reckless economist, rather like the rash Icarus, can feel reasonably free to continue normal practice.

2. Some Numerical and Computable Conundrums

> "More specifically, do computer trajectories 'correspond' to actual trajectories of the system under study? The answer is sometimes no. In other words, there is no guarantee that there exists a true trajectory that stays near a given computer-generated numerical trajectory. ....
>
> Therefore, the use of an ODE solver on a finite-precision computer to approximate a trajectory of a .... dynamical system leads to a fundamental paradox. .... Under what conditions will the computed trajectory be close to a true trajectory of the model?"
>
> [**25**], p.961.

Consider the widely used Verhulst-Pearl model of logisitc growth, one of the simplest nonlinear differential equations used in economic and population dynamics, mathematical biology, ecology, etc:

$$\dot{N} = rN\left(1 - \frac{N}{K}\right), \; r > 0, K > 0 \tag{2.1}$$

For a given initial condition of the population variable, N, at time $t = 0$, say N_0, the solution for (1) is:

$$N(t) = \frac{N_0 K e^{rt}}{[K + N_0 (e^{rt} - 1)]}, \quad \longrightarrow K \qquad \text{as} \qquad t \longrightarrow \infty \tag{2.2}$$

In this model, usually, $N(t)$ signifies a population level at time to and, therefore, N_0 its level at some base or initial point of reference. Of course, it is understood that this variable is defined over integer or natural number values. However, neither the *per capita birth rate*, $r\left(1 - \frac{N}{K}\right)$, nor K, generally known as the carrying capacity of the environment, at least in population and ecology models, are thus constrained. Hence, it is natural that continuous time models have been heavily utilised in these contexts. On the other hand, a naive analogy might suggest that a discrete time equivalence to the above model is:

$$N_{t=1} = rN_t\left(1 - \frac{N_t}{K}\right), \; r > 0, K > 0 \tag{2.3}$$

By now, particularly after all the hype about *chaos,* almost all and sundry know that there is a fundamental qualitative difference between the dynamical behaviour of (1) and (3). So, if one is to study, for empirical or theoretical reasons, (1) by means of a digital computer, then a 'proper' discretisation must, first, be achieved. What, however, does 'proper' mean, in this context? At an intuitive level one would expect that the asymptotic values of the discretized system should be equal, but also that the solution points of the discretised system should lie on the solution curves of (1). Consider, for simplicity, the normalized version of (1):

$$\dot{N} = N(1-N) = N - N^2 \tag{2.4}$$

The *discretisation* that appears, on the surface, to suggest the equivalent *nonlinear difference* equation to the *nonlinear differential* equation (1) is:

$$\Delta N = N_{t+1} - N_t = \left(N_t - N_t^2\right)\Delta h_t \tag{2.5}$$

But, of course, like the naiveté that suggested an analogy between (1) and (3), there are immense pitfalls in this analogy, too[6]. Most crucially, (4) has no closed form solution. In fact it is known that the following discretisation, which has a closed form solution, is equivalent in the above suggested intuitive senses to (1):

$$N_{t+1} - N_t = \left(N_t - N_t N_{t+1}\right)\left(e^h - 1\right) \tag{2.6}$$

Its closed form solution is:

$$N_t = \left[1 - \left(1 - N_0^{-1}\right)e^{-th}\right]^{-1} \tag{2.7}$$

The discrete solutions of (5), $\forall h > 0$, lie on the logistic curve and the qualitative behaviour of its asmptotic dynamics are similar to those of the solutions to (1).

On the other hand, the follwoing discretisation also generates solutions of the logistic curve $\forall h < 1$:

$$N_t = \left[1 - \left(1 - N_0^{-1}\right)(1-h)^t\right]^{-1} \tag{2.8}$$

[6] I am reminded of the wise warning suggested by my old master, Richard Goodwin, half a century ago:

> Combining the difficulties of difference equations with those of non-linear theory, we get an animal of a ferocious character and it is wise not to place too much confidence in our conclusions as to behaviour.

[**9**], footnote 6, p.319. To this we can add the further difficulties arising from the constraints imposed by the finite precision arithmetics of the digital computer, should we decide to study and analyse these equations with such a device.

But this discretization generates oscillatory solutions $\forall h \geqslant 1$, which are, of course, spurious as far as the dynamics of the solution of (1) is concerned.

From the above discussion of the extraordinary simple and well-understood dynamics of (1) and its discretization we get a handle on a possible general theory in terms of local bifurcations with respect to the parameter h. I shall not go into this fascinating and immensely valuable theory, which has only recently gathered considerable theoretical momentum. I refer the that eternally vigilant mythical ceature, the interested reader, to the very readable items (cf. [**12**], [**14**] and [**29**]).

Quite apart from allof the above difficulties, arising from the inerplay between the continuous time dynamics of intuitively simple nonlinear differential equations and numerical methods used to approximate them, there is also the added consideration of the finite precision of the digital computer, to which recourse must be had for a study of almost all interesting such equations.

If such a simple nonlinear dynamical differential equation as that of Verhulst-Pearl, routinely used not just in elementary academic exercises but also heavily used in momentous policy deates and contexts can give rise to such complexities of discretisations, what about the use of higher dimensional nonlinear differential equations or more complex nonlinearities? Does any economist take the elementary precaution of checking for an 'equivalent discretisation', before feeding readily available numbers to implement a simulation, investigate a solution or test a conjecture? After years of fruitless efforts devoted to try to make economists take numerical analysis and recursion theory seriously, at a routine and elementary level, in the study and use of continous time dynamical systems, and having had absolutely no success whatsoever, I now suggest a differnet strategy to overcome these infelicities: *analogue computation*. By employing this device, one circumvents the need to discretize, the conundrums of the finite precision of the digital computer, the need to constrain the domain and range of definition of variables to $\mathbb{N}, \mathbb{Q}$ or $\mathbb{Z}$; the differential equation is fed directly, 'as is', into the analogue device for computaion, simulation, experiment or whatever. Let me illustrate the clarity and conceptual simplicity involved in this suggestion, anticipating the notation and some of the discussion in the next section.

3. Motivating a Return to the Icarus Tradition

> "We have said that the computable numbers are those whose decimals are calculable by *finite means*. ....
>
> We may *compare a man in the process of computing a real number to a machine* which is only capable of a finite number of conditions $q_1, q_2, ..., q_R$ which will be called '$m - configurations$'. The machine is supplied with a 'tape' (the analogue of paper) running through it, and divided into sections (called 'squares') each capable of bearing a

'symbol'. At any moment there is just one square, say the $r-th$, bearing the symbol $\zeta(r)$ which is 'in the machine'. We may call this square the 'scanned square'. The symbol on the scanned square may be called the 'scanned symbol'. The 'scanned symbol' is the only one of which the machine is, so to speak, 'directly aware'. However, by altering its $m-configuration$ the machine can effectively remember some of the symbols which it has 'seen' (scanned) previously. The possible behaviour of the machine at any moment is determined by the $m-configuration$ q_n and the scanned symbol $\zeta(r)$ .This pair $q_n, \zeta(r)$ will be called the 'configuration': thus the configuration determines the possible behaviour of the machine. In some of the configurations in which the scanned square is blank .... the machine writes down a new symbol on the scanned square: in other configurations it erases the scanned symbol. The machine may also change the square which is being scanned, but only by shifting it one place to right or left. In addition to any of these operations the $m-configuration$ may be changed. Some of the symbols written down will form the sequence of figures which is the decimal of the real number which is being composed. The others are just rough notes to 'assist the memory'. It will only be these rough notes which will be liable to erasure.

It is my contention that these operations include all those which are used in the computation of a number."

[**31**], pp.117-8; italics added.

Recall that the caveat 'finite means' is there as a reminder that Turing's construction was attempted as a means to answer the last of the three questions Hilbert had posed to the mathematical community in Bologna, at his famous 1927 address, to resolve, once and for all, Brouwer's challenges to the Formalists and the Logicists. The questions regarding *Completeness* and *Consistency* had been answered in the negative by Gödel a few years before Turing applied his considerable skills and fertile brain to tackling the last remaining problem: that of *Decidability* (by 'finite means'). This caveat reflects, also, another Hilbert 'problem', the 10th of his '23 Problems', from his 1900 Lecture to the International Mathematical Congress in Paris. There, the question was about what came to be known as the problem of 'effective calculability' or 'calculation' or 'computation' by 'finite means'. Thus the Turing Machine was conceived within the context of debates and paradoxes in the foundations of mathematics and not directly related to issues of technology or feasible engineering construction.

Furthermore, Turing also imposed constraints on the architecture of his Turing Machine that reflected *psychological* and *neurological* considerations in a manner that was innovative and, even, audacious. I shall not go further into those issues except recommend that the interested reader at least peruse the original Turing paper, which is still eminently readable and even enjoyably so. Thus, the assumption of 'finite state' was not only motivated by theoretical debates on the foundations of mathematics but also by considerations of the neurophysiological structure of the perceptive mechanism of the brain; that the machine could only scan a finite number of symbols reflected a similar consideration; that the alphabet or the symbols of the language for the machine to interpret or read was finite and its syntax determined also reflected the reality of natural language structures and formations, although there was an element of idealization in this regard; that the domain of definition was constrained to $\mathbb{N}$, $\mathbb{Z}$ or $\mathbb{Q}$, even though the aim was to define 'the process of computing, by finite means, a *real number*', reflected the standard practice of going from these domains, via Cauchy Sequences or Dedekind Cuts to the 'construction' of $\mathbb{R}$. The only real 'concession' to idealization was the assumption that the tape or the medium on which the language or symbols was written could, potentially, extend 'forever'.

None of this 'excess baggage' need worry the architect of an analogue computer! Taking, however, a cue from Alan Turing's characteristically perceptive thought experiment and intuitions in constructing what the Turing Machine, we can ask the following question: given that we want to solve ODEs, without going through the circumlocution of discretisations, numerical analysis, finite precision digital machines and so on, which intuitively acceptable *operations* exhaust the *process* of solving an ODE? If these operations can be identified to an intuitively acceptable degree, then the natural next step would be to ask: what kind of *machine* can implement such *operations* and *processes*? Answers to these two, or related, questions will furnish us with a concept, a thought experiment, of an *analogue machine* for the solution of ODEs. Like in the previous section the best way to get a handle on this question and potential answers is to consider the simplest example that can illustrate the mechanisms and principles that may be involved. Consider the linear, second order, differential equation that once formed the fountainhead of Keynesian, endogenous, macroeconomic theories of the cycle:

$$a\ddot{x} + b\dot{x} + kx = F \tag{3.1}$$

Solving, as in elementary textbook practice, for the second order term, $\ddot{x}$:

$$\ddot{x} = \frac{1}{a}F - \frac{k}{a}x - \frac{b}{a}\dot{x} \tag{3.2}$$

Integrating (10) gives the value for $\dot{x}$ to be replaced in the third term in the above equation; in turn, integrating $\dot{x}$, gives the value for x, and the system is 'solved'[7]. It

[7] This is the reason why analogue computing is sometimes referred to as resorting to 'bootstrap' methods. Recall Goodwin's perceptive observation, in this very Journal, more than half a century ago:

"A servomechanism regulates its behaviour by its own behaviour in the light of its stated object and therein lies the secret of its extraordinary finesse in performance. .... It is a matter of cnsiderable interest that Walras' conception of and term for dynamical adjustment - tâtonner, to grope, to feel one's way - is literally the same as that of modern servo theory." (cf.[**10**] ; italics added)

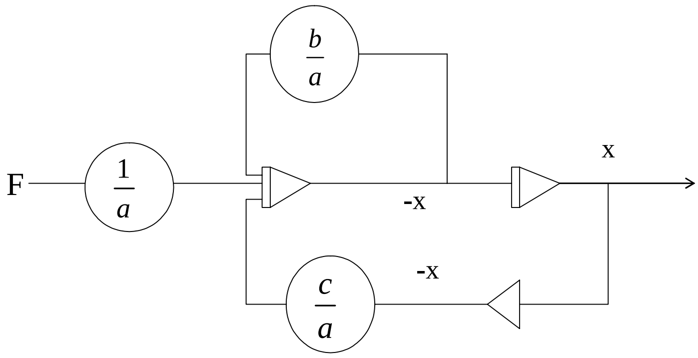

Fig. 1

FIGURE 1

is easy to see that just three kinds of mechanical elements have to be put together in the form of a machine to implement a solution for (9):

- A machine element that would *add* terms, denoted by a circle: ;
- An element that could *multiply* constants or variables by constants, denoted by an equilateral triangle: ;
- An element that could '*integrate*', in the formal mathematical sense, without resorting to sums and limiting processes, denoted by a 'funnell-like' symbol: ;

One adder, three multipliers and two integrators, connected in the following way, can solve[8] (9):

Note several distingushing features of this anlogue computing circuit diagram. First of all, there are no time-sequencing arrows, except as an indicator of the final output, the solution, because all the activity the summing, multiplication and

[8]One must add rules of interconnection such as each input is connected to at most one output, feasibility of feedback connections, and so on. But I shall leave this part to be understood intuitively and refer to some of the discussion in [**20**], pp9-11; observe, in particular, the important remark that (ibid, p.10, italics in the original):

> "*[F]eedback, which may be conceived of as a form of continuous recursion, is permitted.*"

integration, goes on *simultaneously*. Secondly, no approximations, limit processes of summation, etc are involved in the integrator; it is a natural physical operation, just like the operations and displays on the odometer in a motor car or the voltage meter reading in your home electricity supplier's measuring unit. Of course, there are the natural physical constraints imposed by the laws of physics and the limits of precision mechanics and engineering p something that is common to both digital and analogue computing devices, so long as *physical* realizations of mathematical formalisms are required.

In principle, any ODE can be solved using just these three kinds of machine elements linked appropriately because, using the formular for integrating by parts, a need for an element for differentiating products can be dispensed with. However, these machine elements must be supplemented by two other kinds of units to take into account the usual independent variable, time in most cases, and one more to keep track of the reals that are used in the adder and the multiplier. This is analogous to Turing's 'notes to assist the memory', but play a more indispensable role. Just as in Turing's case, one can, almost safely, conclude that 'these elements, appropriately connected, including 'bootstrapping' - i.e., with feedbacks - exhaust the necessary units for the solving of an ODE'. Accepting this conjecture pro tempore, in the same spirit in which one works within the Church-Turing Thesis in Classical Recursion Theory, a first definition of an analogue computer could go as follows:

DEFINITION 1. *A General Purpose Analogue Computer (GPAC) is machine made up of the elemental units: adders, multipliers and integrators, supplemented by auxiliary units to keep track of the independent variable and real numbers that are inputs to the machine process, that are interconnected, with necessary feedbacks between or within the elemental units to function simultaneously.*

EXAMPLE 1. *Consider the Rössler equation system which has occasionally been used in economic dynamics to model business cycles*[9]*:*

$$\frac{dx}{dt} = -\left(y + z\right)) \tag{3.3}$$

$$\frac{dy}{dt} = x + 0.2y \tag{3.4}$$

$$\frac{dz}{dt} = 0.2 + z\left(x - 5.7\right) \tag{3.5}$$

[9]Particularly by Richard Goodwin (cf. [**11**]). I could have chosen the more widely utilised van der Pol equation as an example at this point but a comprehensive 'electro-analog' investigation of it, as a business cycle model, was one of the pioneering analogue computing examples and there is no need for me to repeat that exercise at this level of generality (cf. [**18**])

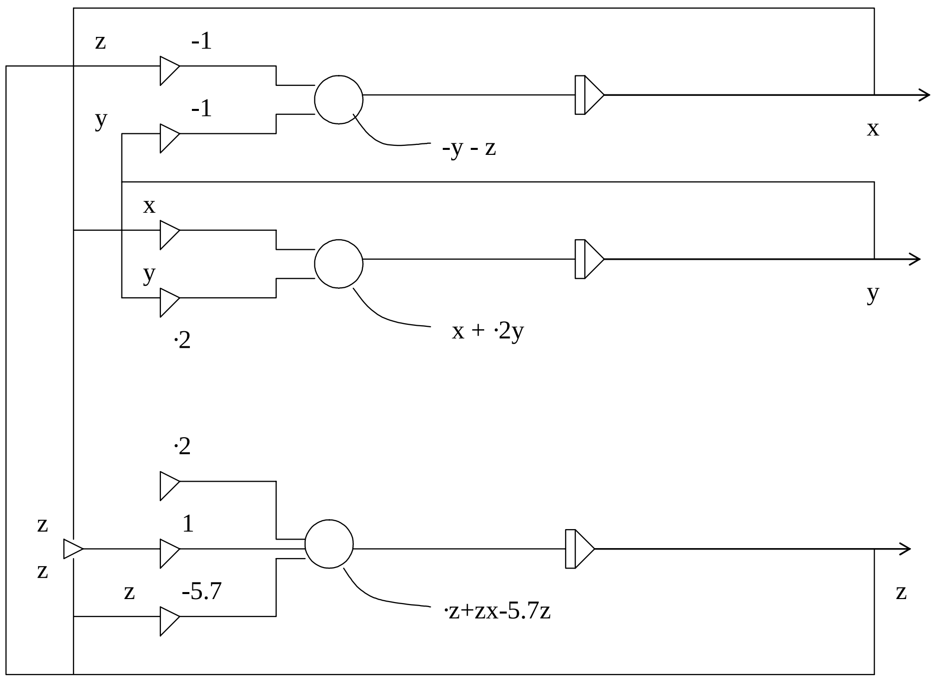

Fig. 2

FIGURE 2

I have chosen this system as an example in the same spirit with which I chose the Verhulst-Pearl and the simple harmonic motion equations, (1) and (10), respectively. They are the simplest in their genre; the *Rössler* equation is the simplest conceivable third order, nonlinear, differential equation - it has only one nonlinearity, in the third equation, between the variables z and x - capable of complicated, untamable dynamics. Its dynamics, using two different numerical algorithms, utilising the ODE Solver in Matlab, are shown below.

On the other hand, the representation of the *Rössler* system in an anlogue computing circuit, with three adders, eight multipliers and three integrators, could be as follows:

In principle, i.e., from a theoretical point of view, this system and its analogue computing representation can be simulated exactly, continually, instantaneously; and all computations are performed simultaneously - i.e., in a parallel mode - without the discretisations that requires deft and careful handling of the underpinnings in the theory of numerical analysis, without the conundrums that the finite precision constraints the digital computer imposes upon systems with SDIC - **S**ensitive **D**ependence on **I**nitial **C**onditions - and without any requirement that the domain and range of definitions of the variables, parameters and constraints should be confined to $\mathbb{N}$, $\mathbb{Q}$ or $\mathbb{Z}$.

Recalling the fertile and mutual interaction between partial recursive functions and Turing Machines, one would seek a definition, if possible by construction, of the class of functions that are analog computable by GPACs. These are precisely the *algebraic differential equations* ([**20**], p.7, [**22**], p.26, [**26**], pp.340-3).

DEFINITION 2. *An algebraic differential polynomial is an expression of the form :*

$$\sum_{i=1}^{n} a_i x^{r_i} y^{q_{0i}} (y')^{q_{1i}} \ldots . \left(y^{(k_i)}\right)^{q_{k_i i}} \tag{3.6}$$

where a_i is a real *number*, $r_i, q_{0i}, \ldots\ldots, q_{k_i i}$ *are non* negative integer *s and y is a function of x.*

DEFINITION 3. *Algebraic differential equations (ADEs) are ODEs of the form:*

$$P\left(x, y, y', y'', \ldots.., y^{(n)}\right) = 0 \tag{3.7}$$

where P is an *algebraic differential polynomial* not identically equal to zero.

DEFINITION 4. *Any solution $y(x)$ of an ADE is called differentially algebraic (DA); otherwise they are called transcendentally-transcendental (*[**22**]*) or hypertranscendental (*[**26**]*).*

Clearly, he definition of ADEs includes all the usual sets of simultaneous systems of linear and nonlinear differential equations that economists routinely - and non-routinely - use. So, we are guaranteed that they are solvable by means of GPACs. Now one can pose some simple questions, partly motivated by the traditions of classical recursion theory:

- Are the solutions to ADEs, generated by GPACs, coomputable?
- Is there a corresponding concept to universal computation or a universal computer in the case of analogue coomputation by GPACs?
- Is there a fix point principle in analogue computing by GPACs that is equivalent or corresponds to the classic recursion theoretic fix point theorem?
- Is there a 'Church-Turing Theses' for analogue computing by GPACs?

The reason I ask just these questions is that an economist who indiscriminately and arbitrarily formulates dynamical hypotheses in terms of ODEs and attempts to theorise, simulate and experiment with them must be disciplined in some way - in the same sense in which recursion theory and numerical analysis disciplines a theorist with warnings on solvability, uncomputability, approximability, etc. It is all very well that the Bernoulli equation underpins the Solow growth model or the Riccati equation underpins the use of control theory modelling environments or the Rayleigh, van der Pol and Lotka-Voltera systems are widely invoked in endogenous business cycle theories. Their use for simulations calls forth the conundrums mentioned above for digital computers and they may require other kinds of constraints to be respected in the case of simulations by GPACs. There will, of course, be engineering constraints: precision engineering requirements on the constructions of the adders, multipliers and the integrators can only achieve a certain level of precision, exactly as the thermodynamic constraints of heat irreversibilities in the integrated circuits of the digital computer. I do not attempt to deal with these latter issues in this paper.

The answer, broadly speaking, to the first question is in the affirmative ([**20**], op.cit, §4, pp.23-27 and [**?**], Theorems 1 and 1', p.1012).

The answer to the second question is easier to attempt if the question is posed in a slightly different way, in tems of the relation between Turing Machines and Diophantine equations (cf. [**17**]).

DEFINITION 5. *A relation of the form*

$$D(a_1, a_2, \ldots\ldots, a_n, x_1, x_2, \ldots, x_m) = 0 \tag{3.8}$$

where D is a polynomial with integer coefficients with respect to all the variables $a_1, a_2, \ldots\ldots, a_n, x_1, x_2, \ldots, x_m$ separated into *parameters* $a_1, a_2, \ldots\ldots, a_n$ and *unknowns* $x_1, x_2, \ldots, x_m$, is called a *parametric diophantine equation.*

A *parametric diophantine equation*, D, defines a set F of the parameters for which there are values of the unknowns such that:

$$\langle a_1, a_2, \ldots\ldots, a_n \rangle \in F \iff \exists x_1, x_2, \ldots, x_m \left[D(a_1, a_2, \ldots\ldots, a_n, x_1, x_2, \ldots, x_m) = 0 \right] \tag{3.9}$$

One of the celebrated mathematical results of the 20th century was the (negative) solution to *Hilbert's Tenth Problem (*[**17**]*).* In the eventual solution of that famous problem two crucial issues were the characterisation of recursively enumerable sets iin terms of parametric diophantine equations and the relation between Turing Machines and parametric Diophantine equations. The former is, for example, elegantly exemplified by the following result ([**15**], Lemma 2, p.407):

LEMMA 1. *For every recursively enumerable set W, there is a polynomial with integer coefficients given by $Q(n, x_1, x_2, x_3, x_4)$, i.e., a parametric diophantine equation, such that, $\forall n \in \mathbb{N}$,*

$$n \in W \iff \exists x_1, \forall x_2, \exists x_3, \forall x_4 \left[Q(n, x_1, x_2, x_3, x_4) \neq 0\right] \tag{3.10}$$

The idea is to relate the determination of membership in a structured set with the (un)solvability of a particular kind of equation. If, next, the (un)solvability of this particular kind of equation can be related to the determined behaviour of a computing machine, then one obtains a connection between some kind of computability, i.e., decidability, and solvability and set membership. This is sealed by the following result:

PROPOSITION 1. *Given any parametric Diophantine equation it is possible to construct a Turing Machine M, such that M will eventually halt, beginning with a representation of the parametric $n-tuple$, $\langle a_1, a_2, ..., a_n\rangle$ iff (16) is solvable for the unknowns $x_1, x_2, ..., x_m$.*

Suppose we think of ODEs as Parametric Diophantine Equations; recursively enumerable sets as the domain for continous functions and GPACs as Tuing Machines. Can we derive a connection between ODEs, continuous functions and GPACs in the same way as above? The affirmative answer is provied by the following proposition, which I shall call Rubel's Theorem:

THEOREM 1. *(Rubel's Theorem): There exists a nontrivial fourth-order, universal, algebraic differential equation of the form:*

$$P(y', y'', y''', y'''') = 0 \tag{3.11}$$

where P is a homogeneous polynomial in four variables with integer coefficients.

The exact meaning of '*universal*' is the following:

DEFINITION 6. *A universal algebraic differential equation P is such that any continuous function $\varphi(x)$ can be approximated to any degree of accuracy by a C^∞ solution, $y(x)$, of P.In other words:*

(3.12)
If $\varepsilon(x)$ is any positive continuous function, $\exists y(x)$ $s.t$ $\mid y(x) - \varphi(x) \mid < \epsilon(x)$, $\forall x \in (-\infty, \infty)$

Recent developments (cf. [**6**],[**3**])have led to concrete improvements in that it is now possible to show the existence of C^n, $\forall n, (3 < n < \infty)$; for example, the following is a specific Universal algebraic differential equation:

$$n^2 y'''' y'^2 - 3n^2 y''' y' + 2n\,(n-1)\,y''^2 = 0 \tag{3.13}$$

In this sense, then, there is a counterpart to the kind of universality propostions in classical recursion theory - computation universality, universal computer, etc., -, also in the emerging theory for analogue computation, particularly, GPACs. Eventually, by directly linking linking such universal equations to Turing Machines via numerical analysis there may even be scope for a more unified and encompassing theory.

As for the third question, my answer goes as follows. GPACs can also be considered generalised fix-point machines! Every solution generated by a GPAC is a fixed-point of an ADE. This is a reflection of the historical fact ad practice that the origins of fixed point theory lies in the search for solutions of differential equations, particularly ODEs[10].

Whether there is a Church-Turing Theses for analogue computation is difficult to answer. The reason is as follows. The concept of computability by finite means was made formally concrete after the notions of solvability and unsolvability or, rather, decidability and undecidability, were made precise in terms of recursion theory. As mentioned in the opening paragraphs of this section, these notions were made precise within the context of a particular debate on the foundations of mathematics - on the nature of the logic that underpinned formal reasoning. As Gödel famously observed:

> "It seems to me that [the great importance of general recursiveness (Turing's computability)] is largely due to the fact that with this concept one has for the first time succeeded in giving an absolute definition of an interesting epistemological notion, i.e., one not depending on the formalism chosen. In all other casws treated previously, such as demonstrability or definability, one has been able to define them only relative to a given language, and for each individual language it is clear that the one thus obtained is not the one looked for.For the concept of computability however, although it is merely a special kind of demonstrability or decidability[11] the situation is different. By

[10] But also PDEs (partial differential equations), as George Temple pointed out ([**30**], p.119):

> "One of the most frutiful studies in topology has considered the mapping T of a set of points S into S, and the existence of fixed points x such that
>
> $$Tx = x$$
>
> The importance of these studies is largely due to their application to ordinary and partial differential equations which can often be transformed into a functional equation $Fx = 0$ with $F = T - I$ where $Ix = x$."

[11] I have always wondered whether this is not a misprint and the word that is meant to be here is not 'decidability' but 'definability'!

> a kind of miracle it is not necessary to distinguis orders, and the diagonal procedure does not lead outside the defined notion. This, I think, shoud encourage one to expect the same thing to be possible also in other cases (such as demonstrability or definability)."
>
> [**8**], p.84.

So, to ask and answer an epistemological question such as whether there is a correspondence to the 'Church-Turing Thesis' in analogue computing by GPACs must mean that we must, first, characterise the formal structure and mathematical foundations of ODEs in a more precise way. I think this is an interesting methodological task, but cannot even be begun to be discussed within the confines of a simple expository paper such as this. I think, however, there will be an interplay between a logic on which continuous processes can be underpinned, say by Lukasiewicz's continuous logic, and the logic of ODEs[12]. My intuition is that there will be some kind of 'Church-Turing Thesis' in the case of analogue computing by GPACs and awareness of it will greatly discipline solution, simulation and experimental exercises by the use of GPACs.

4. Economic Dynamics and Computation

> "What is not so clear is that *continuous processes* (*with differential equations*) may also be regarded as trial and error methods of solution to static equations. The reason why it is not so easy to see is that no human being can make continuous trials infinite in number. This gap in our grasp of the problem has been closed by the perfection of electronic and electro-mechanical computers - sometimes called zeroing servos - which continuously 'feed back' their errors as a basis for new trials until the error has disappeared. *Such a machine is an exact analogue of a continuous dynamic process.* Therefore it seems entirely permissible to regard the motion of an economy as a process of computing answers to the problems posed to it. One might reasonably ask why it is that the *computing* is never finished, why it is not possible to settle down to a correct answer and keep it, thus providing a stationary process to which we could apply static theory with justification. To provide an answer to this question is precisely one of the key problems of *dynamics*."
>
> [**10**], pp.1-2; italics added.

As an elementary application of using a sledge hammer to crack a nut, it is easy to see that the solution function for the Verhulst-Pearl equation (1), given by (2), satisfies all the conditions for the application of the Rubel Theorem on the existence

[12] I suspect that this will be fruitful link to pursue partly because Lukasiewicz, in the development of his continuous valued logic, abandons both the law of the excluded middle and proof by the method of *reductio ad absurdum* - both contentious issues in the debate between Hilbert and Brouwer that led to the foundational crisis in mathematics from which the work of Gödel and Turing emerged.

of a Universal Equation whose solution can be made to agree with $N(t)$ at a countable number of distinct values of t. A similar application of the theorem can be made to the general Bernoulli equation, a nonlinear, first oder, ODE:

$$\dot{y} + g(t) = \psi(t)\, y^n \tag{4.1}$$

Where: $n \in \mathbb{R}$ $(but\ n \neq 0, 1)$.

Although there is an elementary way to transform it into a linear form, the reason I state it in this general way is the fact that n is allowed to be *any* real (except 0 or 1) and it is the general case of which the famous Solow growth equation is a special case. Once again, it is easy to see that the conditions of the Rubel Theorem are satisfied by the normal hypotheses under which this equation is applied in growth theory. Therefore, given observations of growth facts, they can be made to lie on the solution of the Universal Equation which will be withiin $\epsilon(t)$ of the solution of the growth equation[13]. But these are trivial - bordering on the surreal - applications.

The question interesting question is whether it is more fertile to use the digital or the analogue metaphor for a view of the market and for macrodynamic processes as computing devices? I have, for long, felt that the digital metaphor was the more fruitful one; however, partly persuaded by the cogent arguments by my teacher, mentor and friend, the late Richard Goodwin, I have begun to come around to the view that the analogue metaphor does less violation to the basic postulates of economic theory from a dynamical and a computational viewpoint[14]. Richard Goodwin's most persuasive arguments for the analogue metaphor as the more fertile vision for market dynamics and computation was developed in the pages of this Journal ([**10**]). So, it is appropriate that I end with the conjecture that most importantly and crucially, it is in the processes underlying market mechanisms and macrodynamics, particularly in general equilibrium theory, growth theory, cycle theory and growth cycle theory that the benefits of analogue computing will be direct and obvious. I shall consider two well known examples from - one from general equilibrium theory and another from cycle theory to illustrate my point. To take the latter first, consider the final, reduced,

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[13] I must confess that I have never seen a proper discretisation of this equation in the growth literature!

[14] Surely, this agrees with Pareto's famous observation:

> "[In] order to understand what equilibrium is, we had to investigate *how it is determined*. Note, however, that this determination certainly is not for the purpose of arriving at a numerical calculation of prices. .... As a practical matter, that is beyond the power of algebraic analysis ... .. In that case the roles would be changed; and it would no longer be mathematics which would come to the aid of political economy, but political economy which would come to the aid of mathematics. In other words, if all these equations were actually known, *the only means of solving them would be to observe the actual solution which the market gives.*"
>
> [**19**], p.171, italics added.

equation of Goodwin's own celebrated original model of the business cycle ([**?**], p.12, equation (5c); all notations as in the original):

$$\epsilon \dot{y}(t+\theta)+(1-\alpha)\, y\,(t+\theta)=O_{A}\,(t+\theta)+\varphi\left[\dot{y}\,(t)\right] \tag{4.2}$$

Goodwin reduced this delay-differential equation to a second order, nonlinear, differential equation of the Rayleigh type ([**?**], p.13, (7c)):

$$\ddot{x}+\left[X\,(\dot{x})\,/\dot{x}\right]\dot{x}+x=0 \tag{4.3}$$

Equation (23) was 'reduced' to (24) by 'expanding the leading terms [in (23)] in a Taylor series and dropping all but the first two terms in each' ([**?**], p.12). No justification for the validity of this exercise, either mathematical or economic, was given. The paper was instrumental in developing a tradition in endogenous macroeconomic cycle theory, where planar dynamical systems provided the canvas on which to rely for grand themes of economic dynamics. A timely caveat, clearly stated ([**18**], pp.406-7), fell off like water on a duck's back in the macrodynamic community:

> "Had Goodwin approximated his nonlinear difference-differential equation by using the first four terms of the Taylor series expansion of ['the two leading terms'], the resulting approximating equation would have bee a nonlinear differential equation of the fourth order, which we believe would have had two limit cycles solutions rather than one, both dependent on initial conditions. Improving the approximation by retaining more terms of the Taylor's expansion would increase the order of the differential equation and this would increase the number of solutions provided by the approximation. To the extent that this is generally true of nonlinear mixed systems, economic theory encounters a methodological dilemma. .... If mixed systems seem to be required, this implies that we must in general expect a multiplicity of solutions. The resulting indeterminacy must then be overcome by specifying the initial conditions of the model."

Clearly, GPACs and Rubel's Theorem provide a research methodology that would have obviated much of the subsequent straitjacket into which endogenous macroeconomic cycle theory seemed to have fallen. The eventual excitement of using 'chaos theory' could, perhaps, also have been tempered, not to mention the eventual dominance of this area of macroeconomics by stochastic shock theories of one or the other variety[15].

[15]I must emphasise one point at this juncture: the GPACs, as defined here, do not include units for delay and lead dynamics. Nauturally, this is also possible, But I have wanted to concentrate on ODE representations for the limited purposes of this paper.

The real finesse in the use of GPACs to generate solutions to ADEs is the fact that they do not require any compromises with the essential assumptions of continuity, real number domains of definitions for all relevant economic variables, simultaneity and instantaneity. And these virtues, at least at the theoretical level, deserve attention by the computationally minded general equilibrium theorist. The reasons are simple. The general equilibrium theorist proves existence propostions, particularly economic equilibrium theorems, using essentially non-constructive[16]. At a second stage attemtps are made by so-called computable general equilibrium theorists to constructivise or devise effective methods to locate the equilibria. Even at this ostensibly constructive or computabe stage no attempt has even been made to gurarantee that the sought for entity, in most cases, the economic equilibrium configuration, is either constructively defined or computably defined. At most, an approximate equilibirum, hopefully near the supposed equilibrium, is located - but never in any rigorous way, despite many statements to the contrary.

For these aficionados of computable general equilibrium theory, analogue computation is the ideal vehicle. General economic equilibrium theory of the Walrasian variety, in its modern Arrow-Debreu versions, is formalized in terms of real analysis. The problems of discretisations, approximations and simultaneity intrinsic to this theoretical enterprise cannot ever be resolved in any theoretically satisfactory way by adopting ad-hoc constructive or computabe methods. It would be much more direct and theoretically consistent to attempt all numerical, experimental and computational exercises, pertinent to general equilibrium theory with the use of analogue computing machines. Such machines handle data from $\mathbb{R}$ exactly as theory requires it to be handled: continuously. The intrinsic interdependence of a general equilibrium system requires simultaneous - i.e., parallel - computations, continuously executed, and instantaneously available - and all on the basis of analysis in $\mathbb{R}$ (perhaps, occasionally, even in $\mathbb{C}$). None of this is even remotely conceivable with a digital computer. Its mathematical underpinnings are in varieties of constructive and computable mathematics - areas which are totally alien to general equilibrium theory.

For example, even elementary economics textbooks underline the fact that market excess demand functions are the basis upon which any kind of *tâtonnement* rests. These functions are, in turn, built up from individual excess demand functions which are assumed to be continuous and homogeneous of degree zero. To these two assumptions if we add the almost innocuous assumption of Walras' Law, then it is easy to show that the market excess demand function will be normal to the price simplex at the relevant value of the price vector. Then, in view of the celebrated Debreu-Mantel-Sonnenschein Theorem (cf. for example, [**27**])on excess demand functions, the solution to the standard *tâtonnement* process lies on the *vector field* induced by the market excess demand vector on the price simplex. The fascinating implication of all this is

[16]Indeed, not just non-constructive but even worse: cannot be constructivised, without changing the whole mathematical methodology of modelling in standard general equilibrium theory.

that any *arbitrary*, sufficiently smooth, curve on the price simplex will be compatible with some exchange economy. And, conversely, given any *arbitrary* exchange economy, defined by a collection of individual excess demand functions and an appropriate initial price vector, there does not exist enough structure to discipline the dynamics of any *tâtonnement* process.

The question first properly broached by Sonnenschein was: given an excess demand function, knowing that it is continuous if for every price vector $\mathbf{p}$ the inner product $\mathbf{p}.\mathbf{f}(\mathbf{p}) = \mathbf{0}$, where $\mathbf{f}(\mathbf{p})$ is the market excess demand function, *is there a finite number of consumers whose individual excess demand functions sum to* $\mathbf{f}$ (cf. [**5**], p.204). Suppose we ask, instead, given $\mathbf{f}(\mathbf{p})$, is there a dynamical system whose solution is an arbitrary close approximation to it and such that any given finite or countable number of realizations of the solution can be made to lie on $\mathbf{f}(\mathbf{p})$**.** By Rubel's Theorem the answer is in the affirmative and, therefore, the GPAC that is implemented for that particular Universal Equation whose solution approximates $\mathbf{f}(\mathbf{p})$ to any arbitrary degree of accuracy provides a perfectly valid continuous, parallel and instantaneous tracking of the economy in motion toward a fix point solution. One kills, once and forever, the artificial separation between a first, nonconstructive, step in which an uncomputable exchange equilibrium is given a proof of existence and, then, a second step in which it is alleged that an algortihm has been devised to compute an uncomputable equilibrium using a nonconstructive step. This second step is the research program of Computable General Equilibrium theory.

The above two examples are about vector fields. Hence, it may be useful to end this paper be recalling some pertinent observations by a master investigator of vector fields, both its analytical and in its numerical aspects ([**13**]):

> "We regard the computer as an 'oracle' which we ask questions. Questions are formulated as input data for sets of calculations. There are two possible outcomes to the computer's work: either the calculations rigorously confirm that a phase portrait is correct, or they fail to confirm it. .... The theory that we present states that if one begins with a structurally stable vector field, *there is input data that will yield a proof that a numerically computed phase portrait is correct. However, this fails to be completely conclusive from an algorithmic point of view, because one has no way of verifying that a vector field is structurally stable in advance of a positive outcome.* Thus, if one runs a set of trials of increasing precision, the computer will eventually produce a proof of correctness of a phase portrait for a structurally stable vector field. Presented with a vector field that is not structurally stable, the computer *will not confirm* this fact:; it will only fail in its attempted proof of structural stability. *Pragmatically, we terminate the calculation when the computer produces a definitive answer or our patience is exhausted. ....*

> The situation described in the previous paragraph is analogous to the question of producing a numerical proof that a continuous function has a zero. ..... Numerical proofs that a function vanishes can be expected to succeed only when the function has qualitative properties that can be *verified* with finite-precision calculations."
>
> [**13**], pp.154-5, italics added.

One can, of course, repeat this kind of observation for any well posed formal problem and the answer will be the same, tempered, resort to 'exhaustion of patience' or luck. Obviously all the references in the above observations by Guckenheimer are to the digital computer. Thus, one can either theorise *ab initio* in the language that the digital computer can comprehend, or use a different kind of computer that can understand the formal, mathematical, language in which, for example, the economist theorises. It so happens, for reasons of inertia, incompetence, hysteresis or whatever, that the mathematical language of the economic theorist, predominantly if not almost exclusively, is *real analysis*. Either this must be given up or a different kind of computer must be used. I have, after almost a quarter century of 'preaching' the former strategy have begun to wilt in my determination in that task. Therefore these thoughts on adopting a different kind of computer to suit the formal language of the economic theorist.

The above perceptive observations by Guckenheimer also bring up issues of methodology and epistemology, or the domain where the two may meet. Note the liberal sprinkling of words like '*verify', 'confirm'* and '*not confirm*'. That such issues cannot be avoided, even in the most abstract speculations about the foundations of mathematics has been known to those who have fashioned and developed the issues on the basis of which theories of computation and the computer were erected. That the economist continues with princely unconcern for these issues is to be regretted.

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CHAPTER 2

Economic Theory in the Computational and Constructive Mode

1. A Preamble of Sorts

> 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.
>
> [**26**], p.viii; italics in original.

Enormous developments in the theoretical and practical technology of the computer has made a tremendous impact on economics in general, but also in economic theory in particular. However, I do not think I will be misinterpreting the above observation if I point out that it refers to the *digital computer*. But, of course, there are also *analog* and *hybrid* computers[1] that can be harnessed for service by the economist[2] - or any other analyst, in many other fields - to realise the intentions indicated by Richard Stone. Indeed, in many ways, the analog computer should be more suitable for the purposes of the economic analyst simply because, at least as an economic theorist at the microeconomic level, one tends to theorise in terms of *real numbers* and the underpinning mathematics is, almost without exception, *real analysis*. The seemingly simple but, in fact, rather profound observation by Stone that I have quoted above captures one of a handful of insightful visions that the ubiquity of the computer has conferred upon the intellectual adventurer in economics. Stone appeals to the raw quantitative economic analyst to respect the language and architecture of the computer in pursuing precise numerical investigations in economics.

However, in general, we, as economic theorists, tend to 'formulate the questions' in the language of a mathematics that the *digital* computer does not understand - real

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

[2] Charles Babbage, viewed in one of his many incarnations as an economist, can be considered the only one to have straddled both the digital and analog traditions. There is a story to be told here, but this is not the forum for it. I shall reserve the story for another occasion.

analysis - but 'get together the data' that it does, because the natural form in which economic data appears or is constructed is in terms of integer, natural or rational numbers. The transition between these two domains remains a proverbial black box, the interior of which is occasionally viewed, using the lenses of numerical analysis, recursion theory or constructive mathematics. With the possible exception of the core of economic theory[3], i.e., general equilibrium theory, there has been no systematic attempt to develop any aspect of economics in such a way as to be consistent with the use of the computer, respecting its mathematical, numerical and, hence, also its epistemological constraints. To be sure there have been serious applications of concepts of recursion theory and numerical analysis in various disparate, uncoordinated, attempts to many different areas of economics, most particularly game theory and choice theory. But these attempts have not modified the basic mathematical foundations on which the theory of games or choice theory or any other field to which recursion theory or numerical analysis has been applied. The basic mathematical foundations have always remained *real analysis* at suitable levels of sophistication.

Suppose, now, we teach our students the rudiments of the mathematics of the digital computer - i.e., recursion theory and constructive mathematics - simultaneously with the mathematics of general equilibrium theory - i.e., real analysis. As a first, tentative, bridge between these three different kinds of mathematics, let us also add a small dose of lectures and tutorials on computable analysis, at least as a first exposure of the students to those results in computable and constructive analysis that have bearings at least upon computable general equilibrium theory. Such a curriculum content will show that the claims and , in particular, the policy conclusions emanating from applicable general equilibrium theory are based on untenable mathematical foundations. This is true in spite of the systematic and impressive work of Herbert Scarf, Rolf Mantel and others who have sought to develop some constructive[4] and computable foundations in core areas of general equilibrium theory. In this sense, the claims of computable general equilibrium theorists are untenable.

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[3] I should hasten to add another exception: the whole edifying, noble, research program of Herbert Simon is also a notabe exception. But he stands out and aprt from the orthodox traditions of economics where, by orthodoxy, I do not mean just neoclassical economics but all the other standard schools such as the newclassicals, post keynesians, new keynesians, Austrians of all shades, institutionalists and so on; even the behavioural economists, once spanned and spawned by Herbert Simon, have been absorbed into the folds of orthodoxy.

[4] I should mention that Douglas Bridges, a mathematician with impeccable constructive credentials, made a couple of valiant attempts to infuse a serious and rigorous dose of constructivism at the most fundamental level of mathematical economics (cf: [**6**] and [**7**], and two other references mentioned in the latter paper, neither of which have been available to me. They fell like water on a duck's back off the economic theorist's palate). I should also mention that every single explicit proof in the text of Sraffa's classic book is constructive. I have long maintained that there is no need to recast his economics in the formalism of linear algebra to invoke theorems of non-negative square matrices and other theorems of a related sort to re-prove his propositions. (cf. [**24**]). Indeed, I have maintained for years that one can, in fact, use Sraffa's economically motivated techniques to constructivise some of the non-coonstructive methods of proof, for example, for the Perron-Frobenius theorems.

What is to be done? Either we throw away any search for consistent mathematical foundations - the cardinal fulcrum on which general equilibrium theory has revolved for the whole of its development and justification; or we modify general equilibrium theory in such a way as to be based on a mathematics that is consistent with the applicability of the theory in a quantitative sense; or, thirdly, give up relying on the digital computer for quantitative implementations and return to the noble traditions of the analog computer, which is naturally consistent with any theory based on real analysis. I find it surprising that this third alternative has not been attempted, at least not since Irving Fisher.

Complexity theory and the complexity vision in economics is quite another matter. On the one hand there are the formal theories of complexity: computational complexity theory, algorithmic information theory (or Kolmogorov complexity), stochastic complexity theory (or Rissanen's Minimum Description Length principle), Diophantine complexity theory and so on. On the other, there is the so-called 'complexity vision' of economics - a vision much promoted by the Santa Fe research program and its adherents, of varying shades, in economics. Both of these approaches to complexity - as a theory in its own right and as a vision for an alternative economic analysis - have had applications in economics, even in systematic ways. Scarf's work in trying to tame the intractabilities of increasing returns to scale due to indivisibilities has led to pioneering efforts at studying the computational complexity inherent in such problems. Scattered applications of algorithmic information theory, as a basis for induction and as a vehicle through which undecidabilities can be generated even in standard game theoretic contexts, have also had a place in economic theory.

These are all issues that can be subjects for individual research programs and book-length manuscripts, if one is to survey the literature that has dotted the economic journals. I shall not attempt any such survey within the limited scope of cautionary reflections reported in this paper. My aim, instead, is to try to present an overview of the kinds of mathematics that a computer's functioning is based on and ask to what extent the economist has respected this mathematics in posing economic questions and seeking answers via it - i.e., the computer.

With these limited, circumscribed, aims in mind, the next section is devoted to an outline of the kinds of mathematics that underpin theories of computation by the digital computer. The parallel theory for the analog computer is the subject matter of my main contribution to this volume ([**26**]). I do this by superimposing on the standard mathematics of general equilibrium theory some caveats and facts so as to make transparent the non-numerical underpinnings of orthodox theory. The concluding section tries to weave the various threads together into a simple fabric of speculations of a future for economics in the computational mode.

I was motivated to tell a story this way after trying to understand the successes and failures of non-standard analysis in economics. There was a clear, foundational and traditional economic theoretic framework, which, if formalised and interpreted in terms of non-standard analysis, seemed to offer a rich harvest of implications and possibilities for the advancement of economic theory, even in quantitative terms. This latter observation is underpinned by the fact that the practice of non-standard analysis is eminently conducive to numerical exercises without ad-hoc approximations or infelicitous conversions of the continuous to the discrete. That most ubiquitous of the economic theorist's assumption, *price-taking behaviour*, cried out for a formalism in terms of the *infinitesimal*; and much else.

But elementary microeconomic textbooks or advanced macrodynamic textbooks were not rewritten in terms of the formalism of non-standard analysis. Endogenous business cycle theorists did not make any attempt to understand that traditional non-linear dynamical systems, long associated with well characterized basins of attraction, were capable of richer geometric behaviour in the non-standard domain. Intellectual inertia, entrenched concepts, unfamiliar mathematics and, perhaps, even a lack of a concerted attempt by the pioneers of what I would like to call non-standard economics may have all contributed to the lack of success in supplanting traditional mathematical economics. It seems to me the main source of the inertia is the lack of initiative to learn new and alternative mathematics and a monumental lack of knowledge about the mathematical foundations of the computer and computation. Hence, I thought, after also mulling over the fate of non-standard economics, if a story about computability, constructivity and complexity could be told from within the citadel of economic theory, there may well be a more receptive and less obdurate audience.

2. Cautionary Notes

> "Even those who like algorithms have remarkably little appreciation of the thoroughgoing algorithmic thinking that is required for a constructive proof. This is illustrated by the nonconstructive nature of many proofs in books on numerical analysis, the theoretical study of practical numerical algorithms. I would guess that most realist mathematicians are unable even to recognize when a proof is constructive in the intuitionist's sense.
>
> *It is a lot harder than one might think to recognize when a theorem depends on a nonconstructive argument. One reason is that proofs are rarely self-contained, but depend on other theorems whose proofs depend on still other theorems. These other theorems have often been internalized to such an extent that we are not aware whether or not nonconstructive arguments have been used, or must be used, in their proofs.* Another reason is that the law of excluded middle [LEM] is so ingrained in our thinking that we do not distinguish between different formulations of a theorem that are trivially equivalent given

> LEM, although one formulation may have a constructive proof and the other not."
> [**19**]

Why should an *economist* bother about constructive and non-constructive proofs or whether a theorem in economics depends on the use of the law of the excluded middle (LEM) or whether status of the Hahn-Banach theorem, which lies behind the mathematical validity of the Second Fundamental Theorem of Welfare Economics, is dubious in computable analysis or whether the status of the Cauchy-Peano theorem in proving existence of solutions to ordinary differential equations (ODEs), or whether it is really necessary to appeal to topological, non-constructive, fix-point theorems in economic theoretic contexts? My questions are not posed as a methodologist or as an epistemologist, although such questions, when answered, may well have methodological and epistemological implications. I ask these questions as a serious user of the computer in ordinary economic analysis: in simulating policy environments and obtaining usable parameter estimates; in testing conjectures by studying alternative scenarios of analytically intractable non-linear systems of equations; in taming the numerical monsters that can arise in increasing returns to scale technologies caused by indivisibilities that are naturally framed as combinatorial optimization problems; and many other bread-and-butter issues in elementary applied economics that seeks economic theoretic and mathematical foundations for its numerical implementations.

Why does a mathematician express disquiet over the use or the invoking of the axiom of choice[5] in any mathematical exercise in theorem proving? Kuratowski and Mostowski, in their massive and encyclopedic treatise on *Set Theory* gave the standard reason for the general disquiet in mathematical circles over the use of this axiom in proof contexts:

> "The axiom of choice occupies a rather special place among set theoretical axioms. Although it was subconsciously used very early, it was explicitly formulated as late as 1904 ... and immediately aroused a controversy. Several mathematicians claimed that proofs involving

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[5]Compare and contrast the following two views on the status of this axiom in mathematics, the first observation is by two distinguished constructive mathematicians and the second by a an equally distinguished mathematical economist:

> "[The axiom of choice] is unique in its ability to trouble the conscience of the classical mathematician, but in fact it is not a real source of nonconstructivity in classical mathematics. ... The axiom of choice is used to extract elements from equivalence classes where they should never have been put in the first place." ([**4**], p.12)

and,

> "Although the validity of Zorn's lemma is not intuitively clear, it is demostrably equivalent to an important *axiom of choice* that is *accepted today by most mathematicians.*" ([**27**], p.15; second set of italics added.)

the axiom of choice have a different status from proofs not involving it, because the axiom of choice is a unique set theoretical principle which states the existence of a set without giving a method of defining ("constructing") it, i.e., is not effective."

[**16**], p.54; footnote 1.

And they go on, in their text of over 500 pages and hundreds of theorems and lemmas, from that point on by marking 'theorems which are proved using the axiom of choice with the superscript ^{o} ' (ibid, p.54). Another classic in the foundations of mathematics, the *Principia Mathematica* of Russell and Whitehead, observed the same kind of principle. Why, then do economists not point out, say with an asterix (*), those theorems in economic theory that depend on non-constructive reasonings (or appeals to LEM) and, perhaps, a double asterix (**) where reliance is placed on uncountable domains of definitions or non-recursive sets of various sorts such that computability or decidability fails? In particular, where claims are made as to the quantitative applicability and relevance of the implications of the theorems, derivable by utilizing the computer. For example, would it not serve the student of Computable General Equilibrium (CGE) theory to be warned, with such an asterix, whenever a particular proposition is derived with the help of undecidable disjunctions and a cautionary note that the claimed constructions is not theoretically sound or reliable? Or when momentous policy conclusions regarding decentralization and its virtues are drawn on the basis of the Second Fundamental Theorem of Welfare Economics without a warning, with the double asterix, that the status of such a theorem in computable analysis is dubious, to put it mildly and its status in constructive analysis is subject to severe constraints on the domain of definition of the relevant variables - prices and quantities in the case of economics.

I do not have any sensible answers to any of these simple questions except that economists are rather cavalier in their use of mathematics and careless in their reliance on the computer. Hence, in these 'cautionary notes', I wish to try to initiate a tradition where we may serve the prudent and the thoughtful economic theorist, who is also acutely conscious that there is a mathematics underlying the activities of the ubiquitous computer, where the pitfalls may reside and how, in some cases, one may avoid them by adopting deft strategies. With this in mind, I provide, in this section, some rather basic results from computable and constructive analysis that have bearing upon the use (or, rather, the misuse) of well-known classical theorems, with especial reference to CGE. The reason for placing especial emphasis on CGE is that it is the only field in the core of economic theory where there has been a systematic and conscious effort to incorporate both constructive and computable structures in a seriously applicable and positive way, and not only as intellectual curiosa with universal negative results such as undecidabilities, uncomputabilities and unsolvabilities. However, I touch upon implications to other areas in economics, as well.

I think it is too much to expect any single textbook in economics, whether advanced or intermediate, whether exclusively oriented towards mathematical economics or not, to follow the enlightened policies of a Russell-Whitehead or a Kuratwoski-Mostowski, and to mark with an asterisk or a double asterix when constructively or computably dubious assumptions are made. Therefore, I shall list a sample of such fundamental assumptions and theorems that have bearings upon economics in its computation mode. By this I mean, economics, whether micro or macro, where frequent recourse to computation, simulation and existence proofs are made. This means, for example, at the very *broadest* pedagogical, policy oriented and textbook level, general equilibrium theory, growth and cycle theory, welfare economics and so on. I shall stick to these three fields in this essay, simply in view of space considerations.

Debreu, in his classic text that codified the modern version of general equilibrium theory, Theory of Value, stated in the 'Preface' that 'the small amount of mathematics necessary for a full understanding of the text (but not all of the notes) [of the rest of the book] is given in the first chapter in a virtually self-contained fashion. ([**7**], p.viii). Let us suppose Debreu cautioned his readers that he was, in fact, choosing one possible mathematics from a world of many possible mathematics, without giving reasons as to why he chose the one he did rather than any other. However, suppose, in the interests of intellectual honesty he added caveats to some of the definitions, axioms and theorems, pointing out that they did not hold, were invalid, in other possible, equally valid mathematics - *particularly the mathematics underpinning the computer, computation and algorithms.* What kind of caveats might he have added if the aims had been to warn the reader who had computations in mind? I give a small, but fundamental, representative sample.

The four sections dealing with the *fundamentals* of limit processes in Debreu's *classic* are[6] on Real Numbers (§1.5), Limits in R^m(§1.6), Continuous Functions (§1.7) and Continuous Correspondences (§1.8). To this may be added §1.9, culminating in formal separating hyperplane theorems and §1.10 on the two fundamental fixed point theorems of Brouwer and Kakutani. These exhaust, essentially, all the mathematical fundamentals on which is built the formal equilibrium economic *Theory of Value.* Thus, if I single out the *axiom of completeness* (§1.5.*d,* p.10), definitions of *compact*, given on p.15, §1.6.*t, continuity, topologically characterized* in §1.7, the *maximum-minimum theorem* (§1.7.*h*, (4') and named after Weierstrass on p.16 by Debreu) and the two *fixed point* theorems (Brouwer and Kakutani) of §1.10 (p.26), in addition to the theorems of the *separating hyperplanes* given in §1.9 (esp. pp.24-5), as being the fundamental mathematical tools on which the whole edifice of general equilibrium theory stands, I do not think it will be considered a wild exaggeration. If these cannot be given numerical or computational content, how can they be useful in formalising economic concepts and entities with a view to application?

[6]All references are to [**7**].

Now. let us imagine an economic fairy, rather patterned after tooth fairies, appends the following caveats to this chapter, with special reference to the singled-out concepts, axioms, results and theorems..

PROPOSITION 2. *: The Heine-Borel Theorem is Invalid in Computable Analysis.*

The standard, i.e., classical mathematical, statement of the Heine-Borel Theorem is:

A subset S of $\mathbb{R}^m$ is compact if and only if it is closed and bounded (cf. [**7**]*, p.15, §1.6.t)*

PROOF. :See [**1**], §13.2, pp.131-2. □

PROPOSITION 3. *:The Bolzano-Weierstrass Theorem is invalid in Constructive Analysis.*

A classical version of the Bolzano-Weierstrass theorem is:
Every bounded sequence in $\mathbb{R}^m$ has a convergent subsequence.

PROOF. :See [**12**], pp. 14-15. □

AXIOM 1. *Completeness Property (cf.* [**7**]*, §1.5.d, p.10)*
Every non-empty subset X of $\mathbb{R}$ which has an upper bound has a least upper bound.

THEOREM 2. *Specker's Theorem in Computable Analysis (*[**23**]*, pp. 145-58)*
A sequence exists with an upper bound but without a least upper bound.

PROOF. : See [**2**], pp. 97-8. □

Note, also, the following three facts:

(1) There are 'clear intuitive notions of *continuity* which cannot be [topologically] defined'. (cf.[**13**], p.73).
(2) The Hahn-Banach Theorem is not valid in Constructive or Computable Analysis in the same form as in Classical Analysis. ([**17**], esp. §5, pp. 328-32) and [**4**], p.342.
(3) A consequence of the invalidity of the Bolzano-Weierstrass Theorem in Constructive Analysis is that the fixed point theorems in their classical forms do not hold in (Intuitionistically) Constructive Mathematics. ([**8**], pp.1-2).

On the basis of just these, almost minimal, set of caveats and three facts, I can easily state and prove the following two propositions - one, I may call *The Grand Proposition*[7] and the other, *The Non-Constructivity Proposition.* Keeping in mind the insightful and important observations made by Fred Richman with which I began

[7]I hope the reader is able to infuse an appropriate dose of humour at this point lest the irony is mistaken for megalomania!

this section, a 'proof' of The Grand Proposition will require that I take, in turn, every single of the economic axioms, definitions, propositions and theorems in *The Theory of Value* and show where and how they rely on one or the other of the above caveats or facts. This, although a tedious task, is easy to do - even to automate. I leave it for an enthusiastic doctoral dissertation by any imaginative and adventurous student. A possible way to state this proposition in its full generality would be:

PROPOSITION 4. *: The Grand Proposition*

The Arrow-Debreu Axiomatic Analysis of Economic Equilibrium Cannot be Computationally Implemented in a Digital Computer.

PROPOSITION 5. *: The Non-Constructivity Proposition*
Neither of the Fixed Point Theorems given and used by Debreu in The Theory of Value can be Constructivized.

PROOF. A simple consequence of the constructive invalidity of the Bolzano-Weierstrass Theorem. □

What, then, are we to make of grand, quantitative, policy prescriptions emanating from applications of the two fundamental theorems of welfare economics and, even more pertinently, of the results and use of *Computable General Equilibrium* theory (CGE)? I suggest that a large barrel of salt be kept within reaching distance of anyone being subject to quantitative homilies, based on computations that rely on these two classes of models. Let me illustrate this cautionary attitude slightly more formally for CGE.

Walras, Pareto, Irving Fisher, Hayek, Lange, Marschak and others were concerned with the *computational implications* of the solutions of a general economic equilibrium model and each, in their own idiosyncratic and interesting ways, suggested methods to solve it. Walras, Pareto, Hayek and the Lange of the 30s, had, obviously, an interpretation of the market as an *analog* computing machine. The *digital* computing machine, as we know it today, even though conceived by Leibniz and others centuries earlier, came into being only after the theoretical developments in recursion theory that resulted from the work of Gödel, Turing, Church, Kleene and Post. However, it is almost entirely Scarf's single-handed efforts that have made the issue of computing the solution of an existence problem in general equilibrium theory meaningful, within the digital computing metaphor[8], in a numerical and economic sense. Scarf took the Arrow-Debreu version of the general competitive model and, imaginatively exploiting

[8]The two traditions were demarcated with characteristic clarity by Scarf himself in his essay for the Irving Fisher birth centennial volume:

"In *Mathematical Investigations in the Theory of Value and Prices*, published in 1892, Irving Fisher described a mechanical and hydraulic **analogue device** intended to calculate equilibrium prices for a general competitive model. This chapter takes up the same problem and discusses an the implications of the equivalence between the *Brouwer Fix Point Theorem* and the *Walrasian Economic Equilibrium theorem*, proved by Uzawa in 1962 ([**32**]) made the equilibrium 'approximately' constructive.

Scarf's methods and framework are based on mathematical structures and computing tools and concepts of a kind that are not part of the standard repertoire of an economics graduate student's education. Constructive mathematics, recursion theory and computational complexity theory are the main vehicles that are necessary to analyse computable general equilibrium models for their computability and computational efficiency properties. By providing enough of the essentials of computability theory and constructive mathematics to ask interesting and answerable questions about the computable general equilibrium model and its computable and constructive meaning, one enters the citadel with the Trojan Horse of CGE. The strategy of adding constructive and computable analytic caveats and facts to an otherwise orthodox textbook presentation of standard real analysis as a supplement or an appendix to a presentation of general equilibrium theory, the way I have suggested above, is another vantage point.

There are two aspects to the problem of the constructivity and effectivity of CGE models:

- · The role of the Uzawa equivalence theorem.
- · The non-constructivity of the topological underpinnings of the limit processes invoked in the proof of the Brouwer fixed point theorem.

For over half-a-century, topological fixed point theorems of varying degrees of generality have been used in proving the existence of general economic equilibria, most often in varieties of Walrasian economies as, for example, done in Debreu (op.cit). Uzawa's fundamental insight was to ask the important question whether the mathematical tools invoked, say the Brouwer or Kakutani fixed point theorems, were unnecessarily general[9] . His important and interesting answer to this question was to show the reverse implication and, thus, to show the mathematical equivalence between an economic equilibrium existence theorem and a topological fixed-point theorem.

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algorithm for a **digital computer** which approximates equilibrium prices to an arbitrary degree of accuracy." ([**20**], p.207; bold italics added.)

[9] As Debreu noted:

> "[The theorem] that [establishes] the existence of a price vector yielding a negative or zero excess demand [is] a direct consequence of a deep mathematical result, the fixed-point theorem of Kakutani. And one must ask whether the .... Proof [of the theorem] uses a needlessly powerful tool. This question was answered in the negative by Uzawa (1962) who showed that [the existence theorem] directly implies Kakutani's fixed-point theorem."
>
> [**11**], p.719.

In other words, the *Uzawa Equivalence Theorem* demonstrates the mathematical equivalence of (for example) the following two propositions[10]:

Proposition 6. *Brouwer Fix Point Theorem (Debreu, op.cit., §1.10.b (1); p.26)*

If S is a non-empty, compact, convex subset of $\mathbb{R}^m$, and if f is a continuous function from S to S, then $f$has a fixed point.

Proposition 7. *Walras' (Equilibrium) Existence Theorem (Uzawa's Formulation; cf.*[**32**]*, pp. 59-60)*

Let there be n commodities, labelled $1, ..., n,$ and let $p = (p_1, ..., p_n)$ and $x = (x_1, ..., x_n)$ be a price vector and a commodity bundle, respectively.

Let P and X be the sets of all price vectors and of all commodity bundles:

$P = \{p = (p_1, ..., p_n) : p_i \geqslant 0, i = 1, ..., n, \ but\ p \neq 0\}$

$X = \{x = (x_1, ..., x_n)\}$ (i.e., commodity bundles are arbitrary $n - vectors$)

Let the excess demand function, $x(p) = [x_i(p), ..., x_n(p)]$, map P *into* X and satisfy the following conditions:

(A). $x(p)$ is a continuous mapping from P *into* X.

(B). $x(p)$ is homogeneous of order 0; i.e., $x(tp) = x(p)$, for all $t > 0$ and $p \in P$.

(C). Walras' law holds:

$$\sum_{i=1}^{n} p_i x_i(p) = 0, forallp \in P.$$

Then there exists at least an equilibrium price vector $\overline{p}$ for $x(p)$;

Where a price vector $\overline{p}$ is called an equilibrium if:

$$x_i(\overline{p}) \leqslant 0, \qquad (i = 1, ..., n)$$

with equality unless $\overline{p}_i = 0, \qquad (i = 1, ..., n)$ [11].

Now, in Uzawa's equivalence proof there is a 'construction' of a class of excess demand function satisfying conditions (A), (B) and (C), above. A particularly clear and pedagogically illuminative discussion of this construction is given in [**25**], pp. 137-8. This 'construction' does not place either computable or constructive constraints on the class of functions from which they are built. This, compounded by the constructive invalidity of the Bolzano-Weierstrass Theorem, makes Uzawa's Equivalence Theorem neither constructively nor computably implementable in a digital computer.

[10]Paradoxically, even after forty years since Uzawa's seminal result and almost as many years of Scarf's sustained attempts to make general equilibrium theory amenable to computations, I know of only one textbook presentation of the Uzawa equivalence theorem and its ramifications. This is the pedagogically superb textbook Ross Starr ([**25**], esp. chapter 11).

[11]There is a minor mispriint inthe original Uzawa statement at this point where it is written $\overline{p}_1$ insead of $\overline{p_i}$. (Uzawa, op.cit, p.60).

Hence any claim that Computable General equilibrium theory is either computable or constructive is vacuous.

What are we, then, to make of the following assertion:

> "The major result of postwar mathematical general equilibrium theory has been to demonstrate the existence of such a [Walrasian] equilibrium by showing the applicability of mathematical fixed point theorems to economic models.
>
> ...
>
> The weakness of such applications is [that] they provide non-constructive rather than constructive proofs of the existence of equilibrium; that is, they show that equilibria exist but do not provide techniques by which equilibria can actually be determined. ... The extension of the Brouwer and Kakutani fixed point theorems in [the] direction [of making them constructive] is what underlies the work of Scarf on fixed point algorithms ..."
>
> [**21**], pp. 12, 21.

This is where that large barrel of salt to dip into will be quite useful - plus, of course, knowledge of the caveats and facts, given above. For further discussion, details and implications of the non-constructivity and uncomputability inherent in the Uzawa Equivalence Theorem and Scarf's work on fixed point algorithm I refer the reader to [**30**].

An even more dangerous message is the one given in the otherwise admirable above mentioned text by Ross Star ([**25**], p.138):

> "What are we to make of the Uzawa Equivalence Theorem? It says that the use of the Brouwer Fixed-Point Theorem is not merely one way to prove the existence of equilibrium. In a fundamental sense, it is the only way. Any alternative proof of existence will include, inter alia, an implicit proof of the Brouwer Theorem. Hence this mathematical method is essential; one cannot pursue this branch of economic without the Brouwer Theorem. If Walras failed to provide an adequate proof of existence of equilibrium himself, it was in part because the necessary mathematics was not yet available."

Obviously, Ross Starr and others advocating a one-dimensional mathematization of equilibrium economic theory have paid little attention to even the more obviously enlightened visions of someone like Smale, who has thought hard and deep about computation over the reals (cf:,for example, [**5**]):

> “We return to the subject of equilibrium theory. The existence theory of the static approach is deeply rooted to the use of the mathematics of fixed point theory. Thus one step in the liberation from the static point of view would be to use a mathematics of different kind. . . . Also the economic equilibrium problem presents itself most directly and with the most tradition *not as a fixed point problem*, but as an equation, supply equals demand. *Mathematical economists have translated the problem of solving this equation into a fixed point problem.*
>
> I think it is fair to say that for the main existence problems in the theory of economic equilibria, one can now bypass the fixed point approach and attack the equations directly to give existence of solutions, with a simpler kid of mathematics and even mathematics with dynamic and algorithmic overtones.”
>
> [**22**], p.290, italics added.

So, what are we to do? I do not think the situation as far as computational underpinnings for economic theory, even for equilibrium economic theory, is as desperate as it may seem from the above discussions and observations. After all, that great pioneering equilibrium theorist, Irving Fisher, untrammelled by the powers of the digital computer and its mathematics, resorted in a natural way to analogue computing. However, if we, in an age dominated by the digital computer want to preserve the edifice of orthodox equilibrium theory, and also want to make it computable or to constructivize it, then there are several alternative routes to take. The obvious way, the Irving Fisher way, is of no use since our starting point is the dominance of the digital computer. The alternative ways are to harness one of the theories of real computation, say the kind being developed by Smale and his several collaborators, or the approach of Pour-El and Richards and so on. My own suggestion is the following.

In addition to the above caveats and facts, suppose I also introduce the mathematically minded student to the following *constructive* recursion theoretic fixed point theorem (cf., for example, [**18**], pp. 158-9)

Theorem 3. *There is a total recursive function f such that, $\forall x$, if ϕ_x is total, then we have $\phi_{h(x)} = \phi_{\phi_x(h(x))}$.*

In other words, given any total function $f = \phi_x$ the fixed point x of f can be computed through the use of the total recursive function h.

If I, then, work backwards and re-build the economic theoretic formalizations making sure, at each step, that the constituent functions, definitions, and elements are all consistent with the applicability of the recursion theoretic fixed point theorem. Then, of course, the theorem can be applied to prove the existence of an economic equilibrium which will be as much Walrasian as the Arrow-Debreu formalization - indeed, more so. After all, Walras' times were long before compactness, the Brouwer fixed point theorem, the completeness axiom, the separating hyperplane theorems and so on. Walras and Pareto were writing at a time when Picard's *iteration* and rudimentary *contraction mappings* were the metaphors on the basis of which to understand iteration and equilibrium *processes. Nothing more complicated than variations on these concepts are involved in the above recursion theoretic fixed point theorem - not even the completeness axiom, let alone compactness, continuity and all the other paraphernalia of the limit processes underpinning real analysis.*

It may well be true that 'Walras failed to provide an adequate proof of existence of equilibrium himself'. It may even be true that 'it was in part because the necessary mathematics was not yet available'. But it certainly does not imply, as noted by Smale in a different way, that 'the Brouwer Fixed-Point Theorem .. is the only way .. to prove the existence of equilibrium'. Nor does it mean that 'any alternative proof of existence will include .. an implicit proof of Brouwer's Theorem'. This kind of dangerous claim, even while utilizing the Uzawa Equivalence theorem to build the vast edifice of a Computable General Equilibrium structure that is computably and constructively untenable, compounds ignorance of the mathematics of computation, particularly via the digital computer, and, even more importantly, the world of mathematics itself.

3. Economic Theory in the Computational Mode

> "The term 'computing methods' is, of course, to be interpreted broadly as the mathematical specification of algorithms for arriving at a solution (optimal or descriptive), rather than in terms of precise programming for specific machines. Nevertheless, we want to stress that solutions which are not effectively computable are not properly solutions at all. Existence theorems and equations which must be satisfied by optimal solutions are useful tools toward arriving at effective solutions, but the two must not be confused. Even iterative methods which lead in principle to a solution cannot be regarded as acceptable if they involve computations beyond the possibilities of present-day computing machines
>
> [**3**] p.17[12].

If we are to take these thoughts seriously, and remember Stone's admonishment, then I suggest that we reorient our activities as economic theorists in the computational mode with the following strategies in mind[13]:

- · The triple $\{assumption, proof, conclusion\}$ should be understood in terms of $\{input\ data, a\lg orithm, output\ data\}$.
- · Mathematics is best regarded as a very high level programming language.
- · In constructive, computable and (constructive) nonstandard analysis, every proof is an algorithm.
- · To understand a *theorem* (in any kind of mathematics) in algorithmic terms, represent the *assumptions* as *input data* and the *conclusions* as *output data*. Then try to convert the proof into an algorithm which will take in the input and produce the desired output. If you are unable to do this, it is probably because the proof relies essentially on the *law of excluded middle*. This step will identify any inadvertent infusion of non-constructive reasoning.
- · If we take algorithms and data structures to be fundamental, then it is natural to define and understand functions in these terms. If a function does not correspond to an algorithm, what can it be? Hence, take the stand that functions are, by definition, computable or constructive.
- · Given a putative function f, we do not ask "Is it computable?", or "Is it constructive?", but rather "What are the data types of the domain and of the range?" This question will often have more than one natural answer, and we will need to consider both restricted and expanded domain/range pairs. Distinguishing between these pairs will require that we reject excluded middle for undecidable propositions. If you attempt to pair an expanded domain for

[12] I am indebted to my friend and colleague, Enrico Zaninotto, for bringing this paper and, in particular, to this important, early, observation by Arrow, et.al, to my attention.

[13] I have been influenced to formulate a strategy in this way by a reading of [**14**].

f with a restricted range, you will come to the conclusion that $f$is non-computable or non-constructive.

None of these steps are part of the standard repertoire of textbooks, research methodologies or traditions of economic education and practice. No one can contemplate a seriously mathematical, empirical or experimental study of economics without following the above strategy, or some variant of it, if computations by a digital computer is to be a vehicle for modelling, testing and inference.

This strategy,I think, will lead to a formulation of the existence problem as a *decision problem* in the recursion theoretic sense. In particular, we will be more precise in choosing the domain of definition of economic variables and not blindly and arbitrarily let them be the real numbers. I envisage a day when the 'economic problem' will become a decision problem for the solution of diophantine equations. What kind of theorems could we hope to devise if we follow this strategy? I put it this way because, of course, I expect us, as mathematical economists, to have left *Cantor's Paradise* – not driven away – voluntarily; I expect, also, that we would have abandoned Plato in his caves, contemplating ideal existence, inferred from shadows of the mind. Theorems would be invented, not discovered. When theorems have to come with a recipe for construction, we will also wonder about the costs of construction, costs in the widest economic sense. Thus, economists will have to come to terms with the theory of *computational complexity* and try to go beyond worst-case analysis.

If we take too seriously statements like the following, where the implicit assumption is that 'theorems' are the exclusive prerogative of the Formalists, the Bourbakians and varieties of Platonists, then we may be entrapped in the belief that mathematical economics can only be practised in one, unique, way:

> "My guess is that the *age of theorems may be passing* and that of simulation is approaching. Of course there will always be logical matters to sort out, and our present expertise will not be totally obsolete. But the task we set ourselves after the last war, to deduce all that was required from a number of axioms, has almost been completed, and while not worthless has only made a small contribution to our understanding."
>
> [**15**], p. 258; italics added.

The age of theorems of a particular kind of mathematics may be passing; after all nothing has been heard from the Bourbakian stables for quite a number of years (cf. [**9**]). But *not* new kinds of theorems, those based on numerically meaningful assumptions and with computationally rich implications. One of the great merits of recursion theory and constructive mathematics is the explicit awareness and recognition that mathematical concepts are, often, formalizations of intuitive concepts; that in the formalization, which is always *pro tempore*, blurred borderlines are the order of the day.

The traditional mathematical economist does not bat an eyelid when accepting the $\varepsilon - \delta$ definition of continuity even in economic contexts without wondering whether it is appropriate. The recursion theorist or the constructive mathematician is not that cavalier. Two of the great achievements of 20th century mathematics were the mathematical formalization of the intuitive concepts of effective calculability and randomness. The recursion theorist is happy to work with theses that capture, again *pro tempore*, the intuitive contents of such terms, honed by centuries of experience. The real analyst is less disciplined. What would the Bourbakian mathematical economist say if I assert that 'topological space' does not necessarily capture the full intuitive content of continuity? What would the computable economist say if I said that the *Church-Turing thesis* does not capture the intuitive content of effective calculability? Or how would the applied recursion theorist react to a counter-example to the *thesis on randomness* put forward by *Kolmogorov-Chaitin-Solomonoff-Martin-Löf ?* I am sure the latter two - the computable economist and the applied recursion theorist - will happily adjust their mathematical concepts rather than force the intuitive notions to conform to the formal ones. I am not sure about the Bourbakian or Formalist mathematical economist, who must have known, for decades, for example, that the topological definition of continuity is intuitively inadequate, or that the Heine-Borel theorem is invalid in Computable Analysis, or that the Bolzano-Weierstrass Theorem is false in Constructive mathematics, but continues (sic!) to plague economic formalism with definitions that rely on them, as if they were indisputable truths. How can such a formalism adapt to the age of the digital computer?

The main message of this paper is very similar in spirit to the epistemological and methodological points made by Tommaso Toffoli many years ago (cf, [**28**]). Indeed, reading his paper, substituting 'mathematical economics' for 'mathematical physics', makes my case quite clearly (p.117-8; italics in the original):

> "Mathematical physics, both classical and quantum-mechanical is pervaded by the notion of a 'continuum', that is, the set $\mathbb{R}$ of real numbers with its natural ordering and topology. .... How do we manage to specify some constructive way the behavior of a system beset by so many uncountable infinities? ....[In] modeling physics with the traditional approach, we start for historical reasons .. with mathematical machinery that probably has much more than we need, and we have to spend much effort disabling or reinterpreting these 'advanced features' so that we can get our job done in spite of them. On the other hand, ..., we outline an approach where the theoretical mathematical apparatus in which we 'write' our models is essentially isomorphic with the concrete computational apparatus in which we 'run' them.'

The difference is that I am not as wedded to the power of cellular automata as fruitful repositories of economic metaphors as Toffoli is for them as vehicles for modeling physics. This is partly because, paradoxically, I believe economic statics remains an important field in its own right and, therefore, getting our formal language more appropriate for both dynamics and statics entails getting our mathematical theorising less *ad hoc*.

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CHAPTER 3

Preplexed in the Tangled Roots of the Busy Beaver's Ways

".. [T]he efforts to construct examples of non-computable functions reveal the general conviction that over and beyond the class of computable ... functions there is a much wider class, the class of *well-defined functions*. **The scope of this latter class is vague; in some quarters, there exists a belief that this class will be defined some day in precise terms acceptable to all**."

Tibor Rado (1962), p.877; bold emphasis added.

1. Preamble

"Of course, unless one has a theory, one cannot expect much help from a computer (unless *it* has a theory) except for clerical aid in studying examples; ..."

Marvin Minsky (1967), pp. 267-8 (italics in original).

I have often[1] expressed disquiet at the prevailing practice in mathematical economic circles of what seemed a tacit acceptance of the belief that the computable functions were a subset of a larger class of more generally defined and routinely accepted functions. In the years since this disquiet first manifested itself in my thoughts, I have been mulling over the reasons for the tacit belief, in orthodox circles, for this unwarranted belief and, occasionally, lectured on why it is unwarranted and have tried to suggest an alternative vision. Given this opportunity to be in the company of practitioners of the noble art of experimenting with simulations gave me also the chance to make a preliminary attempt at making explicit this alternative vision.

[1]Francesco Luna's perceptive comments saved me from gross errors and also helped improve the statement of theorem 2 and the proof of proposition 1A. pedro campesino (who refuses to use upper case letters and maintains that the tamil alphabet has survived quite admirably without any distinction between upper case and lower case and is prepared to give other such counter-examples) is responsible for all remaining errors, omission and infelicities.

As I understand it[2] , agent based-modelling, and its subsequent simulation, is generally implemented in so-called object oriented programming languages. Agent-based modelling has, of course, a long and venerable tradition in economic theory. It is the emphasis on computational aspects that makes modern agent-based modelling somewhat novel and different from the traditional approaches[3]. The computational aspect is realized via the medium of object oriented programming in general and *SWARM* and other similar languages, in particular.

Given the chosen medium of modelling, viz., object oriented programming, it is clear that the agents, their abstract structure – both static and dynamic – and their interactions will be couched in the categories that characterize this medium: *abstraction, encapsulation, inheritance and polymorphism.* More particularly, any algorithm expressed in an object oriented language is a set of *object definitions* and *rules* that characterize object interactions. Given the quadruple that characterize any object oriented language, the *object definitions* and *rules* are, inevitably, *recursively defined.* Hence it is natural that the computational aspects are, without exception, recursive in the precise sense of recursion theory (or computability theory)[4].

Whether the practitioners of agent based modelling via the medium of object oriented programming state it explicitly or not, are consciously aware of it or not, the class of functions they implement in all their simulations belong, therefore, to the mathematics of recursion theory (or, in possible generalizations, some variety of constructive mathematics[5]). I shall, therefore, frame my discussion of the *tangled roots of the Busy Beaver's ways* by also keeping in mind the fact that it will have implications, however indirect and tenuous they may seem at one remove, for the experimental simulations, in the language of object oriented programming, of agent-based modelling.

I have, however, a more direct and immediate reason for framing the presentation of the activities of the members of the Busy Beaver club in the context of the general research strategy adopted by agent-based modellers. This strategy, if I am even only reasonably correct, relies on connecting a collection of *finite automata* with rules of

[2]An understanding gleaned from a reading of the following two volumes on agent-based modelling (Luna & Stefansson, 2000, Luna & Perrone, 2002), but with the caveat that I am very much of an external observer of these admirable attempts to make economics a laboratory subject. My own view on how economics should be made a laboratory subject is diametrically opposed to the conventional traditions of experimental economics. But that is another story and for another time.

[3]Herbert Scarf and the late Herbert Simon will, of course, have some valid objections to such an assertion.

[4]But, of course, they can also be recursive in the more general sense of *constructive* mathematics of any variety - i.e., Bishop-style constructivism, 'Russian' constructivism, traditional 'Brouwer-Heyting' constructivism, etc.

[5]As mentioned also in the previous footnote. I have, however, in mind the three kinds of constructive mathematics that will be mentioned in the next sections: Brouwerian intuitionistic constructivism, Bishop-style constructivism and Russian constructivism, although there are others (for example Aberth-style constructive mathematics which is a kind of hybrid of computable analysis and Russian constructivism). For reasons of space I omit mentioning the constructive traditions of non-standard analysis.

interaction – possibly also meta-rules for a subclass of rules to allow them to change during the dynamic interactions – which are initialised to a particular configuration that is in some sense faithful to an aspect of economic theory. A dynamic is implemented when particular parameters are chosen, from a feasible set, to set in motion the rules of interaction.

This interpretation of the research strategy – or is it better described either as a *research programme* of the Lakatosian variety or, perhaps, even better, as a research tradition in the Laudan sense – implies two important considerations. The first is obvious: agent-based modelling is intrinsically dynamic and, hence, one can interpret them as dynamical systems in the formal sense. Following on from this interpretation there is, then, a secondary inference, since the caveat *computational* seems usually to be added to characterize the practice of agent-based modelling – to refer to it with the acronym *ABCM*, i.e., *a*gent-*b*ased *c*omputational *m*odelling. The secondary inference, therefore, is that these dynamical systems are, almost without exception[6] , discrete and, hence, can be studied on appropriate lattices as generalized *cellular automata*.

The second implication is more subtle but also more pertinent to the main subject matter of this paper. In all of the exercises in agent-based computational modelling that I have studied[7] the aim seems to have been to study the apparent patterns that emerge from extensive simulations and identify them with the accepted stylised facts of standard theory. This is an inversion of normal practice, but so much the better for it. However, the attempts to identify emerging (collective) patterns implies that the system as a whole is capable, at the minimum, of *self-reproduction* – to put it more prosaically, to 'keep going'. So far as I can see, in practice there seems to be much more than 'simple reproduction'[8]; there is, almost always, a dynamical evolution into basins of attraction that signal something akin to *evolution* (or at least *expanded reproduction* – i.e., growth).

In other words, starting from a collection of simple (measured in a precise quantitative and qualitative senses) and simply connected finite automata, which can, without loss of generality, be assumed arranged on a lattice, there is an evolution into a collective automaton that is capable of self-reproduction (and, in general, more[9]). This is a tacit assumption, and I believe a justified one. But it is, of course, theoretically unsatisfactory and adds a whiff of *ad hockery* to the research enterprise.

The other aspect of this evolution is the claim that it leads to a so-called emergent, complex, aggregative pattern that mimics real-time data. One of my concerns in this paper is to make precise a notion of complexity that can be associated with the evolved collective automaton that is capable, at least, of self-reproduction. To do this I shall

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[6]I do not know of any exceptions.

[7]And I may not even have scratched the surface of the vast and constantly growing literature on the topic so that this implication may not be quite as accurate as I wish it to be.

[8]I have not chosen this Marxian phrase quite as frivolously as it may seem!

[9]To be consistently, unfashionably and terribly Marxian about it, expanded reproduction, is the phrase I should use, again not frivolously. I could, of course, simply resort to the language of growth theory, particularly the von Neumann variants of it (cf. Velupillai, 2003a).

try to exploit the analogies between $\sum(n)$, the busy beaver function, and Chaitin's Ω[10] .

My reason for placing the discussion and description of the activities of the members of the Busy Beaver club in the context of an ABCM exercise is, therefore, also to be able to give some theoretical coherence to what may seem, to unsympathetic adherents to traditional ways of thinking, as *atheoretical* simulation mania[11].

The paper is organised as follows. In the next section I discuss, briefly and in very general terms, the concept of function as defined and used in alternative mathematical traditions. This discussion is the backdrop against which I try to understand the nature and scope of *uncomputable functions*. In §3,there is a straightforward definition and description of the activities of members of the busy beaver club and, to the extent it is formally possible, I also try to characterize them. Can the universe generated by agent-based simulations contain a busy beaver? If so, how will the evolution of the dynamics of agent-based modelling simulations be tamed? If not, how can the agent-based computational model continue its dynamic evolution without dying away? Such questions, and related ones, should be the kind of questions one can raise on the basis of the results and exercises of this section. In §4 I indulge in some formal exercises: proofs and paradoxes inherent in proofs. In the concluding section, §5, I suggest some strategies, having been humbled by the busy beaver, for humbling it.

2. Functions

> "If we take algorithms and data structures to be fundamental, then it is natural to define and understand *functions* in these terms. The phrase 'non-computable function' then becomes problematic, and the understanding which sees *almost all* functions as non-computable becomes mysterious. If a function does not correspond to an algorithm, what can it be?"
>
> Newcomb Greenleaf (1991), p.224 (italics in original).

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[10]A fuller discussion of the *Complexity Vision in Economics* can be found in Velupillai, 2003b.

[11]There are some adventurous claims and attempts in Corazza and Perrone and Delli Gatti et.al., (cf. Luna & Perrone, op.cit, chapters 6 & 7 respectively). There is hardly any connection between the dynamical model of the theoretical part and the SWARM simulation model used by Delli Gatti et.al. In fact a serious dissection of the constraints imposed on the basis of theoretical considerations – economic *and* mathematical: for example assertions about approximating a continuum assumption (p.p.168 & note 7, p.184) – in the first part of their paper will show that they cannot be satisfied by a simulation model of the SWARM type. Bruun's perceptive observation (Luna & Perrone, op.cit., p.29) pre-empts any need for me to elaborate on the lack of any connection between an ABCM philosophy and the model and modelling strategy adopted in Corazza and Perrone. I do not wish to have these negative remarks misconstrued. I make them because I am afraid the easy availability of simulation platforms may lead to careless applications and the tight connection between theoretical model construction and validation by simulation may be forgotten by overenthusiastic *ad-hockeries*.

What is a function? A programmer, not necessarily confined to object oriented programming, is more than likely to answer this question in the language of conventional discourse by saying that 'a function is a named, and often enumerated, collection of statements that performs a specific task'. The emphasis being on 'performing a task'. This answer retains fidelity with the ordinary understanding of the meaning of the word 'function'.

Historically, at least in mathematics, the meaning of the concept function was intimately tied to the notion of a rule, a procedure, a set of instructions to perform a task. In particular, a function f was supposed to enable us to calculate, given a real number x, another real number $f(x)$ such that whenever x $\lesseqqgtr$ y, then f(x) $\lesseqqgtr$ f(y). Following this tradition, in economic theory, particularly in its mathematical modes, it is often explicitly, and always implicitly, assumed that a function is defined and characterized in the (Dirichlet-) Kuratowski[12] sense:

DEFINITION 7. *A function $f : A \to B$ is any subset $f \subseteq (A \times B)$ which satisfies: $(\forall x \in A)\,(\exists y \in B)$ s.t $(x, y) \in f \& (x, y') \in f \Rightarrow y = y'$;(A and B are the domain and range sets, respectively).*

However, this definition makes complete sense only within *set theory.* The definition has severed the connection with the meaning attributed to the word in ordinary discourse: there is little sense in which it can be understood to 'perform a task'. The idea of a *rule*, encapsulated within the definition of the concept of function has disappeared. In a sense, all alternative mathematics can be viewed as an attempt to resurrect this classical notion of a function as a rule to perform a task.

For example, the following 'formulas' for computing the square of two numbers defined on the reals are equivalent in the 'function as a graph' definition implied by the above (Dirichlet-)Kuratowski characterization:

$$\begin{aligned} f(x,y) &= (x+y)^2 \\ g(x,y) &= x^2+2xy+y^2 \end{aligned} \tag{2.1}$$

However, as tasks to be performed, say on a computer via a simple program, they result in different sets of instructions (cf. Moschovakis, 1994, p.41). Whether the notion of a function that is based on 'performing a task' can be represented in set theory in such a way as to capture its full intuitive content remains an open question.

Despite this elementary fact, economists cavalierly move from one domain of definition of functions to another, with princely unconcern for the logical, computational and descriptive fallacies that may well be inherent in any such transfer.

[12] I add the name Dirichlet to the more accepted naming which, justly, credits Kuratowski, for historical reasons. So far as I know the first 'rigorous' definition of a *function*, acceptable to the mathematical community, was given by Dirichlet for an open, continuous, interval (cf. Hobson (1927: p.274): 'It thus appears that an adequate definition of a function for a continuous interval (a,b) must take the form first given to it by Dirichlet'. (cf. also J.P.G.L Dirichlet, 1889-1897, Vol.1, p. 135). The only economist, as far as I know, who has raised explicit doubts about the adequacy of the Dirichlet definition of functions for economics is Georgescu-Roegen (1971).

There is, however, a way out; indeed several ways out of this seeming dilemma. Using the kind of *type theory* developed by Martin-Löf (1972)[13] it is possible to reinterpret sets as problem descriptions. In this case, then, the interpretation extends to viewing sets as a specification of a programming problem. In the former case, the elements of the sets are the *possible* solutions to the problem. In the latter case, the elements of the set are the *programs that satisfy the specification.* These ideas and interpretations go back to Kolmogorov's masterly interpretation of Intuitionistic Logic (Kolmogorov, 1932)[14].

I do not see any difficulty in reinterpreting any work of formalization implemented in object-oriented programming languages such that the sets and the elements of sets in discourse are problem descriptions and possible solutions, respectively. The novelty lies in understanding that such a reinterpretation carries with it an adherence to some kind of constructive mathematics, even if not a full basis in intuitionistic logic.

Thus the class of primitive objects, and their data structures, considered as functions in different mathematical traditions, are not, in general, equivalent to each other. There are no uncomputable functions in any variety of constructive mathematics. The issue does not even arise in classical mathematics. It is, in fact, only in recursion theory that the distinction between computable and uncomputable functions is explicitly made. This is the unfortunate reason for the uninitiated and the uninformed to focus on the apparent fact that the computable functions are only (sic!) a subset of a more general class of functions. *A fortiori*, the uncomputable functions are pathological, but even this only from the particular and idiosyncratic perspective of the recursion theorist.

In defining and describing the activities of the members of the Busy Beaver Club in the next section, I shall begin with the jaundiced vision stated at the end of the above paragraph. However, I shall try to disabuse myself, and those others who share such a view, with an alternative interpretation of the activities of the members of the Busy Beaver Club. The rest of the discussion in this section is made with a view to provide a background to that alternative vision which will, I hope, convince the jaundiced, the sceptics and all others that there are no pathologies involved in the perfectly idiosyncratic activities of the Members of the Busy Beaver Club.

Let me return to the initial question: *what is a function?* How do different mathematical traditions confront the task of answering this question? All traditions[15]

[13]Martin-Löf's type theory was developed with the principal aim of clarifying constructive mathematics.

[14]In my own recent work on formalizing Herbert Simon's thoughts on problem solving I have had occasion to use Kolmogorov's interpretation on Intuitionistic Logic, but I had been groping towards this usage when struggling to complete the manuscript of my *Ryde Lectures* (cf. Velupillai, 2002a and pp.181-2, in Velupillai, 2000). When preparing the *Ryde Lectures I* conjectured that an algorithmic logic – i.e., a programming language - could be developed from Kolmogorov's results. I did not know at that time of Martin-Löf's impressive work in this direction (although I was fully conscious of his work on Kolmogorov Complexity).

[15]Although it may appear slightly paradoxical, I have no hesitation in including nonstandard analysis in the constructive tradition. Ever since Leibniz chose a notation that was conducive to

, with the notable exception of what, for want of a better name, I shall call 'classical mathematics', try, each in their own way, to retain fidelity to the ordinary meaning of the word 'function' in their specialised characterizations within each discipline and to the classical idea of function as a rule to perform a task.

Thus, in *Bishop-style constructive mathematics*, all existence proofs are constructive in the precise sense that every proof can be implemented, in principle, as an algorithm in a computer to demonstrate, by explicit construction, the object in question. As a by-product of such a discipline, all functions are required to be *uniformly continuous in each closed interval*. In other words, if mathematics is about proving theorems and if proofs are to be constructive – i.e., performable, at least in principle, as tasks described by a sequence of explicit instructions – then functions must be characterized in a certain, precise, way. Thus, Bishop-style constructive mathematics retains fidelity with the ordinary meaning of the concept of function by endowing it with certain *mathematical properties* – i.e., uniformly continuous in each closed interval – such that when they are used in the activities of a mathematician – proving theorems – they will facilitate the 'performance of tasks'. This ingenious approach obviates any need for a rule to determine an arbitrary real number and, in one fell swoop, also avoids invoking anything like a Church-Turing Thesis.

On the other hand, in that variant of constructive mathematics known as *Brouwerian Intuitionistic mathematics*, the starting point is what is called 'free choice sequences' – where a *rule* for determining a real number was a result of *free choices* by an autonomous human intelligence, independent of the strictures of the undecidable disjunctions of classical logic. This assumption implied that in Brouwerian intuitionistic mathematics *all functions from the reals to the reals are continuous*. Here, too, starting from a metatheoretic assumption, *construction* of mathematical objects by 'free choice sequences', based on what Brouwer considered was the domain of activity of the *mathematician* – his or her autonomous intelligence – one was led to consider a characterization of functions that obviated the need to invoke rules. Hence there was no need to appeal to a class of exogenous *rules* by way of accepting a 'thesis', as in recursion theory. The functions utilized in Brouwerian intuitionistic mathematics,

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computation in his approach to the differential and integral calculus, a notation that has survived even in the quintessentially non-computational traditions of classical real analysis, non-standard analysis has been developed with a clear view of applicability from the computational point of view. Indeed, the first modern model of non-standard analysis by Schmieden and Laugwitz (1958), (i.e., after the neglected work by Veronese (1891) at the end of the 19th century), was essentially constructive. An excellent, readable and pedagogical exposition of these issues is given in Palmgren (1998). As for why Veronese's work remained unrecognised, in spite of extensions and genralizations by some of his eminent pupils (Levi-Civitta, more than anyone else), I have my own explanations. One was that Veronese's more famous Italian contemporary, Peano, dismissed the work as lacking in rigour; the other was Russell's exaggerated assertion, a few years later, that the triple problems of the *infinitesimal, infinity* and the *continuum* had been 'solved' and there was nothing more to be said about them. Peano's objection turned out to be temporary; Russell's invalid. I cannot go into more details here.

therefore, retained fidelity to the ordinary meaning of the concept function, but by coming to it in a roundabout way.

The simplest and the most intuitive way to characterize the Russian constructivist school is to note that they append something like a Church-Turing Thesis to an otherwise unmodified Bishop-style constructive system. This gives them some freedom to expand the class of functions that can be used in constructions.

Finally, there is the class of computable functions considered in recursion theory. The most direct way of describing these functions is to say that they are that *subset* of the functions defined in classical mathematics, which can be implemented on a *Turing Machine*. Then, invoking the Church-Turing Thesis, one identifies them, depending on the aims of the analysis, as the class of *partial recursive functions or Church's λ-definable functions*, etc. Then, by way of elementary counting arguments it is shown that there are 'only' a countable infinity of *Turing Machines* and, hence, also of *partial recursive functions* implying thereby that the complement of this set in the class of all functions considered in classical mathematics contains the uncomputable functions.

It is at this stage that much discontent arises[16] because the mischievous adherents of a classical mathematical approach to economic theory would, in seeming innocence, ask the heretic: 'If there are so many uncomputable functions, indeed as many as an uncountable infinity, could you kindly show me a simple one and, in particular, one which makes economic sense – i.e., one that is used routinely in economic theory.' The stumped heretic gropes around for an intelligent answer, which he or she feels in the guts must be 'out there', but eventually has no alternative than to rehash a diagonal argument or, sheepishly, start describing the activities of the Members of the Busy Beaver club. This, of course, plays straight into the hands of the complacent idealist, residing in the marble halls of the citadel of traditional economic theory.

There is, however, another way to approach the problem of uncomputability. Taking a cue out of the strategies adopted by Bishop-style constructivists, Brouwerian Intuitionists and the Russian Constructivists, the computable economist can try to abandon the starting point of using classical mathematics as a benchmark from which to define computability. It is this strategy I shall adopt to bring into the fold of computable functions even those exotic activities of the Members of the Busy Beaver Club. Just as the starting point of the Bishop-style constructivists was a class of functions all of whose members were uniformly continuous on every closed interval or that of the Brouwerian Intuitionists was the class of functions such that each member mapped members of the reals to reals continuously, the computable economist's starting point should be that *all functions are computable*.

But there is another important sense in which the computable economist should mimic the strategies of the Bishop-style constructivists and the Brouwerian Intuitionists. Their choice of a class of functions – rules – was based on a prior philosophy

[16]My disquiet arose at this point and I was much gratified to know, some time later, that this was also the origin of a similar uneasiness experienced by my friend, Francesco Luna.

of the kind of activity that they considered appropriate for the activity of the mathematician: proof by construction. Similarly, the computable economist's focus is on computation. Hence the impetus given by the exotic behaviour of the Members of the Busy Beaver Club should encourage the computable economist to re-examine the elementary building blocks of the discipline: data structures and rules. Now, clearly, nothing can be done about the rules of recursion theory, at least for the moment: the Church-Turing Thesis dominates. It will, therefore, not come as a surprise if I release the plot and announce in advance that the key to bringing into the fold of the computable those exotica generated by the members of the Busy Beaver Club will be a re-examination of the kind of domain and range that one must use for the data structures of the computable economist.

3. The Busy Beaver: Definitions and Discussion

> "With the possible exception of bees, beavers are the busiest animals alive. All day they ply quiet northern waters bringing twigs and branches to their dam. It was undoubtedly this behaviour that led Tibor Rado of Ohio State University to name a certain Turing-machine problem the Busy Beaver Game."
>
> Dewdney (1984), p.10.

So far as I know, all the known discussions, formalizations and results about members of the Busy Beaver Club have been in framed in terms of *Turing Machines* (henceforth, TMs)[17] . Moreover, the TMs have been characterized either by their program card equivalents, as in the pioneering papers by Rado (1962) and Lin and Rado (1965)[18] ; or by the flow-graph definition popularised in the classic textbook of Boolos and Jeffrey (1989). I shall follow the former tradition.

Consider the following 3-card TM (3-state TM)[19]:

The meaning of the above table is as follows. Card (I) instructs the TM, when its tape head reads a blank (#), to overwrite it with a '1', move the tape head one square to the left and begin reading card (II). If, on the other hand, the tape head is

[17] A more formal definition of *Turing Machines* is given below, at the end of this section.

[18] "... [A] Turing machine is not a machine, but rather a program (set of instructions) spelled out in a fixed format ... . The instructions are specified on a finite number of 'cards'; ... The term 'card' seems preferable to the term 'state' or 'internal configuration', since the idea of a Turing machine is not dependent upon physical computers." (Lin & Rado, op.cit, p.196)

[19] I constructed this 3-card TM simply by transposing the shift instructions in the analogous TM table in Machlin and Stout (1990), p.87; i.e., by changing the Left shifts to Right shifts and vice versa. It was only at a later stage that I discovered that this 'transposed' 3-card TM is exactly identical to the one given in Rado's original paper (op.cit., p.878)! I do not, of course, know whether Machlin and Stout constructed *their* 3-card TM by such a 'symmetric' transposition of the shift instructions of the original Rado table! Rado's original discussion does not give, explicitly, the sequence of instantaneous configurations. This accidental discovery led to the conjecture that only half the finite number of Busy Beavers need be discovered for any given n-card TM.

	$\#$	1
(I)	$1L\,(II)$	$1R\,(III)$
(II)	$1R\,(I)$	$1L\,(II)$
(III)	$1R\,(II)$	$1L\,(\varnothing)$

TABLE 1. 3-card TM-A

$InitialConfiguration$	.......$\#_{(I)}$.......
$Shift$ 1 :	.......$\#_{(II)}1$.......
$Shift$ 2 :	.......$11_{(I)}$.......
$Shift$ 3 :	.......$11\#_{(III)}$.......
$Shift$ 4 :	.......$111\#_{(II)}$.......
$Shift$ 5 :	.......$1111\#_{(I)}$.......
$Shift$ 6 :	.......$1111_{(II)}1$.......
$Shift$ 7 :	.......$111_{(II)}11$.......
$Shift$ 8 :	.......$11_{(II)}111$.......
$Shift$ 9 :	.......$1_{(II)}1111$.......
$Shift$ 10 :	.......$\#_{(II)}11111$.......
$Shift$ 11 :	.......$11_{(I)}1111$.......
$Shift$ 12 :	.......$111_{(III)}111$.......
$Shift$ 13 :	.......$11_{(\varnothing)}1111$.......

TABLE 2. Sequence of Instantaneous Configurations of the activities of the 3-card TM-A

set against a square on the tape with a '1' on it, then leave it as it is, shift one square to the right and begin reading card (III). Similar meanings are to be attributed to all instructions cards for TMs, of which there are three for the above 3-card TM.

Now initialise the above 3-card TM on a blank tape (the blank squares on the tape, infinite in both directions, are denoted by 'dots' in sequence of *instantaneous configurations*[20] given below) and let it commence its activities. The result is the following sequence of instantaneous configurations:

The sequence of instantaneous configurations of the activities of the above 3-card TM is given in the following diagram:

As we can see, this 3-card TM, initialsed with card 1 facing an infinite blank tape, makes 13 shifts and halts after writing six 1s.

Consider, now, the following 3-card TM:

The sequence of instantaneous configurations of the 3-card TM_B is the following:

This 3-card TM_B, initialised like the previous one, makes 6 shifts and halts after writing three 1s.

[20]In Lin and Rado (op.cit, p.197) this is called an '*operating record*'.

	#	1
(I)	$1L\,(II)$	$1L\,(\varnothing)$
(II)	$1R\,(III)$	$1L\,(II)$
(III)	$1R\,(I)$	$1L\,(III)$

TABLE 3. 3-card TM-B

$.......\#_{(I)}.......$
$.......\#_{(II)}1.......$
$.......11_{(III)}.......$
$.......1_{(III)}1.......$
$.......\#_{(III)}11.......$
$.......1_{(I)}11.......$
$.......\#_{(\varnothing)}111.......$

TABLE 4. Sequence of Instantaneous Configurations of the activities of the 3-card TM-B

	#	1
(I)	$1L\,(II)$	$1L\,(\varnothing)$
(II)	$1R\,(III)$	$1L\,(II)$
(III)	$1R\,(I)$	$1R\,(III)$

TABLE 5. 3-card TM-C

It is easy to verify that the sequence of instantaneous configurations does not terminate; there is, so far as can be inferred from an inspection of the sequence of instantaneous configurations to any length that one wishes to examine, a recurrence of the following pattern:

What are the lessons to be learned and the questions that can be asked from a consideration of the activities of the above three 3-card TMs?

3.0.1. *Queries and Observations.*

(1) TM_A and TM_B *halt* after writing a certain number of 1s;
(2) For all we know, TM_C is a *'never-stopper'* (Lin and Rado, op.cit., p.204), repeating the sequence shown in Fig.3;
(3) The total number of 3-card TMs is finite and is given by[21] : $[4(3+1)]2x3 = 16,777,216$;
(4) *What is the maximum number of 1s any halting 3-card TM can write?*
(5) Is there a method – i.e., algorithm - to determine this maximum number for any given n-card TM?

[21]The formula for determining the total number of n-card, 2-symbol, TMs is $[4\,(n+1)]^{2n}$.

$$\begin{array}{c}
.......\#_{(I)}....... \\
.... \\
.... \\
. \\
. \\
.......1_{(II)}111....... \\
.......\#_{(II)}1111....... \\
.......1_{(III)}1111....... \\
.......11_{(III)}111....... \\
.......111_{(III)}11....... \\
.......1111_{(III)}1....... \\
.......11111_{(III)}....... \\
.......11111\#_{(III)}....... \\
.......111111\#_{(I)}....... \\
.......111111_{(II)}1....... \\
. \\
. \\
.......1_{(II)}111....... \\
.......\#_{(II)}1111.......
\end{array}$$

Table 6. Sequence of Instantaneous Configurations of the activities of the 3-card TM-C

(6) *How many shifts, S, would a TM that writes such a maximum number make before halting and is there a method to determine this number for any given n-card TM?*
(7) Does halting and 'never-stopping' recurrence exhaust the possible patterns of a sequence of instantaneous configurations of 3-card TMs? In other words, are there non-recurring 'never-stoppers'?
(8) If the answer to the second part of 7 is in the affirmative, do such non-recurring 'never-stoppers' encapsulate within themselves a code for 'self-reproduction'?
(9) Is there a method, a rule, an algorithm, to partition the totality of the 3-card TMs, i.e., the 16,777,216 TMs, into, say, the three classes of recurring 'never-stoppers', non-recurring 'never-stoppers' and halting machines?
(10) If there is such a method, is it applicable in general – i.e., to partition the sequences of instantaneous configurations of any given n-card TM into distinct classes of recurring and non-recurring 'never-stoppers' and halting machines?
(11) Clearly, the answers to 4 and the first part of 6, call the values, *pro tempore*, $\sum(3)$ and $S(3)$ respectively, are determined numbers. $\sum(3)$ is, after all, *the maximum of a finite set of numbers*. Once this is determined, the corresponding TMs can be implemented on a blank, two-way infinite tape,

and the number of shifts up to the halting state can simply be counted to give the value of $S(3)$.

(12) Extrapolating from the reasoning for $\sum(3)$ and $S(3)$, can we say that $\sum(n)$ and $S(n)$ can also be determined – and, if so, is there a rule, an algorithm, to calculate them? After all, $\sum(n)$ is the maximum of a finite set of numbers!

The simpler answers are the following. $\sum(3) = 6$ and $S(3) = 13$. Thus, TM_A is a member of the Busy Beaver Club (cf. Def 2, below). This was determined and clearly demonstrated in the classic Lin-Rado paper (Lin and Rado, op.cit.). Clearly, using *Rice's Theorem* and the theorem of the *unsolvability of the halting problem for TMs*, it is easy to show that the answers to queries 9 and 10 are negative: i.e., there is no general purpose algorithm that can determine, given any n-card TM, whether it belongs to the halting class or to one or the other of the two 'non-stopping' classes. The answers to 5, the second part of 6 and the second part of 12 are all summarized in the formal 'Proposition' to be stated and proved, below: i.e., $\sum(n)$ and $S(n)$ are uncomputable. The answer to 7 is also clear: the three classes of halting, recurring 'non-stoppers' and non-recurring 'non-stoppers' exhaust possible patterns for the sequences of instantaneous configurations.

I conjecture that the answer to 8 is in the affirmative. I shall return to this question in the concluding section. That leaves 11 and a part of 12 with a need for clarifying comments. Let me backtrack just a little so that the clarifying comments can be given with some perspective.

Tibor Rado introduced and defined a Busy Beaver in his classic paper: *"On Non-Computable Numbers"* (Rado, op.cit). Till about the time Rado's paper was published, the standard way of demonstrating the existence of non-computable functions was to diagonalize out of the collection of enumerable computable functions. Rado explicitly stated the basic principle he used to construct a non-computable function:

> "The examples of non-computable functions to be discussed below will be well defined in an extremely primitive sense; *we shall use only the principle that a non-empty finite set of non-negative integers has a largest element.* Furthermore, we shall use this principle only for exceptionally well-defined sets;..."
>
> ibid, p.877; italics added.

What is the connection between this 'extremely primitive case', based on a simple and intuitively acceptable principle and the activities we defined above? It is the following. Suppose an arbitrary TM is started scanning a square on a blank, infinite, tape, halts after writing p (not necessarily consecutive) 1s, then the *productivity* of the TM is p. However, any TM, initialised similarly, that does not halt – even if it does 'write' some 1s as it continues to operate, is defined to be of *productivity* 0. Thus we can define the following *total number-theoretic function,* $\sum(n)$ *for the class of n-state* TM_s*, denoted by* TM_n:

DEFINITION 8. $\sum(n) \equiv$ *Maximum productivity of any member of* TM_n *and is called the Busy Beaver Function.*

Definition 9. *Members of the Busy Beaver Club are those n-card TMs (for any given n), when initialised on a blank, two-way infinite tape, that write the maximum number of 1s (not necessarily consecutive) on an otherwise blank tape and halt. The Busy Beaver function is $\sum(n)$ and the shift function associated with this is given by $S(n)$.*

We know, from the above discussion, that the number of n-card TMs, $\forall n$, is a finite integer value. Hence, $\sum(n)$ is, obviously, a total number-theoretic function. It follows, therefore, that $\sum(n)$ *is the largest number in a finite set of natural numbers* and the finite set of natural numbers, in turn, is *the set of productivities of n-card TMs.* This, then, is how Rado uses *the principle that a non-empty finite set of non-negative integers has a largest element.*

How, from this intuitive principle, does one go on to construct a non-computable function *without using a diagonal argument.* For Rado explicitly states that he eschews any reliance on this old workhorse[22]:

> "It may be of interest to note that we shall not use an enumeration of computable functions to show that our examples are non-computable functions. Thus, *in this sense*, we do *not* use the diagonal process."
>
> ibid, p.877; italics added.

In fact the next step taken by Rado, after defining *productivity* and $\sum(n)$, is to invoke, implicitly, the implications of the *Berry Paradox*, in the special context of computable functions as TMs[23] . The Berry Paradox deals with the notion of *finite definability* and is *semantical* in nature. Therefore, it is possible to describe the construction of non-computable functions using a simple programming language.

Proposition 8. $\sum(n)$ *- and a fortiori* $S(n)$ *- is uncomputable.*

I shall give a slightly unconventional proof of this well-known proposition, in the next section, in such a way that makes it possible to exploit analogies between $\sum(n)$ and Chaitin's Ω.

I broached the theme of emergence in the opening pages of this paper. It is appropriate, at this point, to return to that topic. At a very general level of discourse, there seems to be some consensus in stating that emergent behaviour is when 'interesting'

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[22]However, an eminent scholar of Busy Beaver problems asserts the contrary

> "Using essentially a diagonal argument he demonstrated that if f is some computable function then there exists a positive integer n such that $\sum(k) > f(k)$ for all $k > n$." (Brady,1994,p.239;italics added):

Since Rado invokes, albeit implicitly, the Berry Paradox, Brady's remark cannot be quite correct, except by some considerable stretch of the imagination.

[23]Rado, via the *Berry Paradox* to non-computable functions; Gödel via the *Liar and Richard Paradoxes* to incompleteness; Chaitin via the *Berry Paradox* to algorithmic complexity; Turing via *Cantor's Procedure* to uncomputability; such are the ways in which great mathematical sagas have been grounded in paradoxes.

global behaviour results from 'simple' local interactions[24] . Furthermore, if the 'simple' local interactions are defined computationally, then one refers to the 'interesting' global behaviour as an *emergent computation*. Stephanie Forrest (1990, p.2) has tried to characterize the elements that may define *emergent computation* in terms of the following three constituents:

- A collection of individual algorithmic agents; i.e., agents whose local behaviour is stipulated by precise rules given as explicit effective instructions.
- Interactions among the collection of individual agents, stipulated in the above effective instructions, which may lead to global patterns, called *epiphenomena*.
- An interpretations of the epiphenomena as *computations* or *as basins of attraction of dynamical systems*.

The crucial point to observe is that the instructions are implemented at a much 'lower' level – essentially at a quantitatively different level altogether – than the level at which the epiphenomenona occur. Can the activities of n-card TMs be interpreted as emergent computations? Clearly, the answer is in the affirmative. Why? Consider, first, each of the n cards. Each of the cards can be considered as an individual agent with explicit effective instructions stipulated in the cards. Secondly, the individual cards are connected to each other via various transfer principles. Indeed, the instructions can be restated as (computable) coupled transition functions in the sense of a mapping using the standard quadruple definition of a TM:

Q:	finite number, say n, of instruction cards;
$q_0 \in Q$:	a pre-specified initial card;
$q_T \in Q$:	a pre-specified terminal card or instruction;
Λ:	input (and, usually, also output) alphabet (in our examples it is a two-symbol set: <#,1>);
L:	Left shift of the reading head along the two-way infinite input/output tape;
R:	Right shift of the reading head along the two-way infinite input/output tape;
γ:	No shift;

Definition 10. *Given the fiexed input alphabet* Λ, *a Turing Machine is a quadruple:*

$$TM \equiv \{Q, \delta, q_0, q_T\} \tag{3.1}$$

Where the (partial)[25] transition function δ:

[24] I am not sure how 'interesting' and 'simple' can be given rigorous formal definitions.

[25] i.e., not necessarily defined over the whole domain

$$\delta : Q \times \Lambda \to Q \times \Lambda \times \{L, R, \gamma\} \tag{3.2}$$

That 3.2 is a mapping in the conventional sense is obvious and the sequence of values taken by the action of δ is the sequence of instantaneous configurations. Hence, the sequences of instantaneous configurations, viewed as a whole, can be interpreted as trajectories of dynamical systems, those defined by the collection of coupled transition functions. The difficult question is whether these trajectories can be characterised in terms of their basins of attraction[26].

In other words, *an appropriately chosen n-card TM can simulate the behaviour of any ABCM model*, on any suitable platform! The level of disaggregation determines the value of n. The nature and scope of individual explicit effective instructions determines the degree of coupling. The question is whether the emergent computation that gives rise to epiphenomena can be tamed? From the complex behaviour even of simple TMs it does not seem likely that it will be easy to discipline the global dynamics of even moderately realistic local interactions.

The key lesson for practitioners of ABCM modelling, from a study of the activities of Busy Beavers, seems to be the following. Since $\sum(n)$ and $S(n)$ are uncomputable, even though *'exceptionally well-defined by current standards'* (Rado, op.cit, p. 877) in the sense of being based on simple, intuitively acceptable, definitions at the level of individual instructions, and are clearly both a constituent and a cause of the complexity in the epiphenomena, more effort should be devoted to characterising these latter phenomena in terms of computable properties. I suspect, on the basis of the emergence of functions like $\sum(n)$ in exceptionally simple situations, that the emergent phenomena in ABCM models contain computably intractable and undecidable phenomena. I do not think it will be too difficult to demonstrate, by construction, the validity of this conjecture. The strategy would be to work with, and be on the look out for, epiphenomena residing in *recursively enumerable sets* that are **not** *recursive.* Put more concretely, the problem to be tackled will be the task of taming uncomputable trajectories generated by computable initial conditions. It is not an unsolvable problem.

[26] The analogy goes, of course, both ways: i.e., one can interpret any dynamical system and its feasible trajectories as a TM and its collections of instantaneous descriptions. Then halting configurations correspond to limit points, loops and recurring 'non-stoppers' to limit cycles, particular classes of non-recurring 'non-stoppers' to strange attractors. However, the class of nonrecurring 'non-stoppers' encapsulate a broader class of dynamic behaviour than the dynamics of strange attractors. The activities of a Busy Beaver, for example, cannot be encapsulated in the definition of any known dynamical system whose basins of attraction can be formally characterised. I have dealt with some of these issues in Velupillai (1999).

4. The Busy Beaver: Proofs and the Paradoxes

> "... [T]here is the basic fact of the noncomputability of $\sum(n)$, which implies that no single finite computer program exists that will furnish the value of $\sum(n)$ for every n.
>
> *In the absence (at present) of a formal concept of 'noncalculability' for individual well-defined integers*, it is of course not possible to state in precise form the conjecture that there exist values of n for which $\sum(n)$ is not effectively calculable."
>
> Lin and Rado (1965), p.212; italics added.

Is there a formal concept of *randomness* 'for individual well-defined integers'? There is and it is provided by *Algorithmic Complexity Theory.* If it is possible to define the randomness of an integer why is it not possible to define, formally, the "concept of 'noncalculability' for individual well-defined integers"? My basic conjecture is that the 'noncalculability' of Rado's $\sum(n)$ is exactly analogous to the randomness of Chaitin's Ω. To pursue this analogy is not the main aim of this paper (but cf. Chaitin, 1990, Part II, pp.80-82 and Velupillai, 2003b) although I shall make some tentative comparisons in the concluding section. Chaitin constructed his uncomputable and random Ω, exploiting a form of the *Berry Paradox*; I shall, however, exploit it for deriving the crucial contradiction in proving the uncomputability of $\sum(n)$. The reason for emphasising this link between Chaitin's Ω and Rado's $\sum(n)$ via the *Berry Paradox* is, partly, to make a point of the status of proofs by contradiction when impredicative definitions are involved.

A few unnecessary cobwebs must, first, be cleared away. As I pointed out in footnote 20, there are those who claim – even eminent 'Busy Beaver scholars' - that a variant of the diagonal method was used to 'construct' $\sum(n)$, despite an explicit statement to the contrary by Rado himself (op.cit, p.877). Then, there are equally eminent textbook writers who assert that Rado's construction of the Busy Beaver was based on *Richard's paradox*[27]. Neither of these assertions are quite correct. Rado's construction uses, albeit implicitly, the *Berry paradox*, which being semantical is not directly related to the more obviously 'diagonal' based Cantor and Russell logical paradoxes.

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[27]Thus, Neil Jones (1997), in an otherwise exceptionally well-written and admirably pedagogical textbook on 'Computability and Complexity' states (pp. 16-7; second set of italics added):
"The busy beaver *function* ..., due to Rado and *related to the Richard paradox*, is mathematically well-defined, but based on certain reasonable assumptions about the language used to express computation"

Two minor caveats may be called for to make this assertion more acceptable. Firstly, it is not $\sum(n)$ that is 'related to the Richard paradox but the method of proof; secondly, in a convoluted way, it is not incorrect to state what Jones has asserted simply because the Richard paradox is related to the Berry paradox, since they are both semantic in origin (using Ramsey's distinction between logical and semantic paradoxes; cf. Ramsey, 1926 and Kleene, 1967, esp. pp. 188-190).

So far as I know, proof by contradiction has been the only way that the uncomputability of $\sum(n)$ has been 'rigorously' demonstrated. Even in proofs where there seems to be an explicit 'construction', for example in the well-known and clear demonstration in Boolos and Jeffrey (op.cit, especially chapter 4), it is, in reality, by way of a 'thought experiment' to justify the assumption of the contrary to the hypothesis in the theorem. I am never sure that such proofs convey the real meaning of a theorem, particularly when the theorem asserts that something cannot be constructed, decided or computed. Therefore, before I myself give a 'proof by contradiction' of the uncomputability of $\sum(n)$, it may be useful to discuss the intuitive content of the meaning of this strange[28] result.

The intuitive reasons normally given to explain the uncomputability of $\sum(n)$ is that it 'grows' too fast to be computed by the standard operations on intuitively acceptable and formally computable elementary functions. A brilliant and graphic description of this idea is given by Dewdney in his fine expository piece on Busy Beavers:

> "The function $\sum(n)$ has an extraordinary property: it is not computable. It simply grows too fast. From the first four values of $\sum(n)$ – namely 1,4,6 and 13 – it might seem that the rate of growth is only moderate. .... On the other hand, [there is] a 12-state machine that generates so many 1's that the number must be expressed by the following mind-boggling formula:
>
> $$6 \times 4096^{4096^{4096^{\cdot^{\cdot^{\cdot^{4096^4}}}}}}$$
>
> The number 4,096 appears 166 times in the formula, 162 times in the 'twilight zone' represented by the three dots. The formula can be evaluated from the top down: first raise 4,096 to the fourth power, then raise 4,096 to the power of the resulting number, then raise 4,096 to the power of that number, and so on. When you reach the bottom, multiply by 6.
>
> Anyone whose mind does not boggle when confronted by a string of 1s that long is welcome to construct an even bigger number. Write down any formula you like in which numbers are multiplied or raised to a power; you may even replace any of the numbers with n. No matter what formula you devise, for some value of n that is large enough the n-state busy beaver will produce more 1's than the formula specifies. It follows that $\sum(n)$ cannot be calculated for arbitrary large values of n. The best one can do is to calculate $\sum(n)$ for some small, fixed value of n."
>
> Dewdney (op.cit), pp. 10-11; italics added.

[28]'Strange' only because, as noted by Rado himself, $\sum(n)$ is an 'exceptionally well-defined number', in that its existence follows from a simple well-ordering principle: that a finite set of numbers has a maximum.

What is the meaning of 'growing too fast'? Is it that our physical devices for processing numbers cannot 'move' fast enough? But such a constraint is not fundamental. For example, in the case of the above number, we can, patiently, raise $4,096$ to the fourth power to get $16,777,216 \times 16,777,216$ and raise, again, $4,096$ to the power of this number. We may run out of paper or patience or we might be motivated to devise new compression devices, notational short-hands etc., to facilitate this multiplication.

But there seems another catch: "*No matter what formula [we] devise*, for some value of *n that is large enough* the n-state busy beaver will produce more 1's than the formula specifies"! Now, in our 'world' of discourse, all *formulas* are computation rules, programme specifications. Does this mean, *whatever* computation rule we may devise, *whatever* programming language we use, is not powerful enough conceptually, computationally, syntactically or semantically to tame $\sum(n)$? How, we may then ask, was randomness tamed by algorithmic complexity theory? Is there a lesson to be learned and applied from that experience?

There is, however, an almost exact analogy, in the history of classical recursion theory, to the role played by Rado's $\sum(n)$ vis-à-vis TMs, programming semantics and, via the Church-Turing thesis, the *partial recursive functions.* It is the place of the *Ackerman function* vis-à-vis *primitive recursion.* The analogies are uncannily similar and I shall pursue it to elucidate the difficulties one faces with any attempt to tame $\sum(n)$.

By an enumeration of the primitive recursive functions, coupled to a simple diagonalization procedure, the 'existence' of a computable function that was total but not primitive recursive was easily shown. This was similar to the situation with the demonstration of the 'existence' of uncomputable functions via diagonalization out of an enumeration of the computable function. Rado, as I pointed out above, expressed dissatisfaction at this 'nonconstructive' existence demonstration and endeavoured to 'construct, explicitly, an intuitively acceptable function that was, nevertheless, shown to be uncomputable. Similarly, the Ackerman function was accepted as an intuitively computable function that was not primitive recursive. This led to an enlargement of the class of intuitively acceptable computable function and a new operation, minimalization, was introduced and the partial recursive functions were defined. Is the lesson from that important episode in the history of classical recursion theory that we can try to enlarge the rules of operation or expand the class of initial functions so as to bring into the fold of the computable also $\sum(n)$? I do not know. It may, in any case, be useful to expand on the 'story' of the role played by the Ackerman function in classical recursion theory, which I shall now do in the fashion of being a 'Whig historian'.

A version of Ackermann's Function[29] is the following number theoretic formula (4) defined on:

[29] A simple Mathematica code for implementing this function is as follows:
$Ackermann[0, n\_] := n + 1$; \
$Ackermann[m\_, 0] := Ackermann[m - 1, 1]$; \
$Ackermann[m\_, n\_] := Ackermann[m - 1, Ackermann[m, n - 1]]$;

$$A : \aleph \times \aleph \to \aleph$$

$$A\,(0, n) \equiv n + 1 \tag{4.1}$$

$$A\,(m + 1, 0) \equiv A\,(m, 1) \tag{4.2}$$

$$A\,(m + 1, n + 1) \equiv A\,(m, A\,(m + 1, n)) \tag{4.3}$$

Example: How could we compute $A\,(2, 1)$

$$A\,(2, 1) = A\,(2, A\,(2, 0)) \text{ by 4.3}$$

Thus, we need:

$$A\,(2, 0) = A\,(1, 1) \text{ by 4.2}$$

Now we need:

$$A\,(1, 1) = A\,(0, A\,(1, 0)) \text{ by 4.3}$$

And:

$$A\,(1, 0) = A\,(0, 1) = 2 \text{ by 4.2 \& 4.1}$$

Now we can work backwards to calculate $A(2, 1) =$? To get an idea of the rate of growth of the Ackerman function and also to obtain a comparison with the rate of growth of Rado's $\sum(n)$, here are some values for $A(,)$:

$A(0, 1) = 2; A(1, 0) = 2; A(0, 2) = 3; A(2, 0) = A(1, 1) = A(0, A(1, 0)) = A(0, 2) = 3; A(4, 2) = 2256 - 3; A(5, 0) = 253; A(5, 1) = A(4, 253); \ldots..$

The idea one tries to give is that the Ackerman function grows faster than any primitive recursive function, or more accurately, the primitive recursive functions do not grow fast enough – exactly as Rado's $\sum(n)$ grows faster than any partial recursive function. Now, *why* does the Ackerman function grow so fast relative to primitive recursion? In other words, what is it that the Ackerman function does that primitive recursion does not or cannot do?

To sutdy this question, recall the following two standard definitions:

DEFINITION 11. *Primitive Recursion of Computable Functions*

Given the computable functions, $\varphi : N^{n-1} \to N$ *and* $\theta : N^{n+1} \to N$ *the computable function* $\psi : N^n \to N$ *is obtained from* φ *and* θ *by primitive recursion, whenever*

But special care will have to be taken in printing the output, even for small values of $A(m, n)$ because the number of digits becomes, literally, astronomical.

$$\psi\left(0, x_2, \ldots.., x_n\right) = \varphi\left(x_2, \ldots, x_n\right)$$

and

$$\psi\left(x+1, x_2, \ldots.., x_n\right) = \varphi\left(x, \psi\left(x_1, x_2, \ldots.., x_n\right), x_2, \ldots, x_n\right)$$

Definition 12. *Primitive Recursive Function*

The set P of primitive recursive functions are those that are closed with respect to (finite applications of) composition and primitive recursion on the Basic Functions.

Now take another look at the Ackermann Function. Is the recursion *primitive recursive?* It seems to be so - but it is not. Why not? *Too much* seems to be going on! 'Too much'- of what? Of 'recursion'? Yes – because inside the Ackermann recursions there is a *definition by induction on two variables.* Consider all pairs of natural numbers arranged as follows:

$$\begin{array}{ccccc}
(0,0) & (0,1) & \ldots & (0,n) & (0,n+1) \\
(1,0) & (1,1) & \ldots & (1,n) & (1,n+1) \\
. & . & & . & . \\
 & & & & \\
. & . & & . & . \\
(m,0) & (m,1) & \ldots & (m,n) & (m,n+1) \\
(m+1,0) & (m+1,1) & \ldots & (m+1,n) & (m+1,n+1)
\end{array}$$

In ordinary (primitive) recursion, to determine the value of f at $(m+1, n+1)$, that is, $f(m+1, n+1)$, we allow ourselves to look at values of f at places in preceding rows only, $f(x, y)$ such that $x \leq m$. This seems an arbitrary restriction: why shouldn't we allow ourselves to look at values of f at places preceding $(m+1, n+1)$ on the same row, that is, $f(m+1, x)$ for $x \leq n+1$? Moreover, nesting causes no problems; that is, we can apply f to itself, for example, $f(m+1, n+1) = f(m, f(m+1, n))$, for we are again only thrown back to previous calculations of f. To calculate any such f at $(m+1, n+1)$ we need only a finite number of values of f at places that precede $(m+1, n+1)$. When we start at $(m+1, n+1)$ there are only finitely many places to go on the same row before $(m+1, n+1)$. Then we may go to an arbitrarily distant place on a preceding row, say $(m, n+400)$. But then again there are only finitely many places on that row to which we can be thrown back, ... continuing we must eventually reach $(0, 0)$ for which a value is given.

Taking all this into account, a 'new' operation, *minimalization*[30] , was added to *composition* and *primitive resursion* to bring into the fold of the class of computable

[30]**Minimalization**:
Suppose the function $f : N^{k+1} \rightarrow N$ with $k \geq 0$ is given. We write $Mn[f]$ for the function $g : N^k \rightarrow N$

functions also the Ackerman function. The problem, however, with $\sum(n)$ vis-à-vis TMs, partial recursive functions, etc., is that no one seems to have been able to figure out a 'new' operation that can tame the activities of the Busy Beaver.

Let me now prove Proposition 8. There are any number of proofs, all variations on a single theme, best explained and presented, in my opinion, in chapter 4 of Boolos and Jeffrey (op.cit). There is a refreshingly different kind of proof, concentrating on the programming language used to represent computation in Neil Jones (op.cit), pp. 16-18. I do not wish to rehash these well-known presentations, all without exception resorting to 'proof by contradiction', but none making explicit the connection with the *Berry Paradox*. I shall therefore attempt to present an alternative perspective for which it is necessary to restate Proposition 1 in a slightly different, but formally equivalent, way.

Before I present this alternative version of Proposition 1, consider the following standard result in classical recursion theory (Moret, 1998, p.156, theorem 5.5):

THEOREM 4. *The length of the shortest program that prints n and halts is not computable.*

Compare the form and content of the above theorem with the following (Chaitin, 1995, p.4):

THEOREM 5. *The first positive integer that can be proved (relative to a given formal axiomatic system) to have the property that it cannot be specified (relative to a gien Universal Turing Machine) by a computer program with less than N bits is uncomputable.*

Next, compare both of the above theorems with the following version of *Berry's Paradox* (Russell, 1908, p.222). Consider the *least integer not nameable in less than nineteen syllables*. This refers to the particular number 111,777. But the italicised expression also names an integer and contains *eighteen* syllables! In other words, the 'least integer not nameable in less than nineteen syllables', i.e., 111,777, can be named in eighteen syllables. This is the kind of contradiction that is exploited in the proof of the above two theorems.

For example, in theorem 5, the strategy of the proof is to show that there is, in fact, a computer program of length $log_2 N + constant = N$, for sufficiently large N, which will specify the same number and hence to display a contradiction. Exactly the same strategy is employed in proving theorem 4.

Now here is my alternative statement of Proposition 8:

PROPOSITION 9 (A). *For any given n, the largest value that can be printed by a program of length (at most) n, is uncomputable.*

---

defined as:
$g(n_1, n_2, ..., n_k) = \mu m[f(n1, n2, ..., nk, m) = 0$ and such that, for all $j < m, f(n1, n2, ..., nk, j)$ is defined and $\neq 0]$, and $\mu m[f(n1, n2, ..., nk, m) = 0]$ is to mean the least natural number m such that $f(\mathbf{n}, m)$ is equal to 0, where $f(\mathbf{n}, m)$ is any $(k+1)$-ary number-theoretic function. The Greek letter μ followed by a number variable is referred to as the *least-number operator*.

Remark 1. *Clearly, Proposition 1A is a kind of converse of Theorem 4. Hence, a proof of the former will only need to mimic the strategy adopted to prove the latter. The affinity with the form of the Berry Paradox is also eveident. And, with respect to Theorem 5, if we replace the phrase 'first positive integer' with the 'largest positive integer', then the analogy with it is also clear.*

Proof. Suppose $\sum(n)$ is computable. Define an auxiliary constant-valued function f by minimalization such that, for any given m:

$$f(m) = \mu j \text{ and } \sum(j) \geq m$$

The minimalized function f returns the natural number j such that no program of length less than m prints j and halts. Clearly, the length of the program for $f \leq$ constants + the bits necessary to code m so that f can make sure that $\sum(j) \geq m$. The latter value is at most $\log_2 m$; denote the constant value as c. Then the length of the program to implement f is less than $|c + \log_2 m|$. If, now, m is chosen large enough to make sure that it is sufficiently greater than $\log_2 m$, then for such a choice, say m_0, f computes the least value j such that no program of length less than m_0 can print j. But m_0 was chosen to guarantee that the program for computing f was, in fact, less than m_0. This contradiction implies that $\sum(n)$ is uncomputable. □

Proving, by contradiction, the uncomputability of $\sum(n)$ exploits the full force and potentialities of the Berry Paradox. Where lies the *paradoxical* aspect of the *Berry Paradox*? It lies in what was called *impredicative* definitions by Poincaré, definitions that involve some aspect of self-reference where, however, the paradox arises from allowing totalities to be members of themselves. Russell, inspired partly by Poincaré, was led to develop his theory of types to tackle these paradoxes. This is not the place to go into the full and fascinating details of these issues but it may be pertinent to repeat some of Russell's pertinent observations on at least the *Berry Paradox*.

Why are we able to resort to proof by contradiction to demonstrate the validity of any proposition that is related to the *Berry Paradox*? It is, as Russell pointed out almost a century ago, because (op.cit., p.223) :

> "In the cases of names and definitions [i.e., the *Berry* and *Richard Paradoxes*], the paradoxes result from considering nonnameability and indefinability as elements of names and definitions. .... In each contradiction something is said about *all* cases of some kind, and from what is said a new case seems to be generated, which both is and is not of the same kind as the cases of which *all* were concerned in what was said."
>
> Italics in original.

More particularly, regarding the *Berry* (and *Richard*) *Paradox*:

"'The least integer not nameable in fewer than nineteen syllables' involves the totality of names, for it is 'the least integer such that all names either do not apply to it or have more than nineteen syllables'. Hence we assume, in obtaining the contradiction, that a phrase containing 'all names' is itself a name, though it appears from the contradiction that it cannot be one of the names which were supposed to be all the names there are. Hence, 'all names' is an illegitimate notion."

ibid, p.224; italics added.

I do not know of any proof of the uncomputability of $\sum(n)$ that does not invoke some version of this 'illegitimate notion'. On the other hand, Chaitin, in constructing Ω, did circumvent the illegitamacy by restricting the scope of 'nameability' or 'definability'. He achieved this restriction by specifying 'nameability' and definability' to satisfy the requirement of being calculated outputs of a suitably defined UTM.

What are to make of this situation? Perhaps there is no need to 'prove' that $\sum(n)$ is uncomputable; it may be sufficient to demonstrate that dangerous curses of dimensionality may lie wrapped in the enigma that is the Busy Beaver. It may well be that we should resort to a Linnean philosophy of investigation: careful study of small, selected, well structured examples and diligent classification. This is not unlike the practice of the dynamical system theorist who wishes to tame the vagaries of nonlinear systems.

I shall, however, suggest another alternative in the next, concluding, section, in the face of this humbling of formalism and its methods, inflicted by the Busy Beaver. This alternative suggestion, entirely based on an extremely interesting 'attack' on the vagaries of $\sum(n)$ by Greenleaf (op.cit), when supplemented with Chaitin's careful circumvention of the *Berry Paradox*, results in a partial humbling of the Busy Beaver!

5. Humbled by the Busy Beaver - Humbling the Busy Beaver

"[S]uppose a vastly superior alien force lands and announces that they will destroy the planet unless we provide a value for the S function[31], along with a proof of its correctness. If they ask for $S(5)$ we should put all of our mathematicians, computer scientists, and computers to the task, but if they ask for $S(6)$ we should immediately attack because the task is hopeless."

Rona Machlin and Quentin F.Stout (1990), p.98.

I doubt I shall ever qualify for membership in the Busy Beaver club. However I am a life member of all sorts of Gandhi Clubs. In this capacity I would rather

[31] $S(k) = max\ \{s(M) : M \in H(k)\}$, $H(k)$: the set of all k-state Turing Machines which eventually halt when started on a blank tape; $s(M)$: number of steps performed by Turing Machine M before halting.

find a more peaceful solution than 'attack', no doubt without any hope of success, 'vastly superior alien forces'. From the previous section, we know that a strategy mimicking that which was successfully achieved with the Ackermann function and primitive recursion is unachievable with the problems posed by $\sum(n)$ for TMs and partial recursive functions. No new operations have been on offer; nor have there been suggestions on any kind of imaginative expansion of the basic functions. Thus, the beaten track does not offer much hope for a pacifist.

On the other hand, the strategy carried out by Russell and others (Poincaré, Weyl etc.) – to banish impredicative definitions from mathematical discourse - may smack of a mildly defeatist attitude. Paradoxes play the kind of role counter-examples play in sharpening an understanding of the scope of mathematical theorems. A world of mathematical semantics without paradoxes may well be poorer even from an epistemological viewpoint.

There is a more elegant way out of this dilemma. The idea for this alternative way was motivated by trying to define the *value* of $\sum(n)$, for any given n, in such a way that it will dampen its growth rate. To put it in a more prosaically, I want to drug the Busy Beaver into slowing down its eagerness! This is my interpretation of the elegant approach suggested by Greenleaf (op.cit), which is based on a philosophy of mathematics that is particularly significant in any attempt to tame the behaviour of $\sum(n)$, without losing the rich insights into emergent computations that such behaviour gives rise to. Let me summarize, first, the mathematical philosophy that underpins Greeenleaf's concrete suggestions to manage a meaningful study of $\sum(n)$ – and to deflect the hostile threats of 'vastly superior alien forces'.

- The triple {**assumption, proof, conclusion**} can be understood in terms of {**input data, algorithm, output data**}.
- Mathematics is best regarded as a *very high level programming language*.
- In constructive, computable[32] and (constructive) nonstandard analysis, every proof is an algorithm.
- To understand a theorem (in *any* kind of mathematics) in algorithmic terms, represent the assumptions as *input data* and the conclusions as *output data*. Then try to convert the proof into an *algorithm* which will take in the input and produce the desired output. If you are unable to do this, it is probably because the proof relies essentially on the *law of excluded middle*.
- If we take algorithms and data structures to be fundamental, then it is natural to define and understand *functions* in these terms. The phrase "non-computable function" then becomes problematic, and the understanding which sees *almost all* functions as non-computable becomes mysterious. If a function does not correspond to an algorithm, what can it be? There is no higher court corresponding to the set theory of logical mathematics.

[32]In the case of affirmative, existence, proofs. Universal negative propositions use the full paraphernalia of classical logic, including the law of the excluded middle.

- We shall take the stand that functions are, by definition, computable, and then test those phenomena which are standardly taken as evidence for the existence of non-computable functions, to see if we need to yield any ground.
- Given a putative function f – say Rado's $\sum(n)$ - we do not ask "Is it computable?" but rather "What are the *data types* of the domain and of the range?" This question will often have more than one natural answer, and we will need to consider both restricted and expanded domain/range pairs. Distinguishing between these pairs will require that we reject excluded middle for undecidable propositions. *If you attempt to pair an expanded domain for f with a restricted range, you will come to the conclusion that f is non-computable.*

To use this alternative vision of mathematics and mathematical activity towards a reinterpretation and taming of the activities of the Busy Beaver[33] , it will be helpful to begin by considering the rigorous definition of a real number, either via a Dedekind cut or via a Cauchy sequence of rationals[34] . In the former case the set $\mathbb{R}$ of real numbers is defined as the collection of all Dedekind cuts and the elements of $\mathbb{R}$ are then defined as certain subsets of $\mathbb{Q}$. In the latter case, real numbers are equivalence classes of Cauchy sequences of rational numbers. There is, of course, more algorithmic content in the definition of $\mathbb{R}$ as equivalence classes of Cauchy sequences of rational numbers, but the point is that in both definitions a real number is characterized in terms of a collection of rational numbers.

A real number can only be determined up to a pre-assigned degree of approximation. Some real numbers are hard to describe, i.e., compute; they are algorithmically highly complex. It takes time to determine them even to low levels of approximation. If, taking a cue from this setup, we define $\sum(n)$ in some equivalent way, we would kill the proverbial two birds with one stone: on the one hand, slow down the growth rate of $\sum(n)$; on the other, make it analogous to Chaitin's Ω.

Most integers are random and, hence, 'complex'; *a fortiori*, most real numbers are strongly random and highly 'complex'. But how do we make these assertions and notions precise? How does one prove that any given, individual integer, for example $\sum(n)$, is 'random' or 'complex' and, if 'complex', its degree of complexity? By, first, freeing the characterization of *randomness* from its classical underpinnings in probability theory and regrounding it in information theory. This makes it possible to talk meaningfully and quantitatively about the randomness of individual (combinatorial) objects in terms of their information content. Next, to define information *algorithmically* in any one of three equivalent ways: *Kolmogorov complexity, Solomonoff's Universal (Inductive) Probabilities* and *Chaitin's Program Size Complexity.* Finally,

[33]What is the Busy Beaver if not a mathematician? The question is what *kind* of mathematician – constructive, classical, non-standard, computable?

[34]I shall not consider the other two methods: via *nested intervals* or by means of *decimal expansion to a base*, say b, where b is an integer > 0. Goodstein's *uniform calculus*, for example, proceeds by way of the latter method.

to define the notion of *incompressibility* in terms of *minimum programs of a universal computer*. Putting all this together we get:

DEFINITION 13. *Kolmogorov-Chaitin Complexity*
The Kolmogorov-Chaitin complexity, $K_U(x)$ of a (binary) string x w.r.t a UTM, U, is:

$$K_U(x) = \min_{p:U(p)=x} l(p) \tag{5.1}$$

which denotes the minimum length over all programs that print x and halt; i.e., $K_U(x)$ is the shortest description length of x, taken over all possible algorithmic descriptions by the Universal Computer U.

It is immediate from even a cursory reflection over the statements of theorems 5 and 4 of the previous section that it is easy to prove that $K_U(x)$ is uncomputable. A simple extrapolation would also convince the reader that it is possible to define a measure of complexity, entirely analogous to $K_U(x)$, in terms of $\sum(n)$ – but with one caveat: it should also be relative to a pre-specified UTM, say U_{BB}. The idea would be to determine the binary code of any given (combinatorial) object and define its *Busy Beaver complexity* as the minimum k-card TM that would print the number of 1s in that code (perhaps separated by ordered blancs). In this way, we might be able to free the definition of complexity even from its underpinnings on any concept of information; instead defining it dynamically in a natural setting. In other words, every combinatorial object (i.e., algorithmic object) will be the output of some Busy Beaver. The complexity of the given object is the algorithmic description of the minimum Busy Beaver that would print its code and halt. Obviously, this complexity, too, would be uncomputable.

Rissanen's *stochastic complexity* is one approximation scheme to tackle the problem of the uncomputability of $K_U(x)$. It should not be difficult to adapt that method to approximate the Busy Beaver complexity of any object. But this remains an unreflected conjecture.

It was Chaitin's great achievement to have constructed the irrational number Ω and prove that it was random. In the process, he became one of the three pioneers of *algorithmic complexity theory.* Chaitin has himself discussed the analogies between his Ω and Rado's $\sum(n)$ and there is no need to expand on that theme in a short, concluding, section (although I do discuss the analogies more formally and much more extensively in Velupillai, 2003b).

Now to return to Greenleaf's philosophy of mathematics and its use in taming the activities of the Busy Beaver, we exploit the analogies inherent in another aspect of the real numbers: their definitions as *sets of rational numbers* plus *an approximation scheme and degree* (for example in the Cauchy characterization of $\mathbb{R}$). The first question in this endeavour, given the above summary of Greenleaf's mathematical philosophy, is what is the *appropriate data type* for studying $\sum(n)$? It is in answering this question that the analogy of defining the members of $\mathbb{R}$ as subsets of $\mathbb{Q}$ will be exploited. It may be best to state Greenleaf's 'solution' before even describing its structure:

> "The busy beaver function *bb* $[\equiv \sum(n)]$ *becomes computable* when its domain and range are properly defined. When the domain is taken to be $\mathbb{N}$, the range will be the set of 'weak integers', *a superset of* $\mathbb{N}$ ..."
>
> ibid, p.226; italics added.

The 'weak integers', in essence, 'weaken' the over-enthusiasm of the Busy Beavers. 'Weak integers' are constructed and defined relative to a given UTM in the following way:

Step 1:

For any given k, implement the given UTM to enumerate all k-card TMs.

Step 2:

Next, execute each k-card TM from the enumerated list on a standard two-way infinite blank tape for some given, arbitrarily long, time interval.

Step 3:

Whenever a Busy Beaver is found, its productivity is listed on an auxiliary tape.

Step 4:

$\sum(k) : \mathbb{N} \rightarrow \{\mathbb{N} \times \mathbb{N}; UTM\}$

where the range is given by a pair constructed as follows: to each element in the enumerated list of k-state TMs, associate the result, an integer, which is possibly the determined productivity associated with it or a 'temporary' value obtained from an *Oracle* computation, by the given UTM[35].

Thus the set of 'weak integers', say Ξ, are approximations from below in the same sense in which semi-computable functions are defined to approximate from above and below and the analogy goes even further: naturally, Ξ, too, will be a recursively enumerable set that is not recursive. The standard integers will be the singleton sets and, therefore, $\mathbb{N} \subset \Xi$. Natural order relations can be defined over Ξ, but I leave the interested reader to consult Greenleaf (op.cit) for such details.

I return, finally, to the question posed in pt. 8 of §3 on whether non-recurring 'never-stoppers' encapsulate within themselves a code for self-reproduction. After all, both von Neumann (1966) and Turing (1952) initiated the generic field that has spawned, in one version, ABCM activities, on the basis of this fundamental question. Of course, both gave affirmative answers to the question and the pioneering nature of their classic works resides in the way they demonstrated, constructively, the implementation of this programme of research. Encapsulating a code for self-reproduction means constructing the initial conditions in such a way that the dynamics will spawn a 'universal constructor' – i.e., an appropriate UTM. I cannot see any other disciplining mechanism for the simulation activities of ABCM practitioners. On the other hand, I

[35] At this last stage of the last step I part company with Greenleaf, mainly because I wish to retain the analogy with Chaitin's Ω.

have never seen anything like this question being posed by those involved in building and simulating agent-based computational models.

There is, however, a paradox here. von Nuemann's work remained within the digital domain[36] ; Turing's morphogenesis model lent itself to a natural dynamical systems interpretation. Turing's model of computation has been criticised for neglecting the role played by the real numbers in the natural sciences (cf. Blum et.al, et. al, 1997). von Neumann, in his last celebrated work, the *Silliman Lectures* (von Neumann, 1958) felt that the digital domain was inadequate to encapsulate the full reasoning powers of the human brain (not quite the 'mind'!). Resolving this dichotomy should be part of the ABCM research agenda. If not, there will always remain an *ad hockery* between the digital nature of the formalism of programming languages and the dynamical system that is the iteration in a variety of topological spaces. Turing was, of course, aware of these issues even in his original classic (Turing, 1936-7).

Let me conclude this tangled path through the weird and wonderful world of the Busy Beaver and its activities by a final comment on the lessons such a journey may have for the practitioners of the noble art of simulations. Without an articulated mathematical philosophy that is meaningfully tied to the epistemology underpinning any simulation exercise, it is easy to be humbled by the power of simple machines to generate illusions of possible complex worlds. Taming complexity is often also an exercise in reigning illusions. Only disciplined investigations, foot soldiering and soiled hands can defeat powerful alien forces, illusions and complexities.

[36]However, in his famous 'growth model' (von Neumann, 1945-6, but first presented in 1932) the question of self-reproduction was posed and answered topologically. Of course, Kleene's *recursion theorem* and the *recursion theoretic fix-point theorems* were still events of the future. All that von Neumann had at his disposal, in the form of fix-point theorems, were the classical ones, in particular Brouwer's version. The current formalization of von Neumann's model of self-reproduction proceeds via the recursion theoretic fix-point theorem and the recursion theorem.

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CHAPTER 4

Computable Rational Expectations Equilibria

1. Preamble

> "If a macro-system as a whole has coherence, perhaps it would be useful to study directly the reasons that determine its coherence. This probably is the course underlined by Keynes when he stressed his intention of studying 'the system as a whole'. If a macroeconomic logic partially independent of that which determines individual behaviour exists - an underemployment equilibrium is surely an equilibrium relative to the system and not to the individuals composing it - perhaps that logic deserves to be analysed in itself. .... My conviction is that macroeconomics has its own dimension which must be considered and not just alluded to."
>
> [**8**], pp.27-8.

The two fundamental principles that underpin the study of a macroeconomic 'system as a whole' are, firstly, the 'fallacy of composition' and, secondly, the idea known variously as the 'paradox of thrift', 'paradox of saving' or, more dramatically, as the 'Banana parable' (cf. [**16**], pp.176-8). The ubiquitous 'representative agent' has dispensed with these homely wisdoms of a macroeconomic logic. As a result the momentous macroeconomic issues of growth, fluctuations, unemployment and policy are disciplined by the logic of microeconomic behavioural determinants. It could so easily have been otherwise had we, for example, paid more serious attention to one of the great masters of our subject who straddled the micro-macro divide, John Hicks, when, in his summarising statements of the 'Final Discussion' after the IEA Conference on '*The Micoreconomic Foundations of Macroeconomics*', pointed out:

> "We had been supposed to be discussing the microeconomic foundations of macroeconomics, but we had come to realise that there were several kinds of macroeconomics, each probably requiring its own foundations, and though they overlapped they were not wholly the same. One had to distinguish at the least between macroeconometrics and 'macro-political-economy'. ....[W]e had been much more concerned with macro-political-economy'.

> There was a close relation between macro-political-economy and social accounting, so .... it might be useful to arrange our problems in relation to the social accounting framework in order to see how they fitted together"
>
> [**11**],p.373

One of the great merits of Jean-Paul Fitoussi's work as a macroeconomic theoretician, and as a passionate advocate for an active role for policy, has been his ever vigilant attention to the above 'two fundamental principles of macroeconomics' underpinned by their relation 'to the social accounting framework'. Macro-theoretical propositions derived solely on the basis of microeconomic theories, particularly if they are not constrained by the 'two fundamental propositions of macroeconomics', have always left him with a sense of unease. Thus, policy ineffectiveness propositions, based as they are on strong rational expectations hypotheses, time inconsistency results and equilibrium interpretations of fluctuations and unemployment are examples where Fitoussi's critical antennae have been seriously disturbed over the past two decades.

For years I have, myself, been struck by a strange anomaly. Many of the fundamental concepts that lie at the basis of newclassical macroeconomics - policy ineffectiveness, credibility, time inconsistency, rational expectations, the advantages of (transparent) rules over (enlightened) discretion, etc., - were also those that informed the work of the 'old' Stockholm School economics - particularly the work of Erik Lindahl and Gunnar Myrdal from the early 20s through the late 30s. They, in particular Lindahl, also worked these themes and concepts into dynamic equilibrium schemes. I cannot find a better, clearer, statement of the dynamic economic environment, in which what eventually came to be known as the rational expectations hypothesis, than Lindahl's discussion of the idea in a presentation of his vision of the Keynesian system (but it was only a rewording of a basic idea that had been almost a touchstone of his work on monetary policy and capital theory during the 20s and early 30s):

> "It also seems reasonable to postulate an interdependence between the variables entering an economic system in the case concerning the determination of the conditions for *correctly anticipated processes*. **These conditions are that the individuals have such expectations of the future that they act in ways which are necessary for their expectations to be fulfilled**. It follows that the interdependence between present and future magnitudes is conditioned in this case by the fact that the latter, via correct anticipations, influence the former. If we also choose to describe such developments as **equilibrium processes**, this implies that we widen the concept of equilibrium to include also economic systems describing changes over time where the changes that take place from period to period do not cause any interruption in, but, on the contrary, are an expression of the continual adjustment of the variables to each other."

[**13**], p.27; bold emphasis added.

However, their fundamental political sympathies were very similar to those espoused by Fitoussi and they made their framework - accounting systems *par excellence* - substantiate an active role for policy. This made me wonder whether there was something special about the language[1] within which the newclassicals developed their concepts and made them work had a role to play in the scope of the conclusions they reached.

Thus, in recent years, I have tried to resolve the anomaly mentioned above by framing aspects of newclassical macroeconomics with the formalism of an alternative mathematics, of recursion theory, and asking pertinent algorithmic and dynamic questions. Essentially, I have replaced the use of the standard topological fixed-point theorems that have been used to encapsulate and formalise *self-reference* (rational expectations and policy ineffectiveness), *infinite regress* (rational expectations) and *self-reproduction* and *self-reconstruction* (growth), in economic contexts, with two fundamental theorems of classical recursion theory[2]. The idea of self-referential behaviour is, for example, formalized by considering the action of a program or an algorithm on its own description.

A theoretical framework must mesh smoothly with - be consistent with - the empirical data generating process that could underpin it from methodological and epistemological points of view. I do not use these loaded words with grand aims in mind; I refer to the simple fact that a process that generates the macroeconomic data that is the basis on which the processes of scientific validations of any sort can be performed must do so in a way that is consistent with the way the theoretical model postulates the use of the data. I refer to this as a 'simple fact' in the elementary and intuitive sense that data that must be used by rational agents will have to respect their cognitive structures and the structures of the processing and measuring instruments with which they - and the macroeconomic system as a whole - will analyse and theorise with them. There is no point in postulating data generating mechanisms that are incompatible with the cognitive and processing and measuring structures of the analysing agents of the economy - at the individual and collective levels. In one of my own collaborative writings with Fitoussi, we have touched upon themes of this sort ([**9**], esp. pp. 225-32).

In this essay I try to formalise the idea of *Rational Expectations Equilibria*, *REE*, recursion theoretically, eschewing all topological assumptions. The title has the qualifying word 'tutorial' to emphasise the fact that I want to try to suggest a modelling strategy that can be mimicked for other concepts and areas of macroeconomics: *policy ineffectiveness, time inconsistency, growth, fluctuations* and other dynamic issues in

[1] Keeping in mind Samuelson's Gibbsian admonition that *Mathematics is a language (*[**17**], epigraph on the title page).

[2] One of which is also called a *fix point theorem.*

macroeconomics. All recursion theoretic formalizations and results come, almost invariably, ‘open ended’ - meaning, even when uniqueness results are demonstrated there will be, embedded in the recesses of the procedures generating equilibria and other types of solutions, an indeterminacy. This is due to a generic result in computability theory called the *Halting Problem for Turing Machines*. It is a kind of generic *undecidability* result, a counterpart to the more formal, and more famous, Gödelian *undecidability* results. It is this fact, lurking as a backdrop to all the theorems in this essay, that makes it possible to claim that *Computable Macroeconomics* is not as determinate as Newclassical Macroeconomics. This is also the reason why the Swedes, again Lindahl and Myrdal in particular, were able to work with concepts that were, ostensibly, similar to those being used by the Newclassicals, but were actively engaged in proposing and devising enlightened, discretionary, policies at the macroeconomic level. To be categorical about policy - positively or negatively - on the basis of mathematical models is a dangerous sport.

The essay is organised as follows. In the next section I outline the origins of the rational expectations problem as a (topological) fixed-point problem. Next, in the third section, I suggest its reformulation in recursion theoretic terms. This reformulation makes it possible to re-interpret a rational expectations equilibrium as a recursion theoretic fixed-point problem in such a way that it is intrinsically computable *ab initio.* Thus, there is no separation between a first step in which the existence of a rational expectations equilibrium is ‘proved’ and, then, an *ad hoc* mechanism devised to determine it - via uncomputable, equally *ad hoc* learning processes. Moreover, every recursion theoretic assumption, and their consequent formalisms I have employed or invoked, in this essay, is consistent with the known results and constraints on human cognitive structures and all known computing devices, artificial or natural, ideal or less-than-ideal.

In the fourth section, respecting existing tradition, I accept any given *REE* solution from some, prior, economic model or analysis - in the particular case considered it is a standard OLG generated *REE* solution - and devise a recursion theoretic learning mechanism to determine it.

In the concluding section I try to fashion a fabric, or at least its design, from the sketches of the threads outlined earlier, that depicts a possible research program on Computable Macroeconomics as an alternative to the Newclassical Recursive Macroeconomics.

2. Topological Rational Expectations

> "We can now clearly see the unavoidable dilemma we are facing if we want to apply the Brouwer theorem in the present situation: *if we restrict ourselves to a discrete variable, i.e. consider the reaction function f merely as a mapping of P into P* [the discrete (finite) set

of percentages] *we are not entitled to use the Brouwer theorem because of the non-convexity of P. Besides, continuity represents a vacuous condition in this case. On the other hand, if we use a continuous variable we can use the Brouwer theorem, but the fixed point is then generally located outside of P and hence meaningless in the empirical situation at hand."*

[**2**], p.330; italics in the original.

In a critical discussion of the use of the Brouwer fixed point theorem by Herbert Simon, [**19**], that presaged its decisive use in what became the definition of a rational expectations equilibrium, Karl Egil Aubert, a respected mathematician, suggested that economists - and political scientists - were rather cavalier about the domain of definition of economic variables and, hence, less than careful about the mathematics they invoked to derive economic propositions. I was left with the impression, after a careful reading of the discussion between Aubert and Simon ([**2**], [**20**], [**3**] and [**21**]), that the issue was not the use of a fixed point framework but its *nature, scope* and *underpinnings.* However, particularly in a rational expectations context, it is not only a question of the nature of the domain of definition but also the fact that there are self-referential and infinite-regress elements intrinsic to the problem. This makes the choice of the fixed point theorem within which to embed the question of a rational expectations equilibrium particularly sensitive to the kind of mathematics and logic that underpins it. In this section I trace the origins of the 'topologisation' of the mathematical problem of rational expectations equilibrium and discuss the possible infelicities inherent in such a formalisation.

There are two crucial aspects to the notion of rational expectations equilibrium - henceforth, *REE* - ([**18**], pp.6-10): an individual optimization problem, subject to *perceived constraints,* and a system wide, autonomous, set of constraints imposing a consistency across the collection of the perceived constraints of the individuals. The latter would be, in a most general sense, the accounting constraint, generated autonomously, by the logic of the macroeconomic system. In a representative agent framework the determination of *REE*s entails the solution of a general fix point problem. Suppose the representative agent's perceived law of motion of the macroeconomic system (as a function of state variables and exogenous 'disturbances') as a whole is given by H[3]. The system wide autonomous set of constraints, implied, partially at least, by the optimal decisions based on perceived constraints by the agents, on the other hand, imply an actual law of motion given by, say, H^*. The search for fixed-points of a mapping, T, linking the individually perceived macroeconomic law of motion and the actual law of motion:

$$H^* = T(H) \tag{2.1}$$

[3]Readers familiar with the literature will recognise that the notation H reflects the fact that, in the underlying optimisation problem, a *Hamiltonian* function has to be formed.

as the fixed-points of H of T[4]:

$$H = T(H) \tag{2.2}$$

determines REEs.

What is the justification for T? What kind of 'animal' is it? It is variously referred to as a 'reaction function', a 'best response function', a 'best response mapping', etc. But whatever it is called, eventually the necessary mathematical assumptions are imputed to it such that it is amenable to a topological interpretation whereby appeal can be made to the existence of a fix point for it as a mapping from a structured domain into itself. So far as I know, there is no optimising economic theoretical justification for it.

There is also a methodological asymmetry in the determination of H and H^*, respectively. The former has a self-referential aspect to it; the latter an infinite regress element in it. Transforming, mechanically, (1) into (2) hides this fact and reducing it to a topological fixed-point problem does little methodological justice to the contents of the constituent elements of the problem. These elements are brought to the surface at the second, separate, step in which ostensible learning mechanisms are devised, in *ad hoc* ways, to determine, explicitly the uncomputable and non-constructive fixed-points. But is it really impossible to consider the twin problems in one fell swoop, so to speak?

This kind of tradition to the formalization and determination of REEs has almost by default forced the problem into a particular mathematical straitjacket. The mapping is given topological underpinnings, automatically endowing the underlying assumptions with real analytic content[5]. As a consequence of these default ideas the problem of determining any REE is dichotomized into two sub-problems: a first part where non-constructive and non-computable proofs of the existence of REEs are provided; and a subsequent, quite separate, second part where mechanisms - often given the sobriquet 'learning mechanisms' - are devised to show that such REEs can be determined by individual optimising agents[6]. It is in this second part where orthodox theory endows agents with an *ad hoc* varieties of 'bounded rationality' postulates, without modifying the full rationality postulates of the underlying, original, individual optimization problem.

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[4]In a space of functions.

[5]In the strict technical sense of the mathematics of *real analysis* as distinct from, say, *constructive, computable or non-standard analysis.*

[6]A perceptive (*sic*!) reader may wonder whether there should not also be an optimization exercise over the set of feasible or perceived learning mechanisms? Carried to its logical conclusion, this would entail the determination of a set of REEs over the collection of learning mechanisms, *ad infinitum* (or *ad nauseum*, whichever one prefers).

Now, how did this *topological fixed-point REE tradition* come into being? Not, as might conceivably be believed, as a result of Muth's justly celebrated original contribution,[**16**], but from the prior work of Herbert Simon on a problem of predicting the behaviour of rational agents in a political setting, [**19**] and an almost simultaneous economic application by Franco Modigliani and Emile Grunberg, [**10**]. Let me explain, albeit briefly, and to the extent necessary in the context of this essay.[7]

Simon, in considering the general issue of the feasibility of public prediction in a social science context, formalised the problem for the particular case of investigating how 'the publication of an election prediction (particularly one based on poll data) might influence [individual] voting behaviour, and, hence - ... - falsify the prediction'. Simon, as he has done so often in so many problem situations, came up with the innovative suggestion that the self-referential and infinite-regress content of such a context may well be solved by framing it as a mathematical fixed point problem:

> "Is there not involved here a vicious circle, whereby any attempt to anticipate the reactions of the voters alters those reactions and hence invalidates the prediction?
>
> *In principle*, the last question can be answered in the negative: there is no vicious circle.
>
> ....
>
> We [can prove using a 'classical theorem of topology due to Brouwer (the 'fixed-point' theorem)] that it is always possible in principle to take account of reactions to a published prediction in such a way that the prediction will be confirmed by the event."
>
> Simon, op.cit, [**19**], pp. 82-4; italics added.

The 'vicious circle' refers to the self-referential and infinite-regress nature of any such problem where a (rational) agent is placed in a social situation and the individual's behaviour determines, and is determined by, the mutual interdependencies inherent in them. Almost simultaneously with Simon broaching the above problem,

[7] My aim is to show that the framing the *REE* problem as a *topological fixed-point problem* was not necessary. Moreover, by forcing the *REE* problem as a topological fixed-point problem it was necessary to dichotomize into the proof of existence part and a separate part to demonstrate the feasibility of constructing mechanisms to determine them. This is mainly - but not only - due to the utilization of non-constructive topological fixed-point theorems in the first, 'proof of *REE* existence', part. In this sense the *REE learning* research program is very similar to the earlier dichotomizing of the *general equilibrium* problem. In that earlier phase, a long tradition of using topological fixed-point theorem to prove the existence of a economic equilibria was separated from devising *constructive* or *computable* mechanisms to determine them. The later phase resulted in the highly successful *Computable General Equilibrium (CGE)* models. It remains a melancholy fact, however, that even after over forty years of sustained and impressive work on CGE models, they are neither constructive nor computable, contrary to assertions by proponents of the theory.

Grunberg and Modigliani took up a similar issue within the more specified context of individually rational behaviour in a market economy[8]:

> "The fact that human beings react to the expectations of future events seems to create difficulties for the social sciences unknown to the physical sciences: it has been claimed that, *in reacting to the published prediction of a future event, individuals influence the course of events and therefore falsify the prediction.* The purpose of this paper is to verify the validity of this claim."
>
> [**10**], p.465; italics added.

Grunberg and Modigliani recognised, clearly and explicitly, both the self-referential nature of the problem of consistent individually rational predictions in the face of being placed in an economic environment where their predictions are reactions to, and react upon, the aggregate outcome, but also were acutely aware of the technical difficulties of infinite regression that was also inherent in such situations (cf., in particular, [**10**], p. 467 and p. 471). In their setting an individual producer faced the classic problem of expected price and quantity formation in a single market, subject to public prediction of the market clearing price. It was not dissimilar to the crude cobweb model, as was indeed recognised by them ([**10**], p.468, footnote 13). Interestingly, what eventually came to be called *rational expectations* by Muth was called a *warranted expectation*[9] by Grunberg and Modigliani (ibid, pp. 469-70). In any event, their claim that it was '*normally* possible' to prove the *existence* of 'at least one correct public prediction in the face of effective reaction by the agents' was substantiated by invoking Brouwer's Fixed Point Theorem (ibid, p. 472). To facilitate the application of the theorem,

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[8]In their first footnote, Modigliani and Grunberg pay handsome acknowledgement to Herbert Simon for, in particular, suggesting 'the use of Brouwer's Fixed Point Theorem'. ([**10**], p.465, footnote,1).

Simon himself later, during the 'debate' with Aubert, on the appropriateness of the use of the Brouwer Fixed Point Theorem in economic contexts, recalled:

> "More recently, the question of the self-consistency of predictions has arisen again in connection with the so-called rational expectations theories of economic behavior under uncertainty. .... John Muth's important 1961 paper, which introduced the rational expectations theory, acknowledged the Grunberg-Modigliani paper as a direct ancestor.
>
> ..... It was the purpose of my paper, and that of Grunberg and Modigliani, to demonstrate that it was always *in principle* possible to anticipate the reaction in the forecast, however difficult it may be to make the correct forecast. " ([**19**], p.608; italics in original)

[9]I am reminded that Phelps, in one of his early, influential, papers that introduced the concept of the natural rate of unemployment in its modern forms, first referred to it as a *warranted rate*. Eventually, of course, the Wicksellian term *natural rate*, introduced by Friedman, prevailed. Phelps and Grunberg-Modigliani were, presumably, influenced by Harrodian thoughts in choosing the eminently suitable word 'warranted' rather than 'natural' or 'rational', respectively. Personally, for aesthetic as well as reasons of economic content, I wish the Phelps and Grunberg-Modigliani suggestions had prevailed.

the constituent functions[10] and variables - in particular, the *reaction function* and the conditions on the domain of definition of prices - were assumed to satisfy the necessary real number and topological conditions (continuity, boundedness, etc).

Thus it was that the tradition, in the rational expectations literature of 'solving' the conundrums of self-reference and infinite-regress via *topological fixed-point theorems* was etched in the collective memory of the profession. And so, four decades after the Simon and the Grunberg-Modigliani contributions, Sargent, in his influential *Arne Ryde Lectures* ([**18**]) was able to refer to the fixed-point approach to rational expectations, referring to equation (2), above, without blinking the proverbial eyelid:

> "A rational expectations equilibrium is a fixed point of the mapping T."
>
> [**18**], p.10.

Now, fifty years after that initial introduction of the topological fixed-point tradition by Simon and Grunberg-Modigliani, economists automatically and uncritically accept that this is the only way to solve the *REE* existence problem - and they are not to be blamed. After all, the same somnambulent complacency dominates the fundamentals of general equilibrium theory, as if the equilibrium existence problem can only be framed as a fixed-point solution. Because of this somnambulent complacency, the existence problem has forever been severed of all connections with the problem of determining - or finding or constructing or locating - the processes that may lead to the non-constructive and uncomputable equilibrium. The recursion theoretic fixed-point tradition not only preserves the unity of equilibrium existence demonstration with the processes that determine it; but it also retains, in the forefront, the self-referential and infinite-regress aspects of the problem of the interaction between individual and social prediction and individual and general equilibrium.

3. Recursion Theoretic Rational Expectations

> "Suppose that we want to give an English sentence that commands the reader to print a copy of the same sentence. One way to do so is to say:
>
> Print out this sentence

[10] The relation between a market price and its predicted value was termed the *reaction function*: "Relations of this form between the variable to be predicted and the prediction will be called *reaction functions*." ([**10**], p.471; italics in original).

As became the tradition in the whole rational expectations literature, the functional form for the reaction functions were chosen with a clear eye on the requirements for the application of an appropriate topological fixed point theorem. The self-reference and infinite-regress underpinnings were thought to have been adequately subsumed in the existence results that were guaranteed by the fixed point solution. That the twin conundrums were not subsumed but simply camouflaged was not to become evident till all the later activity on trying to devise learning processes for identifying *REE*s.

This sentence has the desired meaning because it directs the reader to print a copy of the sentence itself. However, it doesn't have an obvious translation into a programming language because the **self-referential** word 'this' in the sentence has no counterpart.
...
..
The recursion theorem provides the ability to implement the **self-referential** *this* into any programming language."
[**22**], p.200; italics in original.[11]; bold italics added.

There is nothing sacrosanct about a topological interpretation of the operator T, the reaction or response function. It could equally well be interpreted recursion theoretically, which is what I shall do in the sequel.[12]. I need some unfamiliar, but elementary, formal machinery, not normally available to the mathematical economist or the macroeconomist.

DEFINITION 14. *An **operator** is a function:*

$$\Phi : F_m \longrightarrow F_n \tag{3.1}$$

where $F_k \quad (k \geqq 1)$ is the class of all partial (recursive) functions from $\mathbb{N}^k$ to $\mathbb{N}$.

DEFINITION 15. *Φ is a **recursive operator** if there is a computable function ϕ such that $\forall f \in F_m$ and $\boldsymbol{x} \in \mathbb{N}^k$, $y \in \mathbb{N}$:*

$$\Phi(f)(\mathbf{x}) \simeq y \; iff \; \exists \; a \; finite \; \theta \sqsubseteq f \; such \; that \; \phi\left(\widetilde{\theta}, \mathbf{x}\right) \simeq y$$

where[13] $\widetilde{\theta}$ is a standard *coding* of a *finite* function θ, which is extended by f.

DEFINITION 16. *An operator $\Phi : F_m \longrightarrow F_n$ is **continuous** if, for any $f \in F_m$, and $\forall \mathbf{x}, y$:*

$$\Phi(f)(\mathbf{x}) \simeq y \; iff \; \exists \; a \; finite \; \theta \sqsubseteq f \quad such \; that \; \Phi(\theta)(\mathbf{x}) \simeq y$$

[11]Sipser's reference is to what is called the '*Second Recursion Theorem*'. I shall be working with and appealing to the '*First Recursion Theorem*'. But, of course, they are related. I want to work, explicitly, with a space of functions as the domain or relevance, i.e., with functionals, because the economic setting is dynamic. In the static economic case, it would have been sufficient to work with the '*Second Recursion Theorem*'.

[12]I have relied on the following four excellent texts for the formalisms and results of recursion theory that I am using in this part of the essay: [**6**], [**7**], [**15**] and [**24**].

[13]If $f(\mathbf{x})$ and $g(\mathbf{x})$ are expressions involving the variables $\mathbf{x} = (x_1, x_2, ..., x_k)$, then:

$$f(\mathbf{x}) \simeq g(\mathbf{x})$$

means: for any $\mathbf{x}$, $f(\mathbf{x})$ and $g(\mathbf{x})$ are either both defined or undefined, and if defined, they are equal.

Definition 17. *An operator* $\Phi : F_m \longrightarrow F_n$ *is* ***monotone*** *if, whenever* $f, g \in F_m$ *and* $f \sqsubseteq g$, *then* $\Phi(f) \sqsubseteq \Phi(g)$.

Theorem 6. *A recursive operator is continuous and monotone.*

Example 2. *Consider the following* ***recursive program,*** *Þ ,(also a recursive operator) over the integers:*
Þ: $F(x,y) \Longleftarrow if\ x = y\ then\ y+1,\ else\ F(x, F(x-1, y+1))$
Now replace each occurrence of F *in Þ by each of the following functions:*

$$f_1(x,y):\ if\ x = y\ then\ y+1,\ else\ x+1 \tag{3.2}$$

$$f_2(x,y):\ if\ x \geqq y\ then\ x+1,\ else\ y-1 \tag{3.3}$$

$$f_3(x,y):\ if\ \ (x \geqq y) \wedge (x-y\ even)\ \ then\ x+1,\ else\ undefined. \tag{3.4}$$

Then, on either side of $\Longleftarrow$ *in Þ, we get the* ***identical*** *partial functions:*

$$\forall i\,(1 \leqq i \leqq 3)\,,\ f_i(x,y) \equiv if\ x = y\ then\ y = 1,\ else\ f_i(x-1, y+1) \tag{3.5}$$

Such functions f_i $(\forall i\,(1 \leqq i \leqq 3))$ *are referred to as* ***fixed-points*** *of the recursive program Þ (recursive operator).*
Note that these are fixed-points of functionals.

Remark 2. *Note that* f_3, *in contrast to* f_1 *and* f_2, *has the following special property.* $\forall\langle x,y\rangle$ *of pairs of integers such that* $f_3(x,y)$ *is defined, both* f_1 *and* f_2 *are also defined and have the same value as does* f_3.

- f_3 *is, then, said to be* ***less defined than or equal to*** f_1 *and* f_2 *and this property is denoted by* $f_3 \sqsubseteq f_1$ *and* $f_3 \sqsubseteq f_2$.
- *In fact, in this particular example, it so happens that* f_3 ***is less defined than or equal to*** *all fixed points of Þ.*
- *In addition,* f_3 *is the* ***only*** *partial function with this property for Þ and is, therefore called the* ***least fixed point of Þ.***

We now have all the formal machinery needed to state one of the classic theorems of recursive function theory, known variously as the *first recursion theorem*, *Kleene's theorem* or, sometimes, as the *fixed point theorem for complete partial orders.*

Theorem 7. *Suppose that $\Phi : F_m \longrightarrow F_n$ is a recursive operator (or a recursive program Þ). Then there is a partial function f_ϕ that is the least fixed point of Φ :*
$\Phi(f_\phi) = f_\phi$;
If $\Phi(g) = g$, then $f_\phi \sqsubseteq g$.

Remark 3. *If, in addition to being partial, f_ϕ is also total, then it is the* ***unique least fixed point.*** *Note also that a recursive operator is characterised by being continuous and monotone. There would have been some advantages in stating this famous theorem highlighting the domain of definition, i.e., complete partial orders, but the formal machinery becomes slightly unwieldy.*

It is easy to verify that the domain over which the recursive operator and the partial functions are defined are weaker than the conventional domains over which the economist works. Similarly, the continuity and monotonicity of the recursive operator is naturally satisfied by the standard assumptions in economic theory for the reaction or response function, T. Hence, we can apply the *first recursion theorem* to equation (2), interpreting T as a recursive operator and not as a topological mapping. Then, from the theorem, we know that there is a partial function - i.e., a computable function - f_t that is the least fixed point of T. Stating all this pseudo-formally as a summarising theorem, we get:

Theorem 8. *Suppose that the reaction or response function, $T : H_m \longrightarrow H_n$ is a recursive operator (or a recursive program Γ). Then there is a computable function f_t that is a least fixed point of T :*
$T(f_t) = f_t$;
If $T(g) = g$, then $f_t \sqsubseteq g$

What are the advantages of recasting the problem of solving for the *REE recursion theoretically* rather than retaining the traditional topological formalizations?

An advantage at the superficial level, but nevertheless important, is the simple fact that, as even the name indicates, recursion encapsulates, explicitly, the idea of self-reference because functions are defined, naturally, in terms of themselves. Secondly, again at the superficial level, the existence of a least fix point is a solution to the infinite-regress problem. Thus the two 'birds' are encapsulated in one fell swoop - and, that too, with a computable function. There is, therefore no need to dichotomise the solution for *REE* into an *existence* part and a separate *process* or *computable* or *learning* part.

Think of the formal discourse of economic analysis as being conducted in a programming language; call it $\Im$. We know that we choose the underlying terminology for economic formalisms with particular meaning in mind for the elemental units: preferences, endowments, technology, information, expectation and so on; call the generic element of the set ς. When we form a compound economic proposition out of the ς

units, the meaning is natural and clear. We can, therefore, suppose that evaluating a compound expression in $\Im$ is immediate: given an expression in $\Im$, say $\lambda(\varsigma)$, the variables in λ, when given specific values α, are to be evaluated according to the *semantics* of $\Im$. To actually *evaluate* a compound expression, $\lambda(\varsigma)$, we write a *recursive program* in the language $\Im$, the language of economic theory. But that leaves a key question unanswered: what is the computable function that is implicitly defined by the recursive program? The first recursion theorem answers this question with the answer: the least fixed-point. In this case, therefore, there is a direct application of the first recursion theorem to the semantics of the language $\Im$. The artificial separation between the syntax of economic analysis, when formalized, and its natural semantics can, therefore, be bridged *effectively*.

If the language of economic theory is best regarded as a very high level programming language, $\Im$, to understand a *theorem* in economics, in recursion theoretic terms, represent the *assumptions* - i.e., *axioms* and the *variables* - as *input data* and the *conclusions* as *output data*. State the theorem as an expression in the language $\Im$.Then try to convert the proof into a program in the language $\Im$, which will take in the inputs and produce the desired output. If one is unable to do this, it is probably because the proof relies essentially on some infusion of non-constructive or uncomputable elements. This step will identify any inadvertent infusion of non-algorithmic reasoning, which will have to be resolved - sooner or later, if computations are to be performed on the variables as input data. The computations are not necessarily numerical; they can also be symbolic.

In other words, if we take algorithms and data structures to be fundamental,then it is natural to define and understand functions in these terms. If a function does not correspond to an algorithm, what can it be? The topological definition of a function is not algorithmic. Therefore, the expressions formed from the language of economic theory, in a topological formalisation, are not necessarily implementable by a program, except by fluke or by illegitimate and vague approximations. Hence the need to dichotomise every topological existence proof. In the case of *REE,* this is the root cause of the artificial importance granted to a separate problem of learning *REE*s. Nevertheless, the separation does exist and I shall approach a resolution of it in recursion theoretic terms in the next section.

4. Recursively Learning a REE

> "The development of computable analysis as an alternative to conventional mathematical analysis was essentially complete by 1975, although today this analysis is largely unknown.
>
> A perfectly natural reaction at this point is to ask 'Why bother? Calculus has been in use now for over three centuries, and what possible reason is there for altering its rules?'. The simplest answer to this question must be .... [that] we still do not solve our mathematical

problems precisely. [In computable analysis]...the key mathematical concepts - the real numbers, sequences, functions and so on - are defined in terms of some computation that an ideal computer could perform."

[**1**], pp. 2-3.

In the previous section I took as given by a previous economic analysis the arguments in the operator T. In this section[14] I go behind the scenes, so to speak, and take one of the many possible economic worlds on which T operates, a simple Overlapping Generation Model (OLG), with standard assumptions, which generates *REE*s as solutions to the following type of *functional* dynamic equation (cf. [**4**], pp. 414-6):

$$u'\left(e_1 - m_t\right) = \mathcal{E}\left\{\frac{m_{t+1}}{m_t}\frac{L_{t+1}}{L_t}v'(e_2 + m_{t+1}\frac{L_{t+1}}{L_t}) \mid \mathbf{I}_t\right\}, \forall \mathbf{I}_t \tag{4.1}$$

Where:
u and v are functional notations for the additive utility functions;
The real gross yield on money, $R_t = \frac{p_t x_{t+1}}{p_{t+1}} = \frac{m_{t+1}}{m_t}\frac{L_{t+1}}{L_t}$;
The real per capita currency balances, $m_t = \frac{M_t}{p_t L_t}$;
L_t : size of generation t (a discrete random variable with standard assumptions);
M_t : aggregate stock of currency;
p_t : realized price (of the one consumption good);
p_{t+1} : future price (random variable);
e_t : endowment at time t;
$\mathbf{I}_t$: information set defined by

$$\mathbf{I}_t = I\left\{\mathbf{I}_{t-1}, L_{t-1}, x_{t-1}, p_{t-1}, \theta_t\right\} \tag{4.2}$$

θ_t : vector of all other residual variables that the agent believes will influence future prices;

The problem I pose is the devising of an *effective* mechanism to *learn* and *identify* the above *REE* solution, without asking *how* the solution was arrived at - it could have been arrived at by *magic*, by pronouncements by the *Delphic Oracle, prayers, torture* or whatever. However, it is immediately clear that one must first ensure that the solution is itself a *recursive real*, if an *effective* mechanism is to locate it. *A priori*, and except for flukes, it is most likely that the standard solution will be a *non-recursive real.* To make it possible, therefore, to ensure a recursively real solution to the above functional dynamic equation, this OLG structure must be endowed with an appropriate recursion theoretic basis. I shall, now, indicate a possible set of minimum requirements for the required recursion theoretic basis.

[14]The model and results of this section are an abbreviation, with minor modifications, of what was presented in [**33**], pp. 94-100.

The derivative of the second period component of the additive utility function, v, must be a *computable real function.* Roughly speaking, if the domain of v is chosen judiciously and if $v \in C^2$, and computable, then v' is computable. But, for these to be acceptable assumptions, the arguments of v',i.e., e_2, m_{t+1}, and $\frac{L_{t+1}}{L_t}$, must be computable reals. Since this is straightforward for e_2 and per capita currency balances[15],m_{t+1}, a recursion theoretic interpretation for the random variable L_t will ensure that the assumptions underlying v' are recursion theoretically sound. Now, the random variables in the OLG model above are characterized by finite means and stationary probability distributions. It is, therefore, easy to construct a *Probabilistic Turing Machine*, *PTM*, endowed with an extra random-bit generator which outputs, whenever necessary, the necessary element that has the pre-assigned probability distribution. Next, there is the question of the recursivity of the information set, $\mathbf{I}_t$. Given that a recursion theoretic learning model requires this information set to be *recursively* presented to the agents, it is only the element θ_t that remains to be recursively defined. However, this is a purely exogenous variable that can be endowed with the required recursive structure almost arbitrarily.

Finally, the expectations operator is interpreted as an integration process and, since integration is a computable process, this completes the necessary endowment of the elements of the above OLG model with a sufficient recursive structure to make the *REE* generated by the solution to the functional equation a *recursive real.* The minor caveat 'sufficient recursive structure' is to guard against any misconception that this is the only way to endow the elements of an OLG model as given above with the required assumptions to guarantee the generation of a recursive real as a solution. There are many ways to do so but I have chosen this particular mode because it seems straightforward and simple. Above all, these assumptions do not contradict any of the standard assumptions and can live with almost all of them, with minor and inconsequential modifications.

With this machinery at hand, I can state and prove the following theorem:

THEOREM 9. *A unique, recursively real, solution to (8) can be identified as the REE and learned recursively.*

PROOF. See [**33**], pp. 98-9. □

REMARK 4. *The theorem is about recursive learning; nevertheless it does embody an unpleasant epistemological implication: there is no* ***effective*** *way for the learning agent to know when to stop applying the learning mechanism!*

[15]Provided we assume a straightforward recursive structure for prices, which turns out, usually, to be natural.

Remark 5. *Nothing in the assumptions guarantee tractable computability at any stage.*

5. Recursive Reflections

> "I went out to take a walk and to recollect after dinner. I did not want to determine a route for my stroll; I tried to attain a maximum latitude of probabilities in order not to fatigue my expectation with the necessary foresight of any one of them. I managed, to the imperfect degree of possibility, to do what is called walking at random; I accepted, with no other conscious prejudice than that of avoiding the wider avenues or streets, the most obscure invitations of chance. ... My progress brought me to a corner. I breathed in the night, in a most serene holiday from thought."
>
> Borges: *A New Refutation of Time*, in [**5**], pp. 225-6.

In recent years Sargent and his collaborators have developed what they call a *Recursive Macroeconomics* and before that there was the encyclopedic treatise by Lucas and Stokey (with Prescott) on *Recursive Methods in Economic Dynamics* ([**14**], [**23**]). *Recursive Macroeconomic Theory*, as Sargent et.al see it, is *recursive* in view of the three basic theoretical technologies that underpin the economic hypotheses: *sequential analysis*, *dynamic programming* and *optimal filtering.* To put it in terms of the pioneers whose theories underpin *Recursive Macroeconomic Theory*, the core of this approach harnesses the theoretical technologies of Abraham Wald's sequential analysis, Richard Bellman's dynamic programming and Rudolf Kalman's filtering frameworks. This means, the underlying economic hypotheses of *Recursive Macroeconomic Theory* will be framed and formalised in such a way as to be based on the mathematics of sequential analysis, dynamic programming and optimal filtering - whether or not economic reality demands it; whether or not economic behaviour warrants it; whether or not economic institutions justify it; and most basically, whether or not economic data conform to their requirements.

The word *recursive* is heavily loaded with connotations of dynamics, computation and numerical methods. But these connotations are also fraught with dangers. For example the methods of dynamic programming are provably complex in a precise sense; the equations that have to be solved to implement optimal filtering solutions are also provably intractable; ditto for sequential analysis.

The *recursion theoretic* framework for rational expectations equilibria that I have suggested in the main part of this essay is explicitly computational, algorithmically dynamic and meaningfully numerical. Moreover, the theorems that I have derived above, have an open-ended character about them. To put in blunt words, these theorems tell an implementable story about things that can be done; but they are silent about things that cannot be done[16]. But the stories are always about what can be done with well defined methods to do them - the algorithms. They are never about pseudo-recursive operators that are somnambulatory with regard to computations and numerical methods.

The two exercises presented in the third and fourth sections of this paper are prototypes of a strategy to be applied to defining areas of macroeconomics: growth, fluctuations, policy, capital, monetary and unemployment theories. The general idea is to strip the formal models in the respective fields of their topological underpinnings and replace them, systematically, with recursion theoretic elements in such a way that the open-endedness is enhanced and the numerical and computational contents made explicit and implementable. The specific way it was done in §3 was to concentrate on the use of the topological fixed-point theorem and replace it with a recursion theoretic fixed-point theorem. Similarly, in the case, of growth theory, say of the von Neumann variety, an analogous exercise can be carried out. This will lead to the use of the *second recursion theorem* rather than the one I have harnessed in this paper and growth will mean self-reconstruction and self-reproduction. In the case of fluctuations, the idea would be to replace all reliance on differential or difference equation modelling of economic dynamics and replace them with naturally recursion theoretic entities such as *cellular automata*[17]. The aim, ultimately, is to produce a corpus of theories of the central macroeconomic issues so that they can be collected under the alternative umbrella phrase: *Computable Macroeconomics*.

The question will be asked, quite legitimately, whether this line of attack aims also to maintain fidelity with microeconomic, rationality, postulates and, if so, in what way it will differ in the foundations from, say, *Recursive Macroeconomic Theory*. The canonical workhorse on which *Recursive Macroeconomic Theory* rides is the *competitive equilibrium model of a dynamic stochastic economy*. A rational agent in such an economic environment is, essentially, a *signal processor*. Hence, *optimal filtering* plays a pivotal role in this approach to macroeconomic theory. The simple answer, as a *Computable Macroeconomist,* would be that the rational agent of microeconomics

[16] I cannot resist recalling those famous 'last lines' of the early Wittgenstein:

> "What we cannot speak about we must pass over in silence." ([**27**], §7).

The sense in which this famous aphorism comes to mind is that in the recursion theoretic approach one does not invoke magic, metaphysics or other formal or informal tricks to solve equations. A problem is always posed in a specific context of *effective* methods of solution. The formal mathematical approach in standard economic theory is replete with magical and metaphysical methods to 'solve', 'prove' or determine solutions, equilibria, etc.

[17] There is more to this suggestion than can be discussed here. It has to do with the connections between dynamical systems theory, numerical analysis and recursion theory, if digital computers are the vehicles for experimental and simulation exercises. If, on the other hand, one is prepared to work with special purpose analogue computers, then the connection between dynamical systems and recursion theory can be more direct and it may not be necessary to eschew the use of differential or difference equations in investigating and modelling economic dynamics. I have discussed these issues in [**26**].

would be reinterpreted as a *Turing Machine* - a construction I have developed in great detail in, for example, [**33**], chapter 3. The analogous construction for the other side of the market is equally feasible, starting from re-interpreting the production function as a Turing Machine. This endows the production process with the natural dynamics that belonged to it in the hands of the classical economists and the early Austrians but was diluted by the latter-day newclassicals. What of market structure - i.e., *economic institutions?* Here, too, following in the giant footsteps of Simon and Scarf, there is a path laid out whereby an algorithmic interpretation of institutions is formally natural.

That leaves only, almost, that sacrosanct disciplining rule of economic theory: optimization. Recursion theoretic problem formulations eschew optimizations and replace them with *decision problems.* Simply stated, one asks whether problems have solutions or not and if they do, how hard they are and if they do not how must one change the problem formulation to make them solvable. *Decidability*, *solvability* and *computability* are the touchstones of a modelling strategy in Computable Macroeconomics. I am reminded, once again, as I conclude, of the early Witgenstein's poignant observations ([**27**], §6.51):

> "For doubt can exist only where a question exists, a question only where an answer exists, and an answer only where something *can be said."*

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CHAPTER 5

The Unreasonable Ineffectiveness of Mathematics in Economics

1. Preamble

"Well, you know or don't you kennet or haven't I told you every telling
has a taling and that's the he and the she of it."
James Joyce: Finnegan's Wake, p.213

Eugene Wigner's *Richard Courant Lecture in the Mathematical Sciences*, delivered at New York University on May 11, 1959, was titled, picturesquely and, perhaps, with intentional impishness *The Unreasonable Effectiveness of Mathematics in the Natural Sciences,* [**35**]. Twenty years later, another distinguished scientist, Richard W. Hamming, gave an invited lecture to the Northern California Section of the *Mathematical Association of America* with the slightly truncated title *The Unreasonable Effectiveness of Mathematics,* [**10**]. A decade or so later Stefan Burr tried a different variant of Wigner's title by organising a short course on *The Unreasonable Effectiveness of Number Theory*, [**4**]. Another decade elapsed before Arthur Lesk, a distinguished molecular biologist at Cambridge, gave a lecture at the Isaac Newton Institute for Mathematical Sciences at Cambridge University where he invoked yet another variant of the Wigner theme: *The Unreasonable Effectiveness of Mathematics in Molecular Biology,* [**17**]. First a physicist; then a computer scientist; then number theorists and, finally, also molecular biologists; so why not an economist, too? But note that my title is not about the *unreasonable effectiveness of mathematics in economics;* I am, instead, referring to its *ineffectiveness*. I was not a little influenced by the story behind Arthur Lesk's eventual choice of title (cf. [**18**]).

I.M. Gelfand, a noted mathematician, had suggested as a counterpoint to Wigner's thesis his own principle on *The Unreasonable Ineffectiveness of Mathematics in the Biological Sciences*. Lesk, unaware of this Wigner-Gelfand principle at the time his talk was conceived, had himself suggested a similar title for his own talk at the Newton Institute but was persuaded by the organisers to retain the Wigner flavour by dropping *ineffective* in favour of *effective*. To his surprise, when his talk was published in *The Mathematical Intelligencer*, the editors of the Journal, without his approval or knowledge, had inserted an inset ([**17**], p.29) describing the anecdote of the genesis of the Wigner-Gelfand principle. This prompted Lesk to recount the genesis of his own title in a subsequent issue of the Journal ([**18**])where he admitted that his preferred choice had been with the word *Ineffective.* He had proposed, to the organisers of a conference on 'Biomolecular Function and Evolution in the Context of the Genome Project', at the Newton Institute, in 1998, a talk with the title *On the Unreasonable Ineffectiveness of Mathematics in Molecular Biology*, which he - Lesk - thought reflected 'an echo of E.P.Wigner'. At this point the following reactions by the convener ensued:

> 'A prolonged and uneasy silence. Then: "But, you see, this is not quite the message that we want to send these people." More silence. Then: "Would you consider changing 'ineffective' to 'effective'?" '[**18**].

Lesk acquiesced, but did go on to point out that:

> 'Of course, the change in title had absolutely no effect on my remarks.' (ibid).

Anecdotal amusements apart, there was a more substantive point Lesk was trying to make with the intended title where *Ineffective* was emphasised in a Wignerian context. Lesk had felt that:

> '...biology lacks the magnificent compression of the physical sciences, where a small number of basic principles allow quantitative *prediction* of many observations to high precision. A biologist confronted with a large body of inexplicable observations does not have faith that discovering the correct mathematical structure will make sense of everything by exposing the hidden underlying regularities.
>
> .... A famous physicist once dismissed my work, saying: "You're not doing science, you're just doing archaeology!" .... [I]t emphasizes a genuine and severe obstacle to applications of mathematics in biology.' (ibid).

It is a neoclassical illusion, compounded by newclassical vulgarisations, that economics is capable of a similar 'magnificent compression' of its principles to 'a small number of basic principles' that has led to the faith in the application of the mathematical method in economics. Keynes famously thought 'if economists could manage to get themselves thought of as humble, competent people, on a level with dentists, that would be splendid' ([**16**],p.373). I would happily settle for being thought of as an archaeologist - but with the difference that ours is a subject where we investigate future archaeological sites that we are the architects of, as well as those left for us by a past of which we are the noble inheritors. We excavate, compare, decipher our version of hieroglyphics, decode and reconstruct the past, present and future, and read into and from all three of these repositories of time and its arrows. As a famous mathematician - who also made interesting contributions to analytical economics - observed, the veneer of mathematics tends:

'[T]o dress scientific brilliancies and scientific absurdities alike in the impressive uniform of formulae and theorems. Unfortunately however, an absurdity in uniform is far more persuasive than an absurdity unclad.'

([**28**], p.22)

The aim I have set forth for myself, in this essay, is to unclothe some of the uniforms of this empress, in her economic incarnations as a mathematical economist, and show her naked torso for what it is: *ineffective* and *non-constructive* in *the strict technical sense of formal recursion theory* and *constructive mathematics*; but also to try to unclothe a few of her generals and footsoldiers, as well, and show them in their splendid, unclad, absurdities.

Wigner's essay was admirably concise (it was only 16 pages long) and dealt with a host of vast and deep issues within the confines of those brief number of pages. It was divided into five subsections, in addition to a brief introduction[1]. I shall, to some extent, mimic that structure. Hence, the next section in this essay will try to summarise the salient points underlying alternative mathematical traditions. Wigner's brilliant lecture was delivered at a time when real analysis reigned supreme and formalism of one variety or another ruled, implicitly or explicitly[2]. There was, if not universal agreement, blissful ignorance of alternative traditions that may have provided different perspectives on physical theories, at least in the practice of the more formalized sciences. Hence, Wigner could happily confine his discussions on '*What is Mathematics?*'[3] to just a page and a half! Today such conciseness is almost impossible, even from the point of view of the knowledge of the mathematically minded economist. *Classical real analysis* is only one of at least four mathematical traditions within which economic questions can be formalized and discussed mathematically. *Nonstandard*, *constructive* and *computable* analyses have been playing their own roles in the formalization and mathematization of economic entities - but almost always within what I call the *closure* of neoclassical theory.

[1]The five main subsections were titled:

- What is Mathematics?
- What is Physics?
- The Role of Mathematics in Physical Theories.
- Is the Success of Physical Theories Truly Surprising?
- The Uniqueness of the Theories of Physics.

[2]The one footnote in which *intuitionism* is mentioned was a reference to Hilbert's disdainful dismissal of it (cf. [**35**], footnote 4).

[3]I found it mildly surprising that Wigner, in a *Richard Courant Lecture*, did not refer to Courant's own famous attempt to provide an answer to the seemingly simple question '*What is Mathematics*?' with a whole book with that title (cf. [**6**]). Courant's answer, by the way, was to *show* what *mathematics is* by describing, explaining and demonstrating what they actually *do*. That was, perhaps, not suitable for Wigner's aims in the lecture.

Wigner's discussion of Physics and Physical theories are predicated upon the explicit and implicit fact that such theories have organising and disciplining criteria such as *invariance*, *symmetry* and *conservation* principles (cf. also [**36**]). Lesk, on the other hand, by confining his discussion to that part of Molecular Biology which has come to be called *Computational Molecular Biology*, was able to single out the restraining and guiding hands provided by the laws of physics and chemistry, without subscribing to any kind of reductionism. He coupled these underpinnings to the mechanism of evolution and the role of chance in the latter, in particular, as organising principles to demonstrate the effectivity of mathematical theorising in *Computational Molecular Biology.* These organising principles operate, of course, also in Molecular Biology in general; it is just that, by concentrating on the *computational subset,* Lesk was able to characterize the canonical mathematical methods used as being *sequence alignment* and *structure superposition.*

If I was to follow Lesk's strategy, then I have one of three possibilities. I can either work within the framework of *General Equilibrium Theory* (GET) as the core of neoclassical economics and choose its computational 'subset', i.e., *Computable General Equilibrium* theory (CGE) and discuss the unreasonable effectiveness, or not, of mathematics inside these, narrow but well-defined citadels of application of mathematics in economics. The second possibility is to choose the computable subset of either GET or some other part of economic theory, not necessarily neoclassical in spirit, and highlight the effectivity of mathematical theorising in these subsets. The third alternative is to confine my attention to that amorphous practice, increasingly called Computational Economics, and discuss the effectivity of mathematical theorising in this field. I rule out the latter two alternatives in view of a lack of clearly defined disciplining criteria that would make it possible to provide a decent discussion within the confines of a single, page-constrained, essay. Therefore, I choose, in §3, to define the 'economic theory' to which mathematics has been applied *ineffectively*, and *unreasonably* so, as GET and confine myself to brief remarks on other, related areas of economics aspiring to the status of a mathematical discipline.

I try, in §4 to suggest that we return to the tradition of the methodologies and epistemologies of the natural historian - perhaps, implicitly, also that of the dentist and the archaeologist. This final section is also a reflection of the way mathematics might develop and to speculate that the possible scenarios would reinforce the return of economics to what it once was: Political Arithmetic.

2. Mathematical Traditions

> "Among the abstract arts music stands out by its precise and complex articulation, subject to a grammar of its own. In profundity and scope it may compare with pure mathematics. Moreover, both of these

testify to the same paradox: namely that *man can hold important discourse about nothing.*"

Michael Polanyi: **Personal Knowledge** ([**23**]), p.193; italics added.

If 'Poetry Makes Nothing Happen'[4], what, then, of philosophy and mathematics? Do they make anything happen? Surely, for them - and for poetry - to make anything happen, they have to be about *something.* What are they *about*, then? Michael Dummett's enlightened and informed criteria may offer a starting point[5]:

"The two most abstract of the intellectual disciplines, philosophy and mathematics, give rise to the same perplexity: what are they *about*? .....

An uninformative answer could be given by listing various types of mathematical object and mathematical structure: mathematicians study the properties of natural numbers, real numbers, ordinal numbers, groups, topological spaces, differential manifolds, lattices *and the like.* .....

A brilliant answer to our question .. was, essentially, that **mathematics** is not about *anything in particular*: it **consists**, rather, **of the systematic construction of complex deductive arguments**. Deductive reasoning is capable of eliciting, from comparatively meagre premisses and by routes far from immediately obvious, a wealth of often surprising consequences; **in mathematics such routes are explored** and the means of deriving those consequences are stored for future use in the form of propositions. Mathematical theorems, on this account, embody deductive subroutines which, once discovered, can be repeatedly used in a variety of contexts."

[**9**], pp.11-14; bold emphasis added.

[4]I have in mind Auden's poignant eulogy to Yeats: "Mad Ireland hurt you into poetry, Now Ireland has her madness and her weather still, *For poetry makes nothing happen*: it survives.. ." Auden: 'In Memory of W.B.Yeats' (italics added).

[5]Dummett's question, and enlightened answer, is entirely consistent with Hardy's analogous question and equally felicitous answer - except that the latter aimed at characterizing the *mathematician*: '**A mathematician**, like a painter or a poet, **is a maker of patterns**. If his patterns are more permanent than theirs, it is because they are made with *ideas.* A painter makes patterns with shapes and colours, a poet with words..... .

The mathematician's patterns, like the painter's or the poet's, must be *beautiful*; the ideas, like the colours or the words, must fit together in a harmonious way. ...

It may be very hard to *define* mathematical beauty ... but that does not prevent us from recognising one when we [see] it. ([**12**], pp. 84-5; bold emphasis added).

Hardy's mathematician, who is a 'maker of beautiful patterns', is exactly Dummett's 'constructor of complex deductive arguments'. Just as Dummett's 'complex deductive arguments' are arrived at by 'routes far from immediately obvious', the 'beauty' of the patterns devised by Hardy's mathematicians are 'hard to define'.

In other words, mathematics is about *proof*. I believe this to be a valid and standard characterisation which helps delineate the different 'schools' of mathematics in terms of it. Some 'routes' for the 'construction of complex deductive arguments' are aesthetically more acceptable, on clearly defined criteria, to one class of mathematicians and others to another class and this is one of the ways these different 'schools' have tried to distinguish themselves from each other. As one may expect, different 'routes' may lead the traveller to different destinations - to different classes of mathematical objects and, equally, different classes of mathematicians have approved - and disapproved, as the case may be - for aesthetic and epistemological reasons, as valid or invalid, alternative structures of 'deductive arguments'. In other words, there is no such thing as universally valid and acceptable class of 'deductive arguments' that must exclusively be used in the exploratory journeys along 'far from immediately obvious routes'. Many times, the 'routes' are *discovered*; at other times, they are *invented*. A whole, respectable and resilient, mathematical movement, methodologically and epistemologically rigorous in its ways, has always claimed that there are no 'routes' out there, laid out by the Gods, for mathematicians to *discover*. Mathematicians, equipped with a stock of ideas, explore alternative 'routes' with aesthetically and epistemologically acceptable deductive structures - i.e., construction rules - and create - i.e., invent - new pathways that lead to unexpected destinations. Others live in a world of Platonic shadows and *discover* routes that have been laid out by the Gods[6]. The former are called the *Intuitionists*; the latter are the *formal Platonists*. These two classes do not, of course, exhaust the class of mathematicians; there are varieties of Platonists and, equally, varieties of Intuitionists, and others besides: *Hilbertian Formalists, Bourbakists, Bishop-style Constructivists, Logicists,*and so on. A flavour of the main differences, based on the Dummett-Hardy characterisation of mathematics and the mathematician, can be discerned from the following artificial dialogue between a mythical Intuitionist (**I**) and an undifferentiated Formalist (**ME**)[7]:

EXAMPLE 3.

ME: I have just proved $\exists x A$.
I: Congratulations. *What* is it? *How* did you prove it?
ME: It is an economic equilibrium. I assumed $\forall x \neg A$ and derived a contradiction.
I: Oh! You mean you 'proved' $\neg\forall x \neg A$?
ME: That's what I thought I said.
I: I don't think so.

EXAMPLE 4.

[6]As David Ruells perceptively observed in his 'Gibbs Lecture':' WE LIKE TO THINK OF THE DISCOVERY OF MATHEMATICAL STRUCTURE AS WALKING UPON A PATH LAID OUT BE THE GODS. BUT .... MAY BE THERE IS NO PATH..' ([**24**], p.266).

[7]Adapted from [**21**]. Anticipating the characterisation of a *Mathematical Economist* in the next section, ME in this dialogue refers to such a being.

ME: I have proved $A \vee B$.

I: Excellent. Which did you prove?

ME: What?

I: You said that you had proved *A or B* and I was wondering whether you had proved *A or B or both.*

ME: None of them! I assumed $\neg A \wedge \neg B$ and derived a contradiction.

I: Oh, you mean you proved $\neg [\neg A \wedge \neg B]$?

ME: That's exactly right. Your way of stating it is simply another way of saying the same thing.

I: No - not at all.

As a direct instance of the first example, with immediate implications for the foundations of GET, there is the case of Brouwer's original proof of his celebrated fix point theorem. He - and legions of others after him, scores of whom were economists - did *not* prove that '*every* f (in a certain class of functions) *has a fixed point*' (i.e.,$\exists xA$) . What he did prove was: '*There is no* f (in a certain class of functions) *without a fixed point*' (i.e., $\neg\forall x \neg A$). The equivalence between the two propositions entails an acceptance of the deductive validity of: $\neg(\neg A) \Leftrightarrow A$. Brouwer himself came to reject the validity of this principle and, forty years after the initial publication of his famous result, reformulated the proof without reliance on it [**3**].

The second example illustrates a widely used *non-constructive* principle, most conspicuously utilised in the 'proof' of the Bolzano-Weierstrass Theorem (cf.[**8**], pp.10-11), which is implicitly assumed in all 'constructions' of equilibria in CGE models. The reason for some mathematicians to object to proofs of the sort in the second example is that it shows that one or the other of two specific conditions hold without specifying a means to determine which of them is valid in any specific set of circumstance. It is as if the mathematician in his journey along those characterising 'routes' comes to a fork in the pathway and is told that one or the other of the alternatives will lead to a specified destination, but is not given any further information as to which one might do so. Is she to take both, simultaneously or one after the other - even along mathematical pathways that are routinely non-finite, as, indeed, the case in the Bolzano-Weierstrass Theorem? What are the consequences of traversing an infinite path, speckled with forks, where undecidable disjunctions can paralyse progress? The classical mathematician is not troubled by such conundrums; almost all other traditions tame undecidable disjunctions at their buds. The mathematical economist and almost all applications of mathematics in economics traverse with princely unconcern for the forks, donning the proverbial blind every time such bifurcations are encountered. No wonder, then, that the subject remains entwined and entangled in numerical indeterminacies and logical undecidabilities - but this is an item for the next section.

In the above explicit instance of the first example I have invoked the *idea* of a *function* without trying to define its meaning. So, what is a *function*?[8] How do different mathematical traditions confront the task of answering this question? The ordinary meaning of the word 'function' is associated with the 'idea' of performing a task. All mathematical traditions, with the notable exception of what, for want of a better name, I shall call 'classical real analysis' or 'classical mathematics', each in their own way, retain fidelity to the ordinary meaning of the word 'function' in their specialised characterisations. Historically, in mathematics, the meaning of the concept was intimately tied to the notion of a *rule*, a *procedure*, a *set of instructions to perform a task*. Thus, for example, a function f was supposed to enable a mathematician to calculate, given a number, say x, - real, natural, or whatever - another number, denoted by $f(x)$ such that, whenever $x = y$, then $f(x) = f(y)$. This was to impose some disciplining criteria in the procedures - the methods by which patterns are created. However, at the hands of the classical mathematicians this became ossified as the well-known Kuratowski-Dirichlet definition[9]:

DEFINITION 18. *A function $f : A \longrightarrow B$ is any subset $f \sqsubseteq (A \times B)$ which satisfies: $(\forall x \in A)\,(\exists y \in B)$ s.t $(x, y) \in f \,\&\, (x, y') \in f \Longrightarrow y = y'$.*

However, this definition - 'function as a graph' - makes sense *only within set theory*.[10] The definition has severed all connections with the meaning attributed to the word 'function' in ordinary discourse; there is little sense in which it can be understood to 'perform a task'. The idea of a 'rule', a 'procedure', encapsulated within the historical definition of the idea - concept - of a 'function' has disappeared. This is best illustrated by an example (cf. [**20**], p.41). The following 'formulas' for computing

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[8] I could, instead, proceed, at this point, by asking the analogous question: what is a *number*? or any related question, substituting, for '*function*' and '*number*', other basic '*ideas*' that are the objects manipulated by deductive arguments to construct the patterns that pave the route. For reasons of convenience and familiarity, I shall confine my discussion to the object referred to as 'function'.

[9] I add the name Dirichlet to the standard naming which justly credits Kuratowski with this definition, for historical reasons. It was, however, Dirichlet who initiated this particular tradition, culminating in Kuratowski's 'function as a graph' definition. Dirichlet's definition, in terms of open, continuous, intervals, remains the touchstone, as one can see from the way the Bishop-style constructivists and the Brouwerian Intuitionists have, eventually, defined functions. (cf. for example, [**15**], p.274: 'It thus appears that an adequate definition of a function for a continuous interval (a, b) must take the form given to it by Dirichlet'. Hobson does not elaborate upon the meaning of 'adequate', but it certainly had nothing to do with 'performing a task'.). Of course, the motivation and criteria in the latter two approaches were quite different from those of Dirichlet and Kuratowski.

[10] And *set theory* is only one of at least four sub-branches of mathematical logic; the others being: *proof theory*, *recursion theory* and *model theory*. Loosely speaking, but not entirely inaccurately, it is possible to associate one particular class of numbers with each of these sub-branches of logic: *real numbers*, *constructive numbers*, *computable numbers* and *non-standard numbers*, respectively. Analogously, each of these form the subject matter of: real analysis, constructive analysis, computable analysis and non-standard analysis. Which of these numbers and, hence, which kind of analysis, is appropriate for economic analysis is never discussed in any form or forum of mathematical economics or mathematics in economics. It is taken for granted that real numbers and its handmaiden, real analysis, is the default domain. Why?

the square of two numbers, defined on the reals, are equivalent in the 'function as a graph' definition implied by the above Dirichlet-Kuratowski characterization:

$$f(x, y) \equiv (x + y)^2$$

$$g(x, y) \equiv x^2 + 2xy + y^2$$

However, as tasks to be performed, say on a digital computer via a simple program, they result in different sets of instructions. The key point is this: *whether the notion of a function that is based on 'performing a task' can be represented in set theory in such a way as to capture its full intuitive content remains an open question.* In spite of this indeterminacy, mathematical economists - and, so far as I know, all economists who apply mathematics in economics - routinely rely on this particular definition for their so-called rigorous notion of a function.

On the other hand, almost all other traditions, as mentioned above, in their definitions of the notion of a function, retain fidelity with the ordinary meaning and mathematical tradition. Thus, in *Bishop-style constructive mathematics* the distinguishing starting point is that all existence proofs should be constructive in the precise sense that every proof can be implemented, in principle, as an algorithm in a computer[11] to demonstrate, by explicit construction, the object in question. This means, firstly, that the law of the excluded middle[12] is not invoked in infinitary cases; secondly, as a by-product of such a discipline on existence as construction, all functions are required to be uniformly continuous in each closed interval. In other words, if mathematics is about proving theorems, and if proofs are to be constructive - i.e., performable tasks, at least in principle, by a set of explicit instructions - then each function must be characterized in a certain precise way. Hence, Bishop-style constructive mathematics retains fidelity with the ordinary meaning of the concept of function by endowing it with certain *mathematical properties* - i.e., uniformly continuous in each closed interval - such that when they are used in the pattern formation activities of the mathematician they will facilitate the 'performance of tasks'.

In that variant of constructive mathematics known as *Brouwerian Intuitionism*, the starting point is what is known as 'free choice sequences' - where a *rule* for determining a real number is a result of *free choices* by an autonomous human intelligence, independent of the strictures of the undecidable disjunctions of classical logic. This implied, in Brouwerian Intuitionism, that *all functions from reals to reals are continuous*. Here, too, starting from a metatheoretic assumption - construction of the primitives by 'free choice sequences', based on what Brouwer considered was the domain of activity of the mathematician - his or her autonomous intelligence - one was led to consider a characterisation of functions that retained fidelity with tradition and the ordinary meaning of the word.

[11] The computer could be *digital* or *analog*.

[12] *Tertium non datur.*

Then, there is the class of *computable functions*, the domain of the recursion theorist, acting under the discipline of the *Church-Turing Thesis*. The most direct way of describing or characterising these functions - although not the mode that I find most congenial - is to say that they are that subset of the functions defined in classical mathematics which can be implemented on an ideal digital computer - i.e., the *Turing Machine*. Next, invoking the Church-Turing Thesis, one identifies them, depending on the aims of the analysis, as the class of *partial recursive functions or Church's λ-definable functions*, etc. Then, by way of a elementary counting arguments it is shown that there are 'only' a countable infinity of *Turing Machines* and, hence, also of *partial recursive functions,* implying thereby that the complement of this set in the class of all classically defined functions contains the uncomputable functions. They are, therefore, uncountably infinite in number! This, by the way, is the class of functions routinely used and assumed in mathematical economics of every variety, without exception.

It is, of course, possible to continue a finer classification of varieties of constructive mathematics and, also, varieties of Formalists, Platonists, and Logicists and so on[13]. However, this will achieve no particular purpose beyond that which has been achieved with the above few considerations and characterisations for the following reasons. Given the Hardy-Dummett characterisation of mathematics and the activity of the mathematician in terms of 'the systematic construction of complex deductive arguments', it was inevitable that there would be some dissonance in the meaning and interpretation to be attached to 'construction' and the acceptability or not of valid deductive rules for the 'construction'. Depending on the kind of deductive rules and constructions accepted as valid, there are different ways to characterise mathematics and mathematicians. I have highlighted a few of the possible ways to do this - but many other ways could have been attempted with equal ease, which would have resulted in a many-splendoured world of possible mathematics and mathematicians. The main point to note is that it is not a monolithic world, characterised by one concept of 'proof' and a single way of 'constructing patterns' from an inflexibly determined set of deductive rules.

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[13] Although it may appear paradoxical, I am of the opinion that *non-standard analysis* should be placed squarely in the constructive tradition - at least from the point of view of practice. Ever since Leibniz chose a notation for the differential and integral calculus that was conducive to *computation*, a notation that has survived even in the quintessentially *non-computational* tradition of classical real analysis, the practice of non-standard analysis has remained firmly rooted in applicability from a computational point of view. Indeed, the first *modern* rejuvenation of the non-standard tradition in the late 50s and early 60s, at the hands of Schmieden and Laugwitz (cf. [**26**]), had constructive underpinnings. I add the caveat 'modern' because Veronese's sterling efforts (cf.[**34**]) at the turn of the 19th century did not succeed in revitalising the subject due to its unfair dismissal by Peano and Russell, from different points of view. The former dismissed it, explicitly, for lacking in 'rigour'; the latter, implicitly, by claiming that the triple problems of the *infinitesimal, infinity* and the *continuum* had been 'solved'.

3. A Glittering Deception

"And he wondered what the artist had intended to represent (*Watt knew nothing about painting*), ... a circle and a centre not its centre in search of a centre and its circle respectively, in boundless space, in endless time (*Watt knew nothing about physics*) ... ."

Samuel Beckett: **Watt** ([**2**]), p.127; italics added.

In their seminal textbook on mathematical economics, Arrow and Hahn ([**1**]) state that their 'methods of proof are in a number of instances quite different' from those in Debreu's classic, codifying, text on the *Theory of Value* ([**7**]). Debreu, in turn, claimed that he was treating the theory of value, in his book, 'with the standards of rigor of the contemporary formalist school of mathematics' and that, this 'effort toward rigor substitutes correct reasonings and results for incorrect ones' (ibid, p.viii). But we are not told, by Arrow and Hahn or by Debreu, either what these 'different methods of proof' mean in the form of new insights into economics or what concept of 'rigor' underpins the substitution of 'correct reasonings and results for incorrect ones'[14].

On the other hand, the crowning achievement of the Arrow-Debreu reformulation of the Walrasian problem of the existence of an economic (exchange) equilibrium was its formal demonstration as the solution to a fixed point problem. In addition to this, there was the harnessing of theorems of the separating hyperplane - more generally, the Hahn-Banach Theorem and Duality Theorems - to formally demonstrate the mathematical validity of the two fundamental theorems of welfare economics. Thus, existence of economic equilibrium and welfare economics were given so-called rigorous mathematical formulations and formal demonstrations as theorems of various sorts. Both Arrow and Debreu were handsome in their acknowledgement of debts to the trails that had been blazed by the pioneers in mathematical method for such issues: von Neumann, Wald and Nash being the most prominent among them, but also numerous mathematicians - Brouwer, Kakutani, Banach, to name the obvious ones.

As a sequel to the codification achieved by Debreu, Scarf began a sustained research program to 'constructivise' one aspect of the mathematics of general equilibrium theory: the problem of existence. Early on, he had realised that proving existence by non-constructive means was unsatisfactory from the point of view of economics as an

[14]The inspiration for the heading of this section came about as follows. Jacob Schwartz, a distinguished mathematician, but also the author of a fine, though unfortunately little acknowledged, monograph on 'the mathematical method in analytical economics' [**27**] , observed pungently: "The very fact that a theory appears in mathematical form, that, for instance, a theory has provided the occasion for the application of a fixed-point theorem .. somehow makes us more ready to take it seriously. .. The result, perhaps most common in the social sciences, is *bad theory with a mathematical passport*. .. The intellectual attractiveness of a mathematical argument ... makes mathematics a powerful tool of intellectual prestidigination - *a glittering deception in which some are entrapped, and some, alas, entrappers.* ([**28**] , pp. 22-3, italics added)

applied subject, even apart from possible aesthetic motivations and intellectual challenges to constructivise non-numerical concepts. This is the research program under the rubric of *Computable General Equilibrium theory* (CGE), with far reaching policy implications. Surprisingly, no one has tried to constructivise or effectivise the formalizations of the two fundamental theorems of welfare economics, on which momentous policy prescriptions - even of a philosophical nature - depend.

The main question I wish to pose in this section is the following: suppose the modern masters of mathematical general equilibrium theory had been more enlightened in their attitude and, possibly, knowledge of mathematics and its philosophy and epistemology, and had they taken the trouble to 'treat the theory of value with the standards of rigour of' *not only* 'the contemporary formalist school of mathematics', but with the 'standards of rigour' of other contemporary schools of mathematics, how much of their economic propositions would remain valid? In other words, did the spectacular successes of the *Theory of Value* depend on the fortuitous fact of having been formalised in terms of 'the contemporary formalist school of mathematics'?

A subsidiary question I pose, next, is whether Scarf's program can be carried through successfully. The claim, by leading applied economists, is that it has been carried through successfully and GET is, now, an eminently applicable field, with clear computational and numerical content.

My answer to the first question is that the results are hopelessly sensitive to the kind of mathematics used. The answer to the second question is that the Scarf program cannot succeed in its aim to constructivise the equilibrium existence problem of GET, i.e, the constructive and computable content of CGE is vacuous.

Before I consider the *unreasonable ineffectiveness* of mathematical general equilibrium theory, there are a few ghosts to rekindle and some to lay to rest. The first ghost that deserves a rekindling is the existence problem - and from two points of view. Firstly, is it really necessary to pose as a formal, mathematical, problem the question of equilibrium existence? Hicks did not think so:

> "[T]he [*Value and Capital*] model is not much affected by the criticism, made against it by some mathematical economists, that the existence of an equilibrium, *at positive prices*, is not demonstrated. ..... Existence, from my point of view, was a part of the hypothesis: I was asking, if such a system existed, how would it work?"
>
> [**14**], p.374; italics added.

With an eye at some questions to be raised below, let me ask: why 'at positive prices' and not 'positive *integer* or *rational* prices'?

Next, even if there is a satisfactory answer to the first question - in spite of the weight of Hicks' vision and stand - was it necessary to formulate the equilibrium existence problem as a fix point problem? Smale did not think so:

"We return to the subject of equilibrium theory. The existence theory of the static approach is deeply rooted to the use of the mathematics of fixed point theory. Thus one step in the liberation from the static point of view would be to **use a mathematics of a different kind**. Furthermore, proofs of fixed point theorems traditionally use difficult ideas of algebraic topology, and this has obscured the economic phenomena underlying the existence of equilibria. Also the economic equilibrium problem presents itself most directly and with the most tradition not as a fixed point problem, but as an *equation*, supply equals demand. **Mathematical economists have translated the problem of solving this equation into a fixed point problem**.

I think it is fair to say that for the main existence problems in the theory of economic equilibrium, **one can now bypass the fixed point approach and attack the equations directly to give existence of solutions, with a simpler kind of mathematics** and even mathematics with dynamic and algorithmic overtones."

[**29**], p.290; bold emphasis added.

Why, then, did 'mathematical economists translate the problem of solving' equations 'into a fixed point problem'? Also, suppose we return to the 'equation' tradition but impose natural economic constraints on the variables, parameters and constants of the supply-demand relations. Such natural constraints would imply *integer* and *rational* valued variables, constants and parameters. To return to a variant of the question I posed just after the Hicks quote: why the fetishism of looking for 'non-negative prices' in an equilibrium configuration? Surely, a return to the equation tradition, with non-negative integer or rational valued variables, constants and parameters means a confrontation with a combinatorial monster: *Diophantine equation*. In such an environment, the economic problem would naturally become a (*recursion-theoretic) decision problem* and will no longer be a traditional optimization problem[15].

As a tentative answer to these two questions I can do no better than recall the immortal words of the great Charles Dickens:

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[15] It is worth mentioning, especially in the context of the forum for which this essay is prepared, that the supreme examples of equation systems that were solved without recourse to any kind of fixed point theorem were those presented in Sraffa's remarkable little book ([**30**]). Of course, the Sraffa systems were not of the supply=demand variety; nevertheless, they were equilibrium systems of a sort. That legions of mathematical economists, both well-meaning and hostile, spent time and effort to re-prove what had been proved quite adequately, although not by formalistic means, remains an unfathomable mystery to me. It was as if no one could understand simple, constructive proofs or, worse, that even mathematically competent readers were one-dimensional in their knowledge of techniques of proofs. Why someone did not use Sraffa's perfectly adequate and competent methods to *re-prove*, say, the Perron-Frobenius theorem, and free *it* from the shackles of reliance on the non-constructive Brouwer fixed point theorem is also a mystery to me.

"They took up several wrong people, and they ran their heads very hard against wrong ideas and persisted in trying to fit the circumstances to the ideas, instead of trying to extract ideas from circumstances."

Charles Dickens: **Great Expectations**, italics added.

My point is that the mathematical economists 'persisted in trying to fit the circumstances', i.e., existence of economic equilibrium question, 'to the ideas', i.e., to the mathematics they knew, 'instead of trying to extract ideas', i.e., instead of trying to extract possible mathematical ideas, 'from circumstances', i.e., from the economic circumstances - as Sraffa did.

Let me, now, return to GET and CGE and their *mathematically unreasonable ineffectiveness*. Here I shall mean, by ineffectiveness, the strict technical sense of being uncomputable or non-constructive. The caveat unreasonable signifies the fact that the mathematics used - i.e., methods of proof utilized in GET and CGE - and the axioms assumed - were not only economically injudicious but also unnecessary and irrelevant from every conceivable numerical and computational point of view.

The formal underpinnings of the economic theory enunciated in Debreu's *Theory of Value* depend crucially on the following mathematical axiom, concepts and theorems:

(1) The *axiom of completeness* ([**7**], §1.5.*d*, p.10)
(2) *Compactness* ([**7**], §1.6.*t*, p.15)
(3) *Continuity - topologically characterized* ([**7**], §1.7.*b*, p.15)
(4) The *maximum-minimum theorem* or, as Debreu has named it, the *Weierstrass theorem* ([**7**],§1.7.*h (4')*, p.16)
(5) *Separating hyperplane theorems* ([**7**], §1.9, pp.24-5)
(6) The *Brouwer and Kakutani Fixed Point Theorems* ([**7**], §1.10, p.26)

Let me, now, add, to this mathematical apparatus in the *Theory of Value,* the following six theorems, propositions and facts[16]:

THEOREM 10. *(Specker's Theorem in Computable Analysis)*
A sequence exists with an upper bound but without a least upper bound.

PROPOSITION 10. *The Heine-Borel Theorem (on Compactness) is invalid in Computable Analysis*

CLAIM 1. *There are 'clear intuitive notions of continuity which cannot be topologically defined' (*[**?**]*, p.73)*

[16] I should, for completeness, add a list of the deductive rules that are valid in different kids of mathematics, too. For example, the reason for the failure of the Bolzano-Weierstrass theorem in constructive mathematics is the uncritical use of the law of the excluded middle. This law and the law of double negation are the 'culprits' in the failure of the Brouwer fixed-point theorem in Brouwerian Intuitionistic mathematics. But I have refrained from making these explicit in view of the brief hints given in the previous section.

Proposition 11. *The Bolzano-Weierstrass Theorem is invalid in Constructive Mathematics*

Claim 2. *The Hahn-Banach Theorem is invalid in its classical form in Constructive and Computable analysis.*

Claim 3. *The fixed point theorems in their classical versions are not valid in (Intuitionistically) Constructive Mathematics.*

If the above theorem, propositions and claims are appended to the *Theory of Value*, or to any later 'edition' of it such as [**1**], then it can be shown that none of the propositions, theorems and claims of a mathematical sort would retain their validity without drastic modifications of their economic content and implications. In particular, not a single formal proposition in the *Theory of Value* would have any numerical or computational content.

Suppose we add, to the above six supplementary 'riders', the following **Claim** on the *Uzawa Equivalence Theorem* ([**32**]):

Claim 4. *The Uzawa Equivalence Theorem is neither constructively nor computably valid.*

Then, in conjunction with the invalidity **Proposition** of the Bolzano-Weierstrass Theorem, the above **Claim** implies that the constructive content of CGE models, and their computational implications for economic policy analysis, are vacuous.

A similar exercise can be carried out for *every* sub-field of economic theory to which the mathematical method has been applied - in particular, game theory. It will be a tedious exercise but I suspect that, eventually, such an exegesis can even be automated! The general strategy would be to identify the key mathematical axioms, theorems and concepts that underlie any particular mathematics applied to a sub-field of economic theory and, then, to investigate their constructive, computable, non-standard or real analytic nature. Thus, for example, a seemingly innocuous application of dynamical systems theory in endogenous theories of the business cycle would also be susceptible to such an exegetic exercise. Any use of the Cauchy-Peano theorem in the existence theory for differential equations will fall foul of the failure of the validity of the Bolzano-Weierstrass Theorem in Constructive mathematics. This is because the Bolzano-Weierstrass Theorem is equivalent to the Ascoli Lemma which, in turn, is used to simplify the proof of the Cauchy-Peano Theorem. 'O what a tangled web we weave...' (*pace* Sir Walter Scott)!

In passing, it must, of course, be pointed out that fixed point theorems did not enter economic analysis by way of the existence problem of general equilibrium theory; the entrance points were game theory and growth theory - both at the hands of von Neumann. For reasons of space, my remarks on these two issues will have to be brief. First of all, as regards game theory, I have already tried to make a case for recasting every game theoretic problem in economics as an *Arithmetical Game* (cf. [**33**], ch. 7). This implies that their solutions can be reduced to Diophantine decision problems, in analogy with the equation approach to the economic equilibrium existence problem. Secondly, in the case of growth theory, the original fixed point problem of a minimax system, was 'simplified' into a separating hyperplane problem. But as pointed out above, the separating hyperplane theorem, or the Hahn-Banach theorem, has neither an exact equivalent formulation in constructive mathematics nor is it known, at present, whether it is valid in computable analysis. However, the fact remains that growth theory is a problem of self-reproduction and self-reconstruction, and to that extent the theory can felicitously be reformulated as a recursion theoretic problem and the standard, numerically implementable, fixed point theorem of recursion theory can be applied.

What kind of lessons are we to draw from this particular exercise in exegesis? There is almost no better way to phrase the main lesson to be drawn than in the words of a leading newclassical mathematical economist:

> ".. [A]s economic analysts we are directed by, if not *prisoners of*, the mathematical tools that we possess."
>
> [**25**], p.xix; italics added.

Should we not, if we are 'prisoners' of anything, try to liberate ourselves from that which imprisons us?

4. The Path We Will Never Walk Again

> "Mathematics is not a finished object based on some axioms. It evolves genetically. This has not yet quite come to conscious realization. ...
>
> [T]here might someday be entirely new points of view, even about sets or classes of sets. Sets may someday be considered as 'imaginary.' I think that will come to pass, though at present it is not admissible.
>
> *Mathematics will change. Instead of precise theorems*, of which there are now millions, *we will have, fifty years from now, general theories and vague guidelines*, and the individual proofs will be worked out by graduate students or by computers.
>
> *Mathematicians fool themselves when they think that the purpose of mathematics is to prove theorems, without regard to the broader impact of mathematical results.* Isn't it strange.
>
> *In the next fifty years there will be, if not axioms, at least agreements among mathematicians about assumptions of new freedoms of constructions, of thoughts.* Given an undecidable proposition, there will be a preference as to whether one should assume it to be true or false. Iterated this becomes: some statements may be undecidably undecidable. This has great philosophical interest

Ulam ([**5**], pp. 310-2; italics added)

It is not for nothing that one of the great masters of modern economic theory, even in its mathematical versions, John Hicks, never tired of emphasising the importance of the accounting tradition in economic analysis, particularly dynamic economics:

> "In all its main forms, modern economic dynamics is an accounting theory. It borrows its leading concepts from the work 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."
>
> [**13**]

Somewhere between the Political Arithmetician[17] and the Accountant lies the task of the quantitative economist's analytical role and none of the theoretical or applied tasks of these two paradigmatic figures requires anything more than arithmetic, statistics and the rules of compound interest[18]. These, in turn, require nothing more than an understanding of the conditions under which systems of equations can and cannot be solved. But what kind of quantities do these equations encapsulate as parameters, constants and variables? Surely, the kind of quantities that enter the equations of the Political Arithmetician and the Accountant cannot be other than rational or natural numbers - negative and non-negative?[19] I cannot see any role for real numbers in quantitative economics and, hence, none whatsoever for real analysis.

Richard Hamming wondered, similarly, about the appropriate kind of numbers for probablity theory[20]:

> "Thus without further examination it is not completely evident that the classical real number system will prove to be appropriate to the needs of probability. Perhaps the real number system is: (1) not rich enough - see non-standard analysis; (2) just what we want - see standard mathematics; or (3) more than is needed - see constructive mathematics, and computable numbers. ...
>
> What are all these uncountably many non-computable numbers that the conventional real number system includes?....

[17]The title for the heading of this section was inspired by the last line in one of the stanzas of one of Antonio Machado's great *Cantares* (my translation): *Caminante no hay camino, se hace camino al andar. Al andar se hace camino, y al volver la vista atras se ve la senda que nunca se ha de volver a pisar. ...'*

[18]I have always tried to read Sraffa's *magnum opus* as if it was an accountant's manual, supplemented by ingenious constructive devices to prove the solvability of systems of equations.

[19]The lasting contribution of economic analysis, to the mercantile culture of the modern era, was - in my opinion - double-entry bookkeeping. The Political Arithmetician and the Accountant has to deal with credit as well as the debit side of such bookkeeping discipline and, hence, it is not enough to confine attention to equations constrained by non-negative numbers. Negative numbers, even in their origin, play a role in doulbe-entry bookkeeping.

[20]Simply substitute 'economic theory' for 'probability theory', when reading this quote!

> The intuitionists, of whom you seldom hear about in the process of getting a classical mathematical education, have long been articulate about the troubles that arise in the standard mathematics ....
>
> What are we to think of this situation? What is the role in *probability theory* for these numbers which can never occur in practice?"
>
> [**11**]

Thus, the only kind of equations that can play any role in the analytical activities of the Political Arithmetician and the Accountant are *Diophantine equations*. How can the problem of solvability of such equations be studied and what methods are available to systematise and routinise their use? The paradoxical answer to both of these questions is that the problem of solvability is intractable and their systematic and routinised study is almost impossible. They share, with that other 'Cinderella of pure mathematics', nonlinear differential and difference equations, a Linnean status, as poignantly and accurately described by George Temple[21]:

> "The group of problems which I propose to describe belong to that Cinderella of pure mathematics- the study of *Diophantine equations*. The closely guarded secret of this subject is that it has not yet attained the status and dignity of a science, but still enjoys the freedom and freshness of such pre-scientific study as natural history compared with botany. The student of *Diophantine equations* ... is still living at the stage where his main tasks are to collect specimens, to describe them with loving care, and to cultivate them for study under laboratory conditions. The work of classification and systematization has hardly begun. ....
>
> ... An inviting flora of rare equations and exotic problems lies before a botanical excursion into the *Diophantine* field."
>
> [**31**], p.233.

Why are they intractable? How will they relate to the more conventional analytical approaches via the behaviour of rational agents? Indeed, what kind of animals are they? I cannot, of course, go into the full details of these 'inviting flora of rare equations' but shall try to provide a glimpse into their 'closely guarded secrets'[22]

.

Definition 19. *A relation of the form*

$$D\left(a_1, a_2, ..., a_n, x_1, x_2, ..., x_m\right) = 0$$

[21] I have taken the liberty of substituting *Diophantine equations* for *differential equations* in the quoted paragraph.

[22] I follow the terminology in Matiyasevich's elegant book for the formal statements about Diophantine equations. (cf. [**19**])

where D is a polynomial with integer coefficients with respect to all the variables $a_1, a_2, ..., a_n, x_1, x_2, ..., x_m$ (also integer or rational valued), separated into ***parameters*** *$a_1, a_2, ..., a_n$ and* ***unknowns*** *$x_1, x_2, ..., x_m$, is called a* ***parametric Diophantine equation****.*

DEFINITION 20. *D in Definition 9 defines a set F of the parameters for which there are values of the unknowns such that:*

$$\langle a_1, a_2, ..., a_n \rangle \in F \iff \exists x_1, x_2, ..., x_m \left[D\left(a_1, a_2, ..., a_n, x_1, x_2, ..., x_m\right) = 0 \right]$$

Loosely speaking, the relations denoted in the above two definitions can be called *Diophantine representations.* Then sets, such as F, having a Diophantine representation, are called simply *Diophantine*. With this much terminology at hand, it is possible to state the fundamental problem of Diophantine equations as follows:

PROBLEM 1. *A set, say $\langle a_1, a_2, ..., a_n \rangle \in F$, is given. Determine if this set is Diophantine. If it is, find a Diophantine representation for it.*

Of course, the set F may be so structured as to possess equivalence classes of properties, P and relations, R.Then it is possible also to talk, analogously, about a *Diophantine representation of a Property P* or a *Diophantine representation of a Relation R.* For example, in the latter case we have:

$$R\left(a_1, a_2, ..., a_n\right) \iff \exists x_1, x_2, ..., x_m \left[D\left(a_1, a_2, ..., a_n, x_1, x_2, ..., x_m\right) = 0 \right]$$

Hence, given, say partially ordered preference relations, it is possible to ask whether it is Diophantine and, if so, search for a Diophantine representation for it. Next, how can we talk about the solvability of a Diophantine representation? This is where undecidability (and uncomputability) will enter this family of 'inviting flora of rare equations' - through a remarkable connection with recursion theory, summarized in the next Proposition:

PROPOSITION 12. *Given any parametric Diophantine equation, D, it is possible to construct a Turing Machine, M, such that M will eventually* ***Halt****, beginning with a representation of the parametric n-tuple, $\langle a_1, a_2, ..., a_n \rangle$,* ***iff*** *$D$ in Definition 9 is solvable for the unknowns, $x_1, x_2, ..., x_m$.*

But, then, given the famous result on the *Unsolvability of the Halting problem for Turing Machines*, we are forced to come to terms with the unsolvability of Diophantine equations[23]. Hence, the best we can do, as Political Arithmeticians and Accountants, and even as behavioural agents, however rational, so long as the constraints are Diophantine, is to act according to the gentle and humble precepts enunciated by George Temple: 'collect specimens, to describe them with loving care, and to cultivate them

[23]It must, of course, be remembered that all this is predicated upon an acceptance of the *Church-Turing Thesis.*

for study under laboratory conditions'. Clearly, anyone familiar with the work of Charles Sanders Peirce will also realise that this kind of natural historic study fits comfortably with that great man's advocacy of *retroduction*[24] in such disciplines. The tiresome dichotomy between induction and deduction, refreshingly banished by Peirce more than a century ago, may well get cremated in economics, once and forever, if we combine the methodology of the natural historian with the epistemology that is implied in retroduction.

The headlong rush with which economists have equipped themselves with a half-baked knowledge of mathematical traditions has led to an un-natural mathematical economics and a non-numerical economic theory. Whether this trend will reverse itself of its own volition is very doubtful. But discerning scholars of mathematical philosophy - including front-ranking mathematical theorists like Ulam - have seriously speculated, in the last few years, that the trend in mathematics itself may force a change in its methodology and epistemology. If mathematical traditions themselves incorporate the ambiguities of structures that are replete with undecidabilites in their bread-and-butter research, it will only be a matter of time before such habits will rub off on even the obdurate mathematical economist. Petty, our founding father, wanted only to 'express [himself] in number, weight or measure'. They need only to be linked together by means of parametric Diophantine equations - as Luca Pacioli knew when he devised that lasting contribution to mercantile practice: double-entry bookkeeping. To get our 'pluses' and 'minuses' ordered, we do not need anything more, once again, than parametric Diophantine equations. Through them we enter the weird and wonderful world of undecidabilities and because of that we will happily, in an economics for the future, return to the Linnean fold, to classify and systematise, particular intractable accounting schemes.

[24]Even knowledgeable scholars persist in referring to *retroduction* as *abduction*, in spite of Peirce explicitly stating: '....$\alpha\pi\alpha\gamma\omega\gamma\eta$ should be translated not by the word *abduction*, as the custom of the translators is, but rather by reduction or *retroduction*'. ([**22**], p.141; italics in the original)

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CHAPTER 6

Trans-Popperian Suggestions on Falsification and Induction

1. Preamble

> By these results [i.e., Hume's answers to what Popper called Hume's 'logical' and 'psychological' problems] Hume himself - one of the most rational minds ever - was turned into a sceptic and, at the same time, into a believer: a believer in an irrationalist epistemology.[**12**]

However,we persist in continuing to read this *believer in an irrationlist epistemology* , puzzle over his paradoxical thoughts, ruminate over their unfathomable implications and debate, almost endlessly, about *induction* as *Hume's Problem*, over two centuries after that great man's speculative thoughts were penned[1]. Should we be doing this, particularly mulling over *Hume's Problem*, almost three quarters of a century after one of the great philosophers of the 20th century claimed he had *solved* it? The opening lines of Objective Knowledge, [**12**], assert, with characteristic boldness and without any sense of what may be suspected as false modesty:

> I think I have solved a major philosophical problem: the problem of induction. (I must have reached the solution in 1927 or thereabouts). [**12**], p.1.

Almost half a century after he claimed to have 'solved' the problem of induction there was, in the same opening pages of the above book, a rueful reflection of the seeming failure of this 'solution' to have penetrated the philosophical discourse of the times:

> However, few philosophers would support the thesis that I have solved the problem of induction. Few philosophers have taken the trouble to study - or even criticize - my views on this problem, or have taken notice of the fact that I have done some work on it.[**12**], p.1

[1]Popper has noted he may have been the first to give the name *Hume's Problem* to 'the problem of induction'(after he had, in fact solved it): To my knowledge I was the first to call the problem of induction 'Hume's problem; though of course there may have been others. I did so in 'Ein Kriterium des empirischen Charakters theoretischer Systeme', *Erkenntnis*, 3, 1933, pp.426f., and in L.d.F., section 4, p.7 ...

It would seem possible that 'few philosophers would support the thesis that [Popper had] solved the problem of induction' because they did not think he had solved it. After all some of the philosophers who did not agree that he had solved the problem of induction were not lesser giants of twentieth century philosophy, particularly of the philosophy of science - Carnap, Quine, Putnam, Harré and, of course, Kuhn, Feyerabend, Laudan and a legion of other giants of equal stature.

In my admittedly erratic reading of Popper's massive and impressive writings I have never managed to unearth any admission that some, at least, of the many distinguished philosophers who did not agree that he had 'solved' the problem of induction may have been right. Formulating a famous problem, naming it famously and offering a supposedly famous solution to it are all, by any conceivable standard, arduous endeavours. Popper's irritation that the philosophers of his time did not pay attention to his solution or, worse, did not agree that he had solved it, is understandable - if he was an ordinary mortal. He, however, is supposed to be one of the giants of 20th century western philosophy who, again famously[2], propagated the *credo* that *'we can learn from our mistakes'*, [**10**], p.vii.[3], and argued passionately for *open societies*, [**9**].

I must admit that I detect something more than an irritation - indeed, an *intolerance* with his contemporaries, particularly of course with Carnap[4], that his formulation and solution of the problem of induction was not recognized universally and unconditionally. This is brought out most vividly in Quine's majestic description of Popper's contrived 'clash of titans' to bury Carnap's alternative vision of the problem of induction and its solution:

> Popper was counting on a confrontation of Titans. Carnap's latest work was his ponderous one on induction. The first volume had appeared and the second was in progress. Popper decried induction, and he meant to settle the matter. I sensed that he was deploying his henchman, Imre Lakatos and John Watkins, with military precision as the three of them undertook preliminary skirmishes. But the last scheduled session drew to an end without the anticipated culmination. Popper accordingly declared an additional session, next morning, for all who could see their way to staying. It was strictly Popper vs. Carnap, with an audience of twenty-odd in a seminar room. I was carried back to Carnap's confrontation of Lovejoy in Baltimore thirty years before. Again he met vehemence with the mild but ready answer, the same old

[2] I am repeating the word advisedly and consciously

[3] But, of course, we do also *'learn from our successes'* as John McCall wisely observed during the presentation of his paper at the conference.

[4] In a private conversation in Los Angeles in the early 90s, Spiro Latsis mentioned, during a discussion about the environment at LSE during his period there, that for Popper *'the enemy was Carnap'*. This, surely, reflects the intrusion of an unnecessary personal dimension - I nearly said 'subjective' - into a serious philosophical issue.

> cool, unruffled reason. It is my splendid last memory of Carnap.[**17**]p.337[5]

A similar story of almost passionate intolerance of disagreements with his visions, views and theories can be told for those other great concepts with which we even, indeed especially, as <u>economists</u>, associate Popper's name, the Popperian credo and a Popperian philosophy: *falsifiability*, the rational underpinnings of *the growth of scientific knowledge*, the impossibility of discovering a *method* (an *algorithm*) for the *the logic of scientific discovery*, just to name a few.

An example of this intolerance towards the possibility of falsifying his narrow and logic-based theory and thesis on falsifiability, which I believe illustrates his inability to apply his own precepts consistently, viewed by him to be a cardinal sin, is the way he warned readers not to accept a particular challenge posed by Alan Turing[6]:

> Turing [1950] said something like this: specify the way in which you believe that a man is superior to a computer and I shall build a computer which refutes your belief. Turing's challenge should not be taken up; for any sufficiently precise specification could be used in principle to programme a computer.[**13**],p.208

Why should we not take up 'Turing's challenge'?[7] Should we be afraid that the challenge might 'refute our beliefs'? Surely, the *raison dêtre* of a falsifiable *credo*, buttressed by a philosophy wedded to an untrammelled[8] 'openness', is to be challenged[9]and dethroned. Is this an intolerance or, perhaps, a subjective attachment to personal theories compounded by a fear of some sort? After all, Carnap was 'attacked' almost personally, as if his particular view of inductive probability could not be separated from Carnap's personality,

Above all, however, where I, coming from a Buddhist culture, a Hindu home and a Western education, buttressed also by an undergraduate training in Japan, find a narrowness of vision and a lack of a generosity of spirit, is in the lack of attention given

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[5]Among the 'titans' present at this contrived 'clash' were, in addition to Carnap: Tarski, Bernays, Church, Curry, Kreisel, Mostowski and Kalmar - all of them also 'titans' of recursion theory, of varying degrees.

[6]Of particular relevance in this paper, given my recursion theoretic approach to problems of induction, falsification and scientific discovery

[7]I had the pleasure of meeting Sir John Eccles, in Erice, Sicily, in August, 1987, when we both attended one of the *Ettore Majorana Summer Schools* organised by Pofessor Antonino Zichichi. I took a copy of his book with Popper ([**13**]) in which the above quote appears and showed it to him and asked whether it was not slightly uncharacteristic of the vehement and impatient proponent of falsifiability to make such a statement. He read it carefully, thought for a moment and acknowledged that it was puzzling but that he had not paid much attention to it!

[8]I am sure any number of acolytes of Popper, in the unlikely event they happen to glance at this paper, will take me to task for suggesting that the 'openness' was 'untrammelled'.

[9]As Popper himself explicitly and provocatively stated: '[I]f you can design some experimental test which you think might refute my assertion, I shall gladly, and to the best of my powers, help you refute it.'[**10**]

to alternative *epistemologies*, even if not *philosophies* [10]. In Buddhist epistemology, for example, there are clear precepts for inductive inference that eschew any reliance on an underlying probabilistic framework. Moreover, as McGinn has recently pointed out, in an extremely interesting essay [**7**], there is the necessity, in any Popperian falsification exercise, to rely on an inductive inference:

> But there is a worse problem for Popper's philosophy: he is committed to inductive verification himself. ... Consider, too, that falsifying experiments have to be repeatable so that other researches can duplicate the alleged finding. We have to be able to *infer* that if a *falsifying* result has been found in a given experiment it will be found in future experiments. ... [So] *falsification needs to be inductively justified* if it is to serve as a means of testing theories.
>
> It is generally so justified, of course, but this is not something that Popper can consistently incorporate into his conception of science. [**7**], p.48.(italics added)

In Buddhist epistemology, however, the coupling of any falsification exercise with inductive inference, is tackled in an extremely enlightened manner - enlightened in the sense of trying to inculcate a sense of humility for the human condition in the face of nature's possible intransigence,although there is not that sharp dichotomy between the human being and nature. Popper's seemingly encyclopedic knowledge exhibits no awareness of alternative epistemologies. His underpinnings are best described in Toulmin's brilliant characterization:

> All the way across the field, from logic and mathematics to the human sciences and the fine arts, the essential tasks of intellectual and artistic activity were redefined in static, structural, a-historical, non-representational, and wherever possible *mathematical* terms.
>
> Nowhere were the effects of this reformulation more far-reaching than in the philosophy of science. ... By the early 1920s it was an unquestioned presupposition for philosophers of science that the *intellectual content* of any truly scientific theory formed a timeless "propositional system," like that of which Russell and Whitehead had given a prototype in *Principia Mathematica*.[**19**], p.56; first set of italics added.

In this paper I try to tackle and suggest some trans-Popperian solutions and approaches to the vexed problems of induction, inductive inference and falsifiability. The point of view I take is that it is this predominance of redefining all human activity in 'mathematical terms' and forming a 'timeless propositional system' that has bedevilled Popperian epistemology. However, it is not that I disagree with this double-reliance;

[10]I accept Popper's adherence to a mathematical methodology. However, here, too, there is a narrowness of vision, to which I shall return in later parts of this paper

but it is that there are many ways of relying on 'mathematical terms' and even more ways of underpinning scientific theories on 'propositional terms' that are neither 'a-historical' nor 'timeless'.

Finally, to go back to my initial observation about Hume and our centuries old obsession with his writings, the point I wish to make is the following: would we, at the end of this century, still value the writings of Popper as those of one of the giants of 20th century philosophy and epistemology, or would he have been buried with other transient giants, who dominated transitorily? Would his status become that of a Herbert Spencer, a Larmarck, even a Lysenko or a Cyril Burt or would it be in that pantheon of the other two great contemporary Austrians with whom he shared the century and some of its fame[11]: Wittgenstein and Freud? Naturally, I do not know and I am not sure I want to know, for if he is fated to share the company and fate of the former, I may not have the courage to read his provocative and inspiring writings.

But, contrary to the other participants at this centennial to honour the great Man, I come not to praise him. I am aware, of course, that Popper, had he been alive, would have counter-punched with the ferocity that we have come to associate with him.

2. Introduction

> [T]the method of falsification presupposes no inductive inference, but only the tautological transformation of deductive logic *whose validity is not in dispute*.[**11**],p.42; italics added.

Paradoxically, neither of these assertions are, of course, considered true, as the 21st century dawns - although the cognoscenti were aware of their dubious validity long before even the twilight of the previous century set in.

Economic Methodology, explicitly and implicitly, has been deeply influenced by three of Popper's seminal ideas: falsifiability, the logic of scientific discovery and the twin issues of induction and inductive inference.[12] Of course, all three of the seminal ideas are interlinked and the unified recursion theoretic approach I am able to use, to tackle them analytically, substantiates that particular point. Underpinning them, in almost all their ramifications, is the ubiquitous spectre of *rationality* and its concomitants: rational behaviour, the rational scientist, the rational scientific enterprise and the rationality of the autonomous processes of nature. All these seem to have fallen on receptive ears, at various levels and practice, in the economic community.

Paradoxically, however, these three seminal Popperian conceptual contributions, indeed pioneering research programs, come in the form of negative precepts. Foremost of these negative precepts is, of course, that there is *no* such thing as a *logic* of scientific discovery to discover; that theories can only be refuted and held, at most,

[11] But, apparently, nothing else!

[12] The excellent collection of essays: The Popperian Legacy in Economics, edited by Neil De Marchi, [**3**], is a good place to get an organised guide to the pervasive influence of Popperian ideas in economics.

provisionally, waiting for them to be refuted; and, then, there was that insistence about the impossibility of inductive probability.

Behind these vehement negative precepts there was, implicitly, the insistence that the epistemologist was confronted by an environment that was lawful, about which theories could be conjectured, albeit provisionally. As pointed out by Harré in his surprisingly pungent 'Obituary' of Popper:

> ... Popper's methodology of conjecture and refutation, based upon the idea of of the rationality of rejecting hypotheses which have been shown at a particular time and place to be false, also depends upon an assumption of a form of the uniformity of nature. In his case, it is the negative assumption that the universe will not change in such a way as to make what was disconfirmed today true tomorrow. Popper's methodology of conjecture and refutation makes no headway in the testing of that proposition. His claim to have solved the problem of induction must now be rejected.[**5**]

It was also the point made by Glymour, in a more specific sense:

> Popper ... agreed with Plato that knowledge requires a kind of unalterability, but unlike Plato he did not think that the process of science obtains knowledge.

I shall not address specific issues of economic methodology from any particular Popperian point of view in this paper. Instead, I aim, hopefully, to provide less negative visions of two of these great Popperian themes and help disseminate a more positive attitude towards the rich possibilities of pursuing an inductive methodology in the search for laws of scientific discovery, buttressed by a dynamic, algorithmic, reinterpretation of the meaning of falsifiability. (Classical) recursion theory and applied recursion theory, in the form of algorithmic complexity theory, will be my conceptual and methodological tools in this adventure. Hence, I shall consider the message in this paper fully within the program of research I initiated, about 20 years ago, and coined the phrase 'Computable Economics' to describe it. If, therefore, there is any constructive contribution emanating from it, it will be towards the methodology of that research program. In that specific sense, then, it is squarely within the scope of the title of this volume: *Popper and Economic methodology: Contemporary Challenges*, with the emphasis, almost exclusively, on 'contemporary challenges'.

In his 1972 *Addendum* to the 1972 edition of *The Logic of Scientific Discovery*, [**11**], Popper was quite explicit about the *logical* basis of falsifiability[13]:

[13] I have often wondered why the German original 'Forschung' was translated as 'Scientific Discovery'! I am sure there must be a perfectly 'rational' Popperian explanation for the particular choice of words in English. Something like The Logic of Scientific Research or The Logic of Scientific Investigation would have been a more faithful translation of the title (and its contents). I shall, whenever I refer to this book, refer to it as LdF, even though it will be to [**11**]

> ... [T]he content or the testability (or the simplicity ...) of a theory may have *degrees*, which may thus be said to relativize the idea of falsifiability (whose logical basis remains the *modus tollens*.)[**11**], p.135; italics in original.

Let me refresh possible rusty memories of unlikely readers about *Modus (Tollendo) Tollens*:

In *Modus(Tollendo) Tollens*, by denying - i.e., *tollendo* - the consequent of an implication we deny - i.e., *tollens* - the antecedent. More formally:

$$\sim \mathbf{Q} \& (\mathbf{P} \Rightarrow \mathbf{Q}) \Rightarrow \sim \mathbf{P}$$

It is immediate that two dubious mathematical logical principles are implicitly invoked in any falsifiability exercise based on *Modus (Tollendo) Tollens*: *principium tertium non datur* or the law of the excluded middle and *proof by contradiction.* This means an adherence to non-constructive methods in all cases involving infinite alternatives. How experiments can be arranged and methods devised to test for falsifiability, even abstracting away from inductive inferential problems, in a non-constructive environment, escapes me. Indeed, how any method to test for falsifiability can be anything other than constructive, *in some sense*, is beyond my understanding.

It is this kind of reliance on traditional logic and a limited knowledge of the vast developments in mathematical logic in the 20th century that I find mysterious in a philosopher who seemed to be encyclopedic in his awareness of so much else. I find no evidence, in my perusal and attempted reading of as much as possible of Popper's voluminous writings, of any awareness, either, of the fact that mathematical logic had itself branched off, in the 20th century, into four or five sub-disciplines and, in any case, into: set theory, proof theory, recursion theory and model theory. This is the kind of reason why Glymour, for example, was scathing in his criticism of a class of philosophers in general, but of Popper, in particular:

> With only a little logical knowledge, philosophers in this period understood the verifiable and the refutable to have special logical forms, namely as existential and universal sentences respectively. There was, implicitly a positivist hierarchy .... Positivists such as Schlick confined science to and meaning to singular data and verifiable sentences; 'anti-positivists', notably Popper, confined science to the singular data and falsifiable sentences. In both cases, what could be known or discovered consisted of the singular data and verifiable sentences, although there is a hint of something else in Popper's view.[**4**], p.268.

On the other hand, if one feels it is necessary to retain fidelity to Popper's reliance on *Modus (Tollendo) Tollens* as an underpinning for falsifiability exercises[14], then it seems to me that the best way to do so would be via formalizations using recursion

[14] Even although it is easy to show that it is neither necessary nor sufficient

theory. Classical logical principles retain their validity but methods are given algorithmic content which makes them implementable devices in experimental design. This is, therefore, the mathematical framework I shall invoke in this paper, in spite of the fact that I believe that a thorough constructive approach is epistemologically superior for numerical reasons.

The rest of the paper is structured as follows. In the next section I try to extract recursion theoretic precepts from Popper's own writings for their eventual formalizations in §4. I try to exploit the subtle differences between recursive and recursively enumerable sets to give a broader, more dynamic, definition of falsifiability exercises. Had space permitted, I would have expanded this subtle distinction to include recursive separability, too; but that will have to be attempted in a different exercise.

In §4, I try to suggest that what I have, in other contexts and writings called the 'modern theory of induction' is a perfectly adequate framework to justify and solve *Hume's problem*. This framework is based on (classical) recursion theory and, hence, is an appropriate mathematical structure to encapsulate, formally, the heuristic discussions in §3. Solving the induction problem recursion theoretically also, almost as a by-product, solves the problems that have bedevilled Popper's formalization of falsifiability. But only a sketch is given,although there is enough for any serious reader to complete the mathematical arguments.

In §5, the concluding section, I speculate, on the basis of the results and discussions in the paper, of alternative visions and vistas and on trying to retain a sense of the humble and the steadfast, in the wake of increasing specialisations, intolerances and dogmas in all fields of human endeavour.

3. The Backdrop for Trans-Popperian Suggestions

> Popper's mistake here is no small isolated failing. What Popper consistently fails to see is the *practice is primary*: ideas are not just an end in themselves (although they are *partly* an end in themselves), nor is the selection of ideas to 'criticize' just an end in itself....
>
> ...
>
> The method of testing ideas in practice and relying on the ones that prove successful (*for that is what 'induction' is*) is not unjustified. That is an *empirical* statement. The method does not have a 'justification' - if by a justification is meant a proof from eternal and formal principles that justifies reliance on the method. But then nothing does - *not even, in my opinion, pure mathematics and formal logic.*[**16**], pp.268-9, first and third set of italics added.

Popper does not seem to have paid much attention to the great achievements in recursion theory, proof theory or model theory to substantiate his case for empirical methodology or for falsification. As to why he did not seek recourse to recursion theory, in the case of inductive inference or the logic of scientific discovery, could it, perhaps, be because such a framework may have cast doubts on his negative critique against these thorny concepts? One can only speculate and I do speculate simply because these three branches of modern mathematical logic provide literally the proverbial 'tailor-made' formalisms for empirically implementable mathematical structures for falsifiability, the logic of scientific discovery and for induction in all its manifestations. I shall discuss recursion theoretic formalisms for falsifiability in this section, but for the logic of scientific discovery, due to space limitations, I must refer the vigilant enthusiast to my writings on Simon.

There are two characteristically prescient Popperian observations very early on in *Ldf*:

> [I] am going to propose ... that the *empirical method* shall be characterized as a method that excludes precisely those ways of evading falsification which ... are logically possible. According to my proposal, what characterizes the empirical method is its manner of exposing to falsification, in every conceivable way, the system to be tested. Its aim is not to save the lives of untenable systems but, on the contrary, to select the one which is by comparison the fittest, by exposing them all to the fiercest struggle for survival.
>
> ... The root of [the problem of the validity of natural laws] is the apparent contradiction between what may be called 'the fundamental thesis of empiricism' - the thesis that experience alone can decide upon the truth or falsity of scientific statements - and Hume's realization of the inadmissibility of inductive arguments. This contradiction arises only if it is assumed that all emprical scientific statements must be 'conclusively decidable', i.e., that verification and their falsification must both in principle be possible. If we renounce this requirement and admit as empirical also statements which are decidable in one sense only - unilaterally decidable and, more especially, falsifiable - and which may be tested by systematic attempts to falsify them, the contradiction disappears: the method of falsification presupposes no inductive inference, but only the tautological transformations of deductive logic whose validity is not in dispute.[**11**][15]

Firstly, in what other way, if not by means of an *algorithm*, can we understand the processes implied by implementing an *empirical method*?[16].

Secondly, Popper endeavours to drive a wedge between verifiability and falsifiability in terms of decidability - but, we know, based on *Modus (Tollendo) Tollens*. There

[15]The last part of this quotation formed the lead quote for the previous section

[16]I am simply paraphrasing Nozick's analogous rhetorical query: 'In what other way, if not simulation by a Turing machine, can we understand the process of making free choices? By making them, perhaps?'[**8**], p.303

is, however, a much simpler way to drive this wedge *and* preserve the algorithmic character of implementable *empirical methods* Moreover, it will not be necessary to make the incorrect claim that 'the method of falsification presupposes no inductive inference'[17].

Thirdly, there is the need to be precise about what is meant by a *natural law* and a *scientific statement*, before even discussing the meaning of their *truth* or *falsity*.

I shall take it that Popper means by a *natural law* something as paradigmatic as, for example, Newton's Laws of Motion or, at a slightly more sophisticated level, say, the General Theory of Relativity. As an economist, I have never felt that that we have the equivalent of a *natural law*, in the above senses, in economic theory. Perhaps, at a much lower level sophistication, we may, as economists, invoke one of the popular theories of growth, say the Solow's Growth Model.

Such *natural laws*, for example Newton's Laws of Motion or at a much much more down-to earth level, Solow's Growth Model, are framed, when mathematized, as formal *dynamical systems*. Of such systems we ask, or test, whether, when they are appropriately initialised, they enter the definable *basin of attraction* of, say, a limit point, a limit cycle, a strange attractor or, perhaps, get trapped in the boundaries that separate a limit cycle and a strange attractor. In the case of the Solow Growth Model, theory predicts that the dynamical system, for all economically meaningful initial conditions enters the basin of attraction of a limit point. The theory and its law can, in principle be 'verified'.

However, it is for very few dynamical systems that we can answer the above type of question unambiguously, i.e., 'verifiably'. This is the key point made by Popper in his almost lifelong quest for a kind of scepticism about theories and the natural laws inherent in them. It is just that such a scepticism comes naturally to those accustomed to formalizing in terms of proof theory, model theory and recursion theory - i.e., for those working in the domain of the constructive, non-standard or computable numbers.

Moreover, a *natural law* in any of the above senses is, at least from Popper's point of view, which I think is the commonsense vision, is a *scientific statement*, as indeed referred to as such by Popper in the above characterization. What, next, does it mean to formalize the notion of a *scientific statement*? Clearly, in the form of something like a *well formed formula* in some formal, mathematical, logic. Obviously, what is, then, meant by 'deciding upon the truth or falsity of scientific statements', must also be a commonsense interpretation; i.e., the 'truth' or 'falsity' of the implications of the scientific statement which encapsulates the natural law. I shall assume, therefore, that the set of meaningful scientific statements form an enumerable infinity.

Fourthly, Popper claims that the distinction between verifiability and falsifiability depends on allowing for a certain kind of one-way decidability. More precisely, verifiability is characterized by a 'strong' sense of decidability and falsifiability by a

[17] See above, the observation by Colin McGinn; however, as I proceed, I expect to be able to show that McGinn's doubts 'this [i.e., inductive inference] is not something that Popper can consistently incorporate into his conception of science' is unwarranted. On the other hand I am not at all sure Popper would approve of my solution to this problem!

somewhat 'weaker' concept of decidability. In Popper's case, of course, the underpinning to formalize the distinction between a 'strong' and a 'weak' sense is *Modus (Tollendo) Tollens*. I seek a more dynamic version of the possibility of such a distinction, simply because many, if not most, meaningful *natural laws* are framed dynamically or as dynamical systems. By 'dynamically', I mean, the implication of the theory, when formulated as a *natural law*, and subject to experimental procedures, generates a sequence of outcomes, usually numerical[18], which has to be sequentially monitored and tested.

Fifth, there is a need to be absolutely precise about what Popper means, formally, by 'exposing to falsification, *in every conceivable way*, the system to be tested'. How many conceivable ways would there be, given an 'experimental method', to 'expose to falsification the system to be tested'? Suppose, as in conventional economic theory, the domain of definitions is the real number system. Then, in principle, an uncountable infinity of 'conceivable ways' would have to be devised for 'the system to be tested'. This is meaningless in any empirical system.

The best that can be attempted, in principle, is to enumerate a countable infinity of *empirical methods* and for the case, for example, of *natural laws* formalized as dynamical systems, to quantify the notion of *every conceivable way* by varying the initial conditions in a precisely formalized countably infinite, enumerable, mode - i.e., *algorithmically* - but not necessarily subject to the *Church-Turing Thesis*. In other words, *algorithmically* could also be encapsulated within the broader canvas of *constructive mathematics* (or also more narrowly than even recursion theory)[19].

Finally, there is the need to be precise (and sensible) about what Popper could have meant by 'select the one which is by comparison the fittest, by exposing them all to the fiercest struggle for survival'. It is here, contrary to enlightened Popperian critics, that I find that *inductive inference* enters the Popperian world with almost a vengeance. How does one formalize the selection criterion that is suggested by Popper? What could be meant by 'fittest'? Surely not some facile neo-Darwinian formalism via, say, genetic algorithms in the conventional sense.

This is where Glymour and Harré, for example, presumably locate Popper's adherence to the Platonic assumption of the 'unalterability of nature'. For, if not, we cannot, of course, 'expose them *all*' to any kind of test, let alone the more specific test of 'the fiercest struggle for survival'. By the time we come, say, to *scientific statement*, say, #10948732765923, and the *natural law* implied by it, and say *empirical method* #371952867 for testing it, there is no guarantee that our theoretical world picture would not have changed - from the Ptolemic world vision to the Copernican vision. This would mean some of the *scientific statements* had become meaningless and others, not in the original enumerated list, become feasible candidates for testing.

[18]If not explicitly numerical then, in principle, codifable number theoretically using one of the well-known procedures emanating from 'Gödel Numbering'.

[19]I shall, however, work within the framework of classical recursion theory and, hence, subject to the *Church-Turing Thesis*.

I shall circumvent these issues by suggesting that we interpret Popper's criterion of the 'fittest' by the analogous criterion, in some precise sense formalizable notion, of 'most likely' or 'most plausible' by invoking yet another historical nemesis of Popper: *Ockham*.

In concluding this section, it may be useful to record, at least for the sake of completion, one of Popper's later, more formal, and rather harshly critical statements on The Impossiblity of Inductive Probability. His joint paper with Miller, ([**14**]), begins and ends in almost apocalyptic tones:

> Proofs of the impossibility of induction have been falling 'deadborn from the Press' ever since the first of them (in David Hume's *Treatise of Human Nature* appeared in 1739. One of us (K.P) has been producing them for more than 50 years.
>
> ... This result is completely devastating to the inductive interpretation of the calculus of probability. All probabilistic support is purely deductive: that part of a hypothesis that is not deductively entailed by the evidence is always strongly countersupported by the evidence - the more strongly the more the evidence asserts. This is completely general; it holds for every hypothesis h; and it holds for every evidence e, whether it supports h, is independent of h, or countersupports h.
>
> There is such a thing as probabilistic support; there might even be such a thing as inductive support (though we hardly think so). But the calculus of probability reveals that probabilistic support cannot be inductive support.[**14**], pp. 687-8.

Mercifully for Popperian theories of falsifiability (and for theories of the growth of scientific discovery), this particular 'chronicle of a death foretold' (*pace* Gabriel Garcia Marquez) is as chimerical as many before it[20]. The recurring puzzle is the following: why was it that Popper seemed to have been unaware of developments in applied recursion theory - i.e., algorithmic complexity theory - that gave a new lease of life to induction and inductive inference by returning to one of Popper's earliest preoccupations: that with the attempts he made to formalize the Richard von Mises notion of the *kollektiv*, the *frequency theory of probability* and a formalization of the notion or *randomness* without basing it on probability.

Perhaps his *psychological* commitment to an anti-inductivist stance overcame his scientific predispositions? Even the Gods are fallible, at least in the Hindu mythologies in which I was brought up!

[20]As Wise and Landsberg, in one of the responses to [**14**] put it, mildly and wisely:'As this [i.e., the impossibility of inductive probability] would be a remarkable achievement, it is no criticism of these authors that we raise this question again. In our view the answer is a clear "No".

4. Formalizations of Trans-Popperian Suggestions

> Popper and the positivists agreed that there could not, in any case, be an *algorithm* for carrying out scientific inquiry. Why not? ... For Popper - who quite confused a psychological question with a mathematical issue - it sufficed to quote Einstein to disprove the possibility of a discovery algorithm; for Carnap it sufficed to quote Popper quoting Einstein. [**4**], pp.268-9.

I shall begin this section with a formal proposition which provides the starting point for *selecting*, for any eventual falsifiability exercise, of a *natural law* which may emerge from some *scientific statement*:

PROPOSITION 13. *An event with the highest probability of occurring is also that which has the simplest description*

The kind of analysis that leads to a formal demonstration of this proposition is as follows. Consider a standard version of the Bayes rule subject to a denumerable infinity of hypotheses, H_i, about the occurrences of events, E, with Probability, P:

$$P(H_i \mid E) = \frac{P(EH_i)P(H_i)}{\Sigma_i P(E \mid H_i)P(H_i)} \tag{4.1}$$

In the above relation, apart from absolutely standard, textbook interpretations of all the variables and notations, the only explicit novelty is the assumption of a denumerable infinity of hypotheses. Thus, in a standard inverse probability or Bayesian exercise, E, the class of 'observed' events and $P(H_i)$ are given. What I would call the standard *induction problem* is to find the 'most probable' hypotheses, H_i, that would 'most probably' lead to the observed event of relevance. There is no way Popper, if he is to formulate his falsifiability exercise, along the lines he suggested in *Ldf*, can avoid at least this aspect of the *induction problem.*

To get the Popperian perspective I need, let me first translate (1) into an equivalent 'optimisation' problem (Popper's 'fittest'!) by simply rewriting it as:

$$-log[P(H_i)E] = -logP(E \mid H_i) - logP(H_i) + logP(E) \tag{4.2}$$

In (2), the last term on the r.h.s is a short-hand expression for the denominator in (1) which, in turn, is the normalising factor in any Bayesian exercise. Now, finding the 'most probable hypothesis' becomes equivalent to determining that H_i with respect to (w.r.t) which (2) is <u>minimised</u>. But, in (2), $logP(E)$ is invariant w.r.t H_i and, hence, the problem is tominimise (w.r.t H_i):

$$-logP(E \mid H_i) - logP(H_i) \tag{4.3}$$

However, it is clear that a problem of indeterminacy or circularity would remain in any such formalizaion so long as we do not have a principle of the basis of which P - the so-called *prior* - cannot be assigned *universally*; i.e., independent of any problem cast in the inverse probability mode.

Now let me backtrack and view the problem from a point of view that would lead to the recasting of the induction problem as one in which *Ockham's Razor* becomes a kind of 'dual' to the Bayes rule. The 'inductive enterprise', even in any relevant Popperian sense, is supposed to interpret a class of observations, events, data, etc., in terms of a denumerable infinity of hypotheses in such a way that a general *scientific statement* is formalized as a *natural law* from which, by *deductive* processes, the outcomes with which one began are generated. This is why it is insufficient, inadequate and even disingenious for Popper to claim that 'the method of falsification presupposes no inductive inference'.

As far as the requirements of the logic of the inductive method is concerned, I shall assume that we need only formalise, at most, a denumerable infinity of outcomes in an observation space. This particular assumption may well be the only one that goes against a Popperian vision of the empirical world.[21] As for the number of hypotheses, there is no incongruence with Popper's visions and assumptions in assuming a denumerable infinity as their upper limit (as argued for iin the previous section).

Thus the space of computable numbers is sufficient for this formalisation exercise. Suppose, now, that every element in the outcome space and every potential hypothesis - both being denumerably infinte - is associated with a positive integer, perhaps ordered lexicographically. More precisely and technically speaking, every outcome and hypothesis is, normally, framed as a logical proposition (the former especially when formalised for falsifiability purposes), particularly by Popper with his absolute and almost fanatical faith in classical logic.

Every such proposition can be assigned one of the computable numbers - those that form the domain of recursion theory. Such numbers can be processed by an 'ideal computer', the *Turing Machine*. The 'ideal computer', however, accepts input in 'machine language', i.e., in binary code. Construct, therefore, the list of binary codes for the denumerable elements of the elements of the outcome space and the hypotheses. In other words, every hypothesis (i.e., *scientific statement*) - which, in principle, underlies a potential general law that is the aim of an eventual falsification exercise - has a computable number associated with it and the number is represented in *bits*. It has, therefore, an unambiguous *quantitative measure* associated with it. A similar association can be constructed for the elements of the outcome space. Then, the basic result in what I have inother context called the *modern theory of induction* is derived by operating the following rule:

<u>Rule of Induction</u>

- The 'best theory' is that which *minimises* the sum of:
  (1) The length, in *bits*, of the number theoretic representation of the denumerable infinity of hypotheses;

[21] There are analytical ways to circumvent this assumption and allow for the possibility of a continuum of observations, but I shall reserve that analysis for another exercise.

(2) The length, in *bits* of the elements of the space of outcomes, which are also, by assumption, denumerably infinite.[22]

The conceptual justification for this prescription is something like the following. If the elements of the observation space (E in Bayes's rule) have any *patterns* or *regularities*, then they can be encapsulated as *scientific statements* implying *natural laws*, on the basis of some *hypothesis.* The *best law* - i.e., Popper's 'fittest system' - is that which can extract and summarise the *maximum* amount of regularities or patterns in E and represent them *most concisely.* The idea of the '*maximum amount of regularities*' and their representation '*most concisely* captures the workings of *Ockham's Razor* in any inductive exercise. If two hypotheses can encapsulate the patterns or regularities in the data equally well, in some sense, then the above prescription is 'choose the more concise one'.

The final link in tis inductive saga is a formula for the *universal prior* in Bayes's rule in terms of recursion theoretic 'regularities':

PROPOSITION 14. *There exists a probability measure $m(\cdot)$ that is universal in the sense of being invariant except for an inessential additive constant such that:*

$$\log_2 m(\cdot) \approx K(\cdot) \tag{4.4}$$

In Proposition 2, $K(\cdot)$ is the Kolmogorov-Chaitin *algorithmic complexity* of the *best theory* - once again, Popper's *fittest system* - generated in the operation of the 'rule of induction'. All of the operations and formalisms that generate $K(\cdot)$ are known; i.e., there are no probabilistic elements in any step that leads to a value for $K(\cdot)$. The measure $m(\cdot)$ can be substituted for the $P(\cdot)$ in Bayes's rule, for any inverse probability problem.[23]

The above is a trans-Popperian suggestion on how not to avoid the inductive needs of a falsification exercise. What of the falsification exercise itself? The trans-Popperian suggestion for this formalism proceeds as follows. First, three definitions.

DEFINITION 21. *Recursive Set*
$S \subseteq \aleph$ is recursive iff $\exists$ *a Turing Machine for* deciding *wether any given member of* $\aleph$ *belongs to S.*

DEFINITION 22. *Decidable Set*
A set S is decidable *if, for any given property $P(s)$, $\forall s \in S$, $\exists$ a Turing Machine such that it halts* iff *$P(s)$ is valid.*

[22] I hope the careful reader will realise that the minmization is not over a denumerably infinite sum!

[23] It is seen that induction and inductive processes are intrinsically 'complex' phenomena in a precise sense. The complexity indicator is also a measure of the *randomness* of the phenomenon from which the underlying probability structure can be derived (or *inferred*). There is, thus, a kind of 'duality' between Bayes's rule and Ockham's Razor and, depending on the problem, the scientist can opt for the logic of the one or the other.

Definition 23. <u>*Recursively Enumerable Sets*</u>
$S \subseteq \aleph$ *is* recursively enumerable *(R.E)* iff *it is either empty or the range of a Turing Machine (i.e., the range of a partial recursive function).*

Thus, for any decidable set, we know there will be *effective* experimental methods - i.e., algorithms - to characterize any member of the set. It is clear from the above definitions that a *recursive set is decidable.* This is the universe of the verifiable.

Falsifiability and verifiability are methods, i.e., procedures to decide the truth value of propositions. Popper claims, in view of his allegiance to classical logic and *Modus)Tollendo) Tollens* that the only viable procedure in a scientific enterprise is one which is capable of falsifying a law. This translates into the following: a set has to exhibit *undecidabilities.* This means it is not sufficient to work with an outcome space that is confined to recursive sets. A subtle modification of the definition of a recursive set to allow for an open-endedness, suggested as a requirement by Popper, will achieve it.

The intuitive idea is the following. Suppose the inferred *scientific statement* and its implied *natural law* are formalized as the hypothesis that is to be experimentally tested. The idea is that some implication of the hypothesis is to be verified or falsified. If the set of outcomes of the implication forms a recursive set, then we know that it is decidable and, hence, verifiable. Suppose, however, the set of outcomes of the implications form a *recursively enumerable set.* Then, whether or not any particular $P(s)$ is valid is undecidable in the following precise sense. Given an arbitrary predicted outcome of the experimental procedure of the law, say $n \in \aleph$, we test whether it is the range of a Turing Machine. If it is, it can, eventually, be decided. If it is not, we will never know. The next output of the experimental setup, after say output # 32786591 may well be the confirming instance. But there will be an open-endedness which means such laws can, at best, be accepted provisionally if they meet other criteria of adequacy.

There is a precise sense in which the above scheme generalises and meets objections to Popper's more classical definition of falsifiability. Even although recursion theory is based on classical logic, the exclusive reliance on *Modus (Tollendo) Tollens* and singular data and falsifiable sentences are removed to be special cases.To put it in a different way, as Glymour did, the verifiable relied on the existential form for a testable sentence (i.e., $\exists x$ s.t $S(x)$); and the falsifiable relied on the universal quantifier (i.e., $\forall x$, s.t $S(x)$).

In terms of Gödel's results, my suggestions can be stated in yet another, equivalent, form. The G̈del scheme shows how to transform any given proposition into one about polynomials. Then, there exist arithmetical equations, linking two polynomials representing propositions, *preceded by some finite sequence of existential and universal quantifiers* that are effectively undecidable. This is the sense in which there is no longer any reliance on singular data or singular sentences.

5. Transcending Dogmas and Intolerances

> [I]n retrospect, a concern with systematizing inductive logic has been the oldest concern of empiricist philosophers from Bacon on. No one can yet predict the outcome of this speculative scientific venture. But it is amply clear, whether this particular venture succeeds or fails, that the *toleration* of philosophical and scientific speculation brings rich rewards and its suppression leads to sterility.[**15**],p.304; italics added.

von Mises and his valiant attempts to define *place selection* rules received considerable attention in *LdF*, cf. [**11**], Ch.VIII, §50, ff. It is, therefore, somewhat surprising that the evolution and remarkable development of that von Mises tradition at the hands of Kolmogorov and a legion of recursion theorists and philosophers[24] seemed to have by-passed the eagle eyed Popper (but cf., [**11**],Appendix vi). It is particularly surprising in view of the fact that success in resolving the difficulties with defining *place selection rules*, admittedly on the basis of the *Church-Turing Thesis* and what I have called, in citeve, the *Kolmogorov-Chaitin-Martin-Lf̈ Thesis*, resulted in the *modern theory of induction.* My trans-Popperian suggestion, particularly the first part of the previous section, owes much to this development.

There is a further paradox in this saga. Popper defined, in his pursuance of a resolution of the problem of defining *place selection rules*, the concept of 'freedom from after effect' for a sequence of outcomes, say:

$$x_1, x_2, x_3, \ldots \tag{5.1}$$

Where the outcomes take on binary values, o and 1. For such a sequence, Arthur Copeland,citeco, some years earlier than Popper[25], but also inspired by the von Mises framework for a frequency theory of probability, defined the *admissible numbers* as follows:
If, for any choice of integers,

$$r_1, r_2, \ldots, r_k, s \tag{5.2}$$

where,

$$1 \leq r_1 < r_2 < \ldots < r_k \leq s \tag{5.3}$$

$$\lim_{n\to\infty} \frac{1}{n} \sum_{m=0}^{n-1} x_{r_1+m_s} x_{r_2+m_s} \ldots + x_{r_k+m_s} = p^k \tag{5.4}$$

where $p \in \Re$ and $0 \leq p \leq 1$. Martin-Löf, whose excellent exposition I follow here,[**6**] calls it the 'success probability of the sequence'. Now, Copeland, [**2**], proves that for an arbitrary $p, 0 < p < 1$, the set of admissible numbers has the power of the continuum.

[24] Some of them, like Alonzo Church and Hilary Putnam, wore both hats

[25] There is hardly a single serious reference to Copeland's work in the mighty Popperian writings!

In addition, if p is a *computable real*, Copeland's proof seems to provide an effective construction of an admissible number with success probability p.

Then, since Popper's definition of a sequence *free from aftereffect* has been shown to be equivalent to Copeland's definition of the admissible numbers, the problem of handling the possibility of the outcome space having the power of the continuum, as required by many physical laws and almost all economic theories, may seem to be solved, without sacrificing computable underpinnings.

However, such sequences as defined by Popper and Copeland are defined by a mathematical law, such as given above, and von Mises objected that they cannot serve as 'idealizations of sequences obtained by actual coin tossing', i.e., as truly random, i.e., impossibility of a gambling system which guarantees success. Popper himself stated that his own aims in treading that early frequentist path was for different purposes ([**11**], p.361) and, furthermore:

> I have meanwhile found that the 'measure-theoretical approach' to probability is preferable to the frequency interpretation ..., both for mathematical and philosophical reasons[26].

I feel that this preference, due also, naturally, to his adherence to his own, flawed, 'propensity interpretation of probability', blinded him to the possibilities of an enlightened view of the problem of induction, which would also have salvaged falsifiability, even in a broader context than that tied to *Modus (Tollendo) Tollens* and the *universal quantifier*. Perhaps it was also due to the seeming intransigence towards any concept of induction and inductive procedures.

The point I am trying to make is that Popper had all the concepts and the advantages of the correct starting points to tackle falsifiability and inductive inference in one fell swoop. Somehow, he avoided that path and, as a result, hie fertile concepts and precepts have suffered interminable criticisms. I suppose all I have tried to do in the previous two sections is to return to Popperian themes, with potential Popperian concepts and tools to salvage the ruins!

I have left aside the third of the triptych that forms one set of the Popperian scientific world vision: the logic of scientific discovery. For reasons of space I must refer any interested reader, that perennially 'elusive creature', to two of my related writings, [**21**], [**22**]. I can, however, add that in this case I find Popper's nihilism quite unwarranted and his criticism or non-criticism of attempts to forge, for example, a (computational) theory of scientific discovery as intolerant and misguided as his attitude towards Carnap and the induction problem.

One last technical point has to be faced. In the previous section I mentioned that one assumption - that of a countably infinite observation space - may well be running against the spirit of a Popperian vision of the natural world and its laws. How, then, can a recursion theoretic resolution of the problem be attempted. The issue is something like the following (cf, for example, [**1**]). Many are now aware of ways of constructing simple dynamical systems with complex dynamics. For example, simple

[26] Almost the exact opposite path was taken by Kolmogorov

'laws' generate extraordinary complex dynamics resulting in sets that are familiar even to children palying around with computers: the *Mandelbrot set*, *the Julia set*, and so on. In these particular cases the domain of definition happens to include the complex plain and deciding whether a particular initial configuration of the 'simple law' which generates the Mandelbrot set retains its dynamics within the set will require considerations of an outcome space that has the power of the continuum. Is there a computable way to make such question decidable or, at least, make decidability questions meaningful?

I think there are two ways to proceed. One is to adopt the point of view advanced by Smale ([**1**]) and his co-workers and define computation over the reals. The other is to remain within computable analysis and find ways to finesse the structure of the computable reals. I prefer the latter alternative but any incursion into that domain, even at an elementary level, is far beyond the scope envisaged for this paper. I should just like to record my belief that nothing in the framework I have suggested in §will need to be modified, except that some seemingly sophisticated mathematics may have to be invoked. As I mentioned at the outset, I shall have to avoid going into a discussion of issues like *recursively inseparable sets* so that this paper remains manageable.

Popper's was a lifelong voice against intellectual intolerances and dogmas of any sort. However, he does not seem to have been a great practitioner of his own precepts. Putnam ([**16**]) perceptively noted:

> Failure to see the primacy of practice leads Popper to the idea of a sharp 'demarcation' between science, on the one hand, and political, philosophical, and ethical ideas, on the other. *This 'demarcation' is pernicious in my view*; fundamentally, it corresponds to Popper's separation of theory from practice, and his related separation of the critical tendency in science from the explanatory tendency in science. Finally, the failure to see the primacy of practice leads Popper to some rather reactionary political conclusions. Marxists believe that there are laws of society; that these laws can be known; and that men can and should act on this knowledge. It is not my intention to argue that this marxist view is correct; but surely any view that rules this out *a priori* is reactionary. Yet this is precisely what Popper does - and in the name of an *anti-a priori* philosophy of knowledge![**16**], p. 269; first set of italics, added.

The pernicious influence of 'demarcationists' has resulted in intolerances and dogmas permeating all the affairs of society where the role of the narrow expert has been extolled beyond limits envisaged by the sages and the saints. The walls, whether it be the ones in Beijing or Berlin, Jerusalem or in the Ghettos of Warsaw, reflect the demarcationist's attitude in political ideology and practice. In the sciences, whole theories have been rejected on unenlightened attitudes that smack of the demarcationist: the rejection, for example, of Dirac's delta Function, the controversy over hidden-variables in quantum mechanics and the fate meted out to that impeccably erudite scientist of integrity, David Bohm. In economics, the continuing dominance of a narrow application of a narrow and irrelevant part of mathematics to formalize economic entities and derive momentous policy conclusions; and it is not too many years since Lysenko and Cyril Burt ruled wholly different political societies with equally dogmatic, demarcatioinst, visions.

I conclude with Edward Said's poignant call, in the fourth of his *BBC Reith Lectures*, for the intellectual to become, once again an amateur, thus reversing the trend towards increasing specialisation, underpinned by the demarcationists philosophies and epistemologies:

> An amateur is what today the intellectual ought to be, someone who considers that to be a thinking and concerned member of a society one is entitled to raise moral issues at the heart of even the most technical and professional activity as it involve one's country, its power, its mode of interacting with its citizens as well as other societies. In addition, the intellectual's spirit as an amateur can enter and transform the merely professional routine most of us go through, into something much more lively and radical; instead of doing what one is supposed to do one can ask why one does it, who benefits from it, how can it reconnect with a personal project and original thought.[**18**].

The absence of the 'amateur' in Popper was, I think, the cause of much of the intolerance he displayed - in spite of advocating criticism and openness. These advocacies were not graced by the soft touch of the amateur's genuinely open mind.

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