



UNIVERSITA' DEGLI STUDI DI TRENTO - DIPARTIMENTO DI ECONOMIA

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**VARIATIONS ON THE THEME OF  
*CONNING IN MATHEMATICAL  
ECONOMICS***

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Discussion Paper No. 3, 2007

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# Variations on the Theme of *Conning*\* in *Mathematical Economics*†

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March 2, 2007

## Abstract

The mathematization of economics is almost exclusively in terms of the mathematics of real analysis which, in turn, is founded on set theory (and the axiom of choice) and orthodox mathematical logic. In this paper I try to point out that this kind of mathematization is replete with economic infelicities. The attempt to extract these infelicities is in terms of three main examples: dynamics, policy and rational expectations and learning. The focus is on the role and reliance on standard fixed point theorems in orthodox mathematical economics.

*JEL Classifications:* C02, C60, D50, E 61

*Keywords:* General Equilibrium Theory, Mathematical Economics, Theory of Policy, Rational Expectations Equilibrium

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\*I am invoking three meanings of this word, simultaneously. Firstly, in the sense of one of the meanings given in **The Shorter Oxford English Dictionary on Historical Principles** as an ‘*argument or arguer against*’ (orthodox mathematical economics); secondly, in the sense of ‘*to swindle, trick*’ (given in **Longman’s Concise English Dictionary**); thirdly, in the sense of a ‘*confidence trick*’, as used in US English slang. The second and third senses are, of course, closely related. I suspect it is the third sense that is invoked in the justly celebrated articles by Leamer ([26]) and Roth ([40]) on econometrics and experimental economics, respectively.

†Of course, no one is implicated in any of the errors and omissions that remain in this final version of a paper that has been in embryo for many years. However, I cannot help suspecting that my critical friends, Tom Boylan, Bob Clower, Duncan Foley, Steve Kinsella, Francesco Luna, John McCall, Srinivas Raghavendran and Stefano Zambelli may have tried, without much success, to ameliorate the infelicities by gentle suggestions. Alas, pure stubbornness is the only reason for my mule-headed refusal, sometimes, to take into account their sensible suggestions, particularly with regard to tone and nuance.

# 1 Preamble

"It has been correctly said that mathematical economics is flying high these days. So *I come, not to praise mathematics, but rather to slightly debunk its use in economics*. I do so out of tenderness for the subject, since I firmly believe in the virtues of understatement and lack of pretension."

Paul Samuelson ([41], p.58; italics added)

Edward Leamer's eloquent critique ([26]) of the dissonance between the practice of econometric research and its public dissemination via articles in peer-reviewed journals brought to the forefront the dilemmas faced by an econometrician who was also an experimenter. Alvin Roth, a little over a decade after Leamer, took up a similar issue confronting the experimental economist, as that subject itself came of age ([40]) - warning of the pitfalls inherent in the divergence between 'the way we report experiments ... and the way an experiment is actually conducted'. Leamer, however, concluded that 'the atmosphere of econometric discourse would be sweetened', if serious attention was paid to two words: *whimsey* and *fragility*:

"In order to draw inferences from data as described by econometric texts, *it is necessary to make whimsical assumptions*. The professional audience consequently and properly withholds belief until an inference is shown to be adequately insensitive to the choice of assumptions. The haphazard way we individually and collectively study the fragility of inferences leaves most of us unconvinced that any inference is believable. If we are to make effective use of our scarce data resource, it is therefore important that we study fragility in a much more systematic way. If it turns out that almost all inferences from economic data are fragile, I suppose we shall have to revert to our old methods .... ."

[26], p.43; italics added

In this paper I aim to point out that the dilemmas discussed by Leamer and Roth for econometrics and experimental economics are alive and well also in mathematical economics. I shall, in analogy with Leamer, discuss this dilemma paying close attention to the two words *whimsey* and *fragility* but, implicitly in the case of the latter, also in terms of the 'fragility of deduction'<sup>1</sup> in addition to

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<sup>1</sup>*One aspect* this 'fragility of deduction' was perceptively noted by Samuelson (op.cit, pp. 59-60):

"[T]here is for all of us a psychological problem of making correct deductions. That is why pencils have erasers and electronic calculators have bells and gongs.

I suppose this is what Alfred Marshall must have had in mind when he followed John Stuart Mill in speaking of the dangers involved in *long* chains of logical reasoning. Marshall treated such chains as if their truth content was subject to radioactive decay and leakage .... . Obviously, in making such a statement, Marshall was describing a property of that biological biped or

the ‘fragility of inferences’. In fact, the way I shall discuss and demonstrate the role of *whimsey* and *fragility* in the *conning* that is mathematical economics, it will become evident that the ‘fragility of inferences’ is a by-product of the whimsical assumptions of mathematical economics.

It may not be out of place, given the nature of this paper, to point out that Sir Michael Atiyeh’s Fields Institute Lecture in Toronto, given in June 2000, was titled, *Mathematics in the 20th Century* ([2]). Significantly, he began with an important *caveat emptor*<sup>2</sup>(p.1):

"I will say nothing .. about the great events in the area between logic and computing associated with the names of people like Hilbert, Gödel and Turing."

I shall, however, try to dissect *conning* in mathematical economics primarily on the basis of ‘the great events in the area between logic and computation’, in particular mathematical logic and theories of computation, thus encompassing both recursion theory and constructive mathematics in the latter and some non-classical logics in the former.

I shall illustrate the role and mode of *conning* in mathematical economics by discussing three famous examples in economics: the role, functions and claims made for the *Walrasian Auctioneer*<sup>3</sup>; the various formulations and conclusions about the scope for formal policy, particularly in macroeconomics; and the formalization of the notion of a *Rational Expectations Equilibrium (REE)* using topological fixed-point theorems and then, separately, the devising of learning mechanisms to determine it.

The paper is, therefore, structured as follows. In the next section I discuss, formally, the meaning and (extravagant) claims on economic dynamics by the mathematical economists and show the nature of cons involved in the various formal exercises. In §3, a similar exercise is attempted for the formalized theory of economic policy. The kind of conning implicit and explicit in the fix-point approach to REE is the subject matter of §4. In the concluding section, §5, speculative hints are discussed on how a *Conless Mathematical Economics* might be devised, paying close attention to the interaction between the ontology of economic entities and their quantitative realisations and verifications<sup>4</sup>. I include a substantial discussion of an alternative tradition in game theory, unfortunately

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computing machine called *homo sapiens*; for he certainly could not be describing a property of logical implication."

My own focus will be on the ‘fragility of deduction’, whether of long or short ‘chains of reasoning’, due to the background implicit assumptions in almost every step of any such chains of reasoning and the nature of the deductive process itself. Thus, for example, in assuming a *continuum* of agents and then reasoning as if a particular agent in the continuum can be *identified* requires the explicit assumption of the *axiom of choice*; hence, that particular identification is *algorithmically non-effective*.

<sup>2</sup>To be read, instead of ‘let the buyer be aware’, as ‘let the reader be aware’!

<sup>3</sup>Alias the *Walrasian Demon*, for the purposes of this paper (see below for the justification for the alias).

<sup>4</sup>I choose to use this word quite deliberately and not the more Popperian - *whimsical* - word, *falsification*, that is uncritically adopted, particularly in econometric discourse.

quite unfamiliar to most economists - even those who may well be fairly competent in mathematics - with the purpose of debunking the *con* that is implied in uncritical invoking of and reliance on the *axiom of choice*.

In a sense the main theme of the paper is the attempt to disabuse economic theorists in general, and mathematical economists in particular, of uncritical reliance on, and unwarranted acceptance, of certain kinds of mathematical formalizations and uncritical or ignorant acceptance of controversial or meaningless mathematical axioms.. Such mathematical formalizations, I argue in terms of the main three examples, leads to the cons that are replete in a kind of mathematical economics that relies on them for formalizations, theorizing and inferences. Reliance on them leads to whimsical assumptions, entirely determined and dictated by the mathematics and not by the ontology of economic entities, institutions and behaviour. As a consequence the inferences are inherently fragile or even senseless, since they require impossible approximations from uncomputable entities and undecidable propositions.

The main - but not the sole one - culprit is easy to identify. It is best identified in terms of a programmatic assertion - not a proof or an inference from empirical or experimental observations - by the doyen of 20th century mathematical economics, Gerard Debreu. In his Frisch Memorial Lecture , delivered at MIT in August, 1985, he asserts the following<sup>5</sup>:

" "Thus von Neumann's lemma, reformulated in 1941 as Kakutani's fixed point theorem, was an accident within an accidental paper<sup>6</sup>. But in a global historical view, *the perfect fit between the mathematical concept of a fixed point and the social science concept of an equilibrium* stands out. .... In this view, fixed point theorems were slated for the prominent part they played in game theory and in the theory of general economic equilibrium after John Nash's one-page note of 1950."

Debreu ([14], p. 1262; italics added)

He may well be right about the 'mathematical concept of a fixed-point', but his unawareness of the existence of different kinds of 'fixed point theorems' - especially recursion theoretic fixed point theorems - shunted the formalization of economic theory in a direction that made it prone to *conning*, particularly about dynamics, in general, and processes, such as learning, in particular.

As a result of this monomaniacal reliance and belief in the validity of the assertion that there is a 'perfect fit between [particular kinds of] fixed point

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<sup>5</sup>This is a theme, with minor variations, Debreu has emphasized in various writings since the early 80s (cf., for example [12] and [15]). The other mathematical concept to which he makes reference in a similar fashion is the separating hyperplane theorem (or the Hahn-Banach theorem). My strictures against the particular kind of fix-point theorems to which he and his followers appeal and invoke for use in economic theory apply equally forcefully and rigorously also the use, invoking and application of such 'duality' theorems.

<sup>6</sup>It is not clear that von Neumann's growth paper (presented, first, in 1932, published in German in 1938 and in English in 1945-6, [66]) was, in fact, 'an accident within an accidental paper' (cf. [20]).

theorems and the social science concept of equilibrium', all kinds of problems in economics have been forced into a formalization that can exploit the use of such theorems. A whole subject has been conned into believing this 'perfect fit' on no empirical, experimental or even historical grounds or necessity. It was not an economist but an eminent applied mathematician who had the courage, hindsight and foresight to point out that:

"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.**"

[51], p.290; bold emphasis added.

Smale could have added that there is a 'simpler kind of mathematics' that combines the notion of fixed points and algorithms - hence dynamics - in one fell swoop. The implications are that we have not only been *conned* into formalizing unnaturally; but have also been *conned* into using a more complex mathematics<sup>7</sup>.

## 2 Exposing the Dynamics *Con* in Economic Theory

We have got accustomed to referring to computable and constructive methods of algorithmic analysis in the *digital mode* in economic theory. However, at the

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<sup>7</sup>The kind of uncritical acceptance of a *conning* exercise like that which is here pointed out by Smale leads to further irrelevant cons like the following (Suzumura, [56], p.67):

"Some years ago, Professor Uzawa established a remarkable theorem to the effect that the Walras' existence theorem and the Brouwer fixed point theorem are equivalent. The importance of this equivalence theorem lies in the fact that *it accounts for the intrinsic necessity of the fixed point type of topological considerations in the analysis of general economic equilibrium.*"

outset, our neoclassical masters, when they referred to the market as an equation solver, were thinking in terms of *analogue devices*<sup>8</sup>. It is worth pointing out then, at the outset, that the explicit computing tradition in economics can be said to have begun with Walras’s fertile idea of *tâtonnement* and Pareto’s famous invoking of the analogy of the market’s dynamics as a solver of an equilibrium system of equations. The key distinction between the two great neo-classical pioneers of general equilibrium theory is that Walras envisaged *tâtonnement* to be a *gedankenexperiment* to organise his thoughts about equilibrium solutions to a multi-market system of equations<sup>9</sup>; Pareto, on the other hand, was explicit that market dynamics was acting as a solver for the equilibrium of a system of simultaneous equations:

“It may be mentioned here that this [analytic] determination has by no means the purpose to arrive at a numerical calculation of prices. Let us make the most favourable assumption for such a calculation, let us assume that we have triumphed over all the difficulties of finding the data of the problem and that we know the ophelimités of all the different commodities for each individual, and all the conditions of production of all the commodities, etc. This is already an absurd hypothesis to make. Yet it is not sufficient to make the solution of the problem possible. We have seen that in the case of 100 persons and 700 commodities there will be 70,699 conditions (actually a great number of circumstances which we have so far neglected will further increase that number); we shall therefore have to solve a system of 70,699 equations. This exceeds practically the power of algebraic analysis, and this is even more true if one contemplates the fabulous number of equations which one obtains for a population of forty millions and several thousand commodities.

In this case the rôles would be changed: it would not be mathematics which would assist political economy, but political economy would assist mathematics. In other words, if one really could know all these equations, *the only means to solve them which is available to human powers is to observe the practical solution given by the market.*”

[34], pp. 233-4; italics added

The pioneers, therefore, were trying to supplement their formalizations of the supply – demand equilibrium nexus with a mechanism for solving for equilibrium. The separation between proving existence of an equilibrium and finding methods to solve for it was not part of the tradition of 19th (or earlier) century

<sup>8</sup>I have discussed the possible role of analogue computing in economic dynamics in a recent paper and shall not further touch on that topic in this paper ([61]).

<sup>9</sup>He did not, as generations of critical mathematical economists have alleged, simply count equations and variables and satisfy himself about the existence of solutions in a facile way; he was, after all, a 19th century scientist, for whom solving an equation still meant devising methods to find the solution. The somnambulance of existence proofs without accompanying constructions had to wait for the 20th century. Progress and its paradoxes have many facets.



mathematics (as mentioned earlier). However a natural formalization of the problem of ‘supply equals demand’, without *conning* us into believing that the real numbers are the adequate, ideal or the default domain, is as a *Diophantine decision problem*<sup>10</sup>. This suggests that the market *mechanism*, in seeking and, perhaps, finding equilibrium prices, is *solving* the formally *unsolvable*! This would imply hypotheses or thought experiments on plausible economic processes suggesting that they might, in fact, be formal algorithms, not necessarily subject to the strictures of the Church-Turing Thesis. Any such thought experiment on feasible economic processes not subject to the strictures of the Church-Turing Thesis raises the question of the formal meaning of *mechanism* and whether or not the economic system is to be viewed as a *non-mechanism*. I shall use the term relying on the classic definition given by Gandy ([18]) and a series of exceptionally suggestive questions and tentative answers broached by Kreisel on the question of *mechanism* (for example, [21], [22]), Clearly the market system functions in ways that violate these definitions and characterizations, and hence any claims about constructing mechanisms or algorithms to depict its smooth and successful functioning, as made by orthodox economic theorists is an unadulterated *con*.

In his characteristically prescient fashion, Steve Smale, although not an economist, hit upon the central problem confronting the dynamic economic theorist by stating, as the 8th of 18 ‘Mathematical Problems’ for the 21st Century - in ‘Hilbertian Mode’ - the following<sup>11</sup>:

*"Extend the mathematical model of general equilibrium theory to include price adjustments"*

[53], p.10; italics in original

The analogy of the dynamics of a market mechanism in search of an equilibrium of supply and demand<sup>12</sup> with the paradigmatic actions and functions of a computer<sup>13</sup> has a long and distinguished tradition in economics. The changing mathematical underpinnings and formalisms, with the evolution not only

<sup>10</sup>In a sense I was inspired to move in this direction as a result of Smale’s efforts ‘show how the existence proof for equilibria can be based on Sard’s theorem and calculus foundations .. [using] equations such as "supply equals demand" .. rather than fixed points’, [52]. I have dealt with this approach in greater detail in [62].

<sup>11</sup>The prelude to the actual statement of the ‘8th Problem’ reads as follows:

"The following problem is not one of pure mathematics, but lies on the interface of economics and mathematics. It has been solved only in quite limited situations."

[53], p.10

<sup>12</sup>There is an implicit teleological element in stating ‘the dynamics of a market mechanism *in search* of an equilibrium of supply and demand’. From Adam Smith’s famous observation that the economic agent is ‘..led by an invisible hand to promote an end which was no part of his intention’ right through all the modern classics - particularly those inspired by Hayek - the teleological element has been kept in a penumbra, so to speak. I hope the reader will not try to place the teleological element in the forefront of my mild distortion of a noble tradition.

<sup>13</sup>During the first hundred years of the invoking of this analogy, the idea of a computer, in the economists who referred to it, was confined to its *analog* incarnation. Since then the analogy has exclusively (and often explicitly) referred to the *digital* computer. The emphasis

of the sophistication of mathematical economics but also of the development of mathematics, has brought the question of price adjustments of a market *mechanism* within the formal ambit of both recursion theory and constructive analysis. It is reasonably well known that the hypotheses of economic theory imply non-recursive outcomes even if the input data is computable. Against the backdrop provided by this theory, and assuming as usually done in economics that economic activity takes place in continuous time and market decisions are, in general, asynchronous, it is easy to show, experimentally, how Turing Machine constructions can be devised to show the generation of non-computable economic data.

The tricky question of how such uncomputable data can be used for inference and in which way they become inputs again into the dynamic economic system is one important aspect of the conning exercise in this part of economic theory. How can one infer anything in a quantitative mode from uncomputable data? By definition uncomputable data cannot even be represented in any meaningful finite mode.

In a perceptive review of the important papers by Pour-El and Richards ([36]), Kreisel ([22], p.900) observed:

"The [papers by Pour-El and Richards] add to the long lists of operations  $\mu$  in analysis with some recursive 'input'  $I$  for which no output in  $\mu(I)$  is recursive. ... Familiar examples are provided by (i) Brouwer's fixed point theorem in dimension  $>1$  .... where  $I$  ranges over (i) continuous maps of the unit circle into itself ... and where  $\mu(I)$  is the set of (i) fixed points.... ."

Contrast this with the fundamental theorem of Computable General Economic Equilibrium *Theory*:

**Theorem 1** *The Walrasian Equilibrium Existence Theorem (WEET) is equivalent to the Brouwer Fixed Point theorem (BFPT)*<sup>14</sup>.

on the mathematics of computation and mathematical logic in my efforts to expose *conning* in mathematical economics is partly due to this latter analogy. If the reader keeps in mind the following perceptive observation, then the main thrust of the arguments in this paper will be clear:

"The badly named *real number system* is one of the triumphs of the human mind. It underlies the calculus and higher analysis to such a degree that we may forget how impossible it is to deal with real numbers in the real world of finite computers. But, however much the real number system simplifies analysis, practical computing must do without it."

Forsyth ([17], p.932; italics in the original)

<sup>14</sup>For the benefit of readers unfamiliar with either or one of these celebrated theorems, here are two concise statements. Define the unit simplex as:

$$S_n^+ \equiv \{p \mid p \in \mathbb{R}_{n+1}^+ \& \|p\| = 1\} \tag{1}$$

**Theorem 2** *Brouwer Fixed Point Theorem*

*Let  $f : S_n^+ \rightarrow S_n^+$ , where  $f$  is continuous. Then there is a  $p^* \in S_n^+$  s.t  $p^* = f(p^*)$*

Now, mathematical equivalence between two propositions entails not just that each implies the other; but it also means that the ‘objects’ defined by each of the propositions are, mathematically identical. In other words, in the world of mathematical objects, the two objects defined by the two propositions are, ostensibly, simply the same items with two different names. Kreisel’s perceptive point, therefore, means that the Walrasian economic equilibrium is *non-recursive*. This means whatever economic process is devised, observed or inferred to locate or reach the equilibrium must, at some point in its path - perhaps just at the ‘final’ step - *transform a recursive input into a non-recursive output*. How can a realistic or meaningful mechanism be devised, at least as a thought experiment, to perform this transformation? Even a die-hard orthodox mathematical economist or economic theorist must admit that only some form of serious conning can achieve this - i.e., a fictitious mechanism which, even if postulated to possess ideal properties can achieve the transformation only by conning.

I have demonstrated, in [60], how the hypotheses underlying WEET, together with the naive *tâtonnement* dynamics of:

$$\frac{dp}{dt} = z(p) \tag{2}$$

can be shown to be consistent with the following theorem in Pour-El and Richards (op.cit, p. 61):

**Theorem 4** *There exists an ordinary differential equation with initial condition*

$$\varphi'(t) = F(t, \varphi(t)), \quad \varphi(0) = 0 \tag{3}$$

*such that  $F(x, y)$  is computable on the rectangle  $\{0 \leq x \leq 1, -1 \leq y \leq 1\}$ , but no solution of the differential equation is computable on any interval  $[0, \delta]$ ,  $\delta > 0$ .*

Using the recursive construction deftly used by Pour-El and Richards it can be shown how a mechanism can be envisaged for *tâtonnement* dynamics to generate the *equilibrium uncomputable solutions*. The best that an applied, empirical or experimental economist can do under these circumstances is to assume disequilibria, since approximating uncomputable equilibrium solutions is a meaningless activity. The whole exercise shows the fundamental dissonance between reckless mathematical theory and impossible empirical and experimental inferences.

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**Theorem 3** *Walrasian Equilibrium Existence Theorem:*

Let  $z : S_n^+ \rightarrow \mathbb{R}_{n+1}^+$  s.t:

(1).  $z(p)$  is continuous  $\forall p \in S$

(2).  $p \cdot z(p) = 0, \forall p \in S$ ; Then:

$\exists p^* \in S_n^+$  s.t  $z(p) \leq 0$

with  $p_i^* = 0$  for  $i$  s.t  $z_i(p) < 0$ .

To put the nature of the task facing the *Walrasian Demon*<sup>15</sup> in complete perspective let me also state the problem in terms of mappings (discrete time). The naively equivalent *mapping* for the dynamics of *tâtonnement*, as given in textbooks for example, is:

$$p_{t+1} = \frac{p_t + \Theta(p_t)}{[p_t + \Theta(p_t)]e} \quad (4)$$

Where  $\Theta(P_t)$ : mapping depending on the excess demand function,  $z(p)$ ;

$e$ : the appropriately dimensioned normalizing column vector;

The economic significance and mathematical purpose of this mapping is encapsulated in Scarf's lucid description of what exactly is accomplished by this mapping:

“The particular mapping is a modification of the fundamental price adjustment mechanism in which *prices are revised* in proportion to excess demand – the discrepancy between demand and supply. If the mapping *is iterated*, we obtain a sequence of price vectors, each one responsive to the excess demand evaluated at the previous price vector. While economic intuition might suggest that this sequence of price vectors converges to an equilibrium price vector, this need not be the case. Unless some restrictive assumptions are placed on the excess demand functions, the price sequence may oscillate and approach no limit at all. On the other hand, the fixed point implied by Brouwer's theorem does indeed serve as an equilibrium price vector. [This] illustrates quite well *the role that Brouwer's theorem has played in providing existence proofs*, rather than a constructive and computationally oriented procedure, *for obtaining an equilibrium price vector*.

[43], p.30; italics added

Note two standard infelicities in this otherwise impeccably orthodox observation: ‘*prices are revised*’, but *who* by?; secondly, ‘*the mapping is iterated*’ by *what mechanism* and *who* keeps account of the process? This is where the omniscience and omnipotency of the *Walrasian Demon* is implicitly invoked in the analysis of market dynamics. Omniscience, omnipotent or omni whatever notwithstanding, the basic task of the *Walrasian Demon* is to find a way of processing recursive inputs to produce (at least one) non-recursive output. This means somehow, somewhere, the *Walrasian Demon* will have to violate one or the other of Gandy's ([18]) defining criteria for *mechanisms*. Let me suggest

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<sup>15</sup>The ‘cognoscenti’ would have realised that I am using the term *Walrasian Demon* in analogy with the term *Maxwell's Demon*. Of course it is the *Walrasian Auctioneer* that I am re-naming the *Walrasian Demon*. Some may even know that Axel Leijonhufvud, when he coined the term *Walrasian Auctioneer*, did so in analogy with his (incomplete) understanding of the scope and functions of *Maxwell's Demon*, as gleaned from his reading of the *Mr Tompkins books* of George Gamov. The tortuous history of false analogies add to confusion and conning, as evidenced by some totally absurd remarks by Robert Axtell regarding the formal computational capabilities of the *Walrasian Auctioneer* (cf. [5]).

another of the ways - in addition to the one suggested above via the Pour-El/Richards theorem - the *Walrasian Demon* might devise a strategy to locate the non-recursive equilibrium.

For concreteness and simplicity I shall only consider the scalar case. Generalising it to a vector equilibrium is conceptually immediate although technically much more complicated. Let the non-recursive Walrasian economic equilibrium be  $\vartheta$ . Consider:

$$\Psi(p) = (p + \vartheta) \pmod{1} \text{ for } \Psi : [0, 1) \rightarrow [0, 1) \quad (5)$$

$\Psi$  is, therefore, given by:

$$\Psi(p) = \begin{cases} p + \vartheta, & 0 \leq p < 1 - \vartheta \\ p + \vartheta - 1, & 1 - \vartheta \leq p < 1 \end{cases} \quad (6)$$

To generate a binary sequence using the techniques of symbolic dynamics, define:

$$\Xi(p) = 0 \text{ for } x \in [0, 1 - \vartheta) \quad (7)$$

$$\Xi(p) = 1 \text{ for } x \in [1 - \vartheta, 1) \quad (8)$$

Now, apply  $Q$  to  $\Psi$ , initialising it on 0, and generate the binary sequence  $\{\pi_n\}$ , i.e.,

$$\pi_n = \Xi(\Psi^n(0)), \quad n \in \mathbb{Z}_+ \quad (9)$$

It is easy to show that the long-term average of the sequence  $\{\pi_n\}$  is  $\vartheta$  which, by construction, is the Lebesgue measure of that part of the interval that maps to 1 under  $\Xi$ ; i.e.,

$$\text{if } N_v \equiv \text{number of 1's in } \{\pi_n : 1 \leq n \leq v\} \quad (10)$$

$$\text{then } \lim_{v \rightarrow \infty} v^{-1} N_v = \vartheta \quad (11)$$

Thus, the best that the *Walrasian Demon* can generate is the non-recursive equilibrium value as a long run equilibrium outcome<sup>16</sup>.

But do we need this extra-territorial being, the *Walrasian Demon*, to perform this task? Does it possess any special knowledge or skills that ordinary mortals do not (thinking, again, analogously with the special characteristic of being a tiny, molecular, sized being the *Maxwell Demon* was, to perform the task of a gatekeeper to disorderly molecules)? Seemingly not. Therefore, we can actually dispense with any and all assumption of such a being and assume, hereafter, that any rational economic agent can perform the same task, given the necessary mathematical and computing skills. This is precisely what is assumed in any

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<sup>16</sup>But we in economics are only painfully aware of the great Keynesian aphorism: *In the long run we are all dead!*

mathematical economic framework where the assumption of the representative agent is fundamental. So, there is no point in exorcising the *Walrasian Demon* since it is embodied in the person of the omniscient representative agent, where the conning is supremely dominant via the embodiment of attributes that go *beyond mechanism* (in the Gandian<sup>17</sup> sense).

### 3 Dissecting *Conning* in the Mathematical Theory of Economic Policy

I shall assume that ‘elementary characterizable<sup>18</sup> attractors’ are the standard *limit points*, *limit cycles* and ‘*strange*’ (i.e., ‘*chaotic*’) *attractors*. All known dynamical systems in economic theory belong to one of these attractors. In particular, macroeconomic growth and cycle theories (including ‘growth cycle’ theories), can be shown to lead to one of the ‘elementary attractors’. Then, given the observable trajectories of a dynamical system, say computed using simple Poincarè maps or the like, an ‘elementary characterizable attractor’ is one that can be associated with a Finite Automaton<sup>19</sup>. Thus, limit points, limit cycles and strange attractors are *effectively characterizable* in a *computably trivial sense*. This means that every dynamical system encapsulating or representing any kind of dynamics in economic theory, particularly in macrodynamics, is computably trivial. I need not emphasise the nature of the conning exercise perpetrated by the purveyors of dynamics in economics, if this notion of ‘trivial’ is to be taken seriously.

Only dynamical systems capable of ‘*computation universality*’ are non-trivial in a computable sense. However, dynamical systems capable of computation universality have to be associated with Turing Machines. Such dynamical systems, by a formal process of elimination can be shown to be those that are poised delicately at the boundaries of the elementary characterizable dynamical systems. Constructing them is as delicate a task as constructing a dynamical system to generate equilibrium uncomputable solutions.

Thus, trajectories that are generated by dynamical systems poised on the boundaries of the basins of attractions of simple attractors may possess *undecidable* properties due to the ubiquity of the *Halting problem for Turing Machines*, the emergence of *Busy Beavers* (i.e., uncomputabilities), etc. Any theory of policy, i.e., any rule - fixed or discretionary - that is a function of the values of the dynamics of an economy formalized as a dynamical system capable of computation universality, will share these exotic properties.

I shall assume, simply for the sake of the discussion in this paper, an abstract

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<sup>17</sup>I hope the readers do not think this is a misspelling of ‘*Gandhian*’!!

<sup>18</sup>By ‘characterizable’ I shall understand ‘effective characterization of defining basins of attraction’, using ‘effective’ in the strict sense of recursion theory and ‘basin of attraction’ in the sense in which it is defined in formal dynamical systems theory (but see below, too).

<sup>19</sup>The analogy here is like that between the Chomsky hierarchy of formal languages and abstract computing machines. Wolfram, in [67], developed these ideas in the direction that I am trying to exploit here.

model of a ‘complex economy’, or of an ‘economy capable of complex behaviour’, to be a dynamical system capable of *computation universality*. By implication, then, the converse - i.e., a ‘simple economy’ - is one whose dynamics is formalizable as a Finite Automaton. This means that we have been *conned* by the purveyors of economic dynamics in the mathematical mode into accepting the formalization of complex economic dynamics by simple attractors! Is any act of *conning* more treacherous than this? Perhaps the two mentioned above are candidates?

I shall now assume familiarity with the formal definition of a dynamical system (cf. for example, the obvious and accessible classic, [19] or the more modern, [7]), the necessary associated concepts from dynamical systems theory and all the necessary notions from classical computability theory (for which the reader can, with profit and enjoyment, go to a classic like [39] or, at the frontiers, to [9]). Just for ease of reference the bare bones of relevant definitions for dynamical systems are given below in the usual telegraphic form<sup>20</sup>. An intuitive understanding of the definition of a ‘basin of attraction’ is probably sufficient for a complete comprehension of the main result - provided there is reasonable familiarity with the definition and properties of Turing Machines (or *partial recursive functions* or equivalent formalisms encapsulated by the Church-Turing Thesis).

**Definition 5** *The Initial Value Problem (IVP) for an Ordinary Differential Equation (ODE) and Flows. Consider a differential equation:*

$$\dot{x} = f(x) \tag{12}$$

where  $x$  is an unknown function of  $t \in I$  (say,  $t$ : time and  $I$  an open interval of the real line) and  $f$  is a given function of  $x$ . Then, a function  $x$  is a solution of (12) on the open interval  $I$  if:

$$\dot{x}(t) = f(x(t)), \forall t \in I \tag{13}$$

The initial value problem (ivp) for (12) is, then, stated as:

$$\dot{x} = f(x), \quad x(t_0) = x_0 \tag{14}$$

and a solution  $x(t)$  for (14) is referred to as a solution through  $x_0$  at  $t_0$ . Denote  $x(t)$  and  $x_0$ , respectively, as:

$$\varphi(t, x_0) \equiv x(t), \text{ and } \varphi(0, x_0) \equiv x_0 \tag{15}$$

where  $\varphi(t, x_0)$  is called the **flow** of  $\dot{x} = f(x)$ .

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<sup>20</sup>In the definition of a dynamical system given below I am not striving to present the most general version. The basic aim is to lead to an intuitive understanding of the definition of a basin of attraction so that the main theorem is made reasonably transparent. Moreover, the definition given below is for scalar ODEs, easily generalizable to the vector case.

**Definition 6 Dynamical System**

If  $f$  is a  $C^1$  function (i.e., the set of all differentiable functions with continuous first derivatives), then the **flow**  $\varphi(t, x_0), \forall t$ , induces a **map** of  $U \subset \mathbb{R}$  into itself, called a  $C^1$  **dynamical system on  $\mathbb{R}$** :

$$x_0 \longmapsto \varphi(t, x_0) \tag{16}$$

if it satisfies the following (one-parameter group) properties:

1.  $\varphi(0, x_0) = x_0$
2.  $\varphi(t + s, x_0) = \varphi(t, \varphi(s, x_0)), \forall t \ \& \ s$ , whenever both the l.h and r.h side maps are defined;
3.  $\forall t, \varphi(t, x_0)$  is a  $C^1$  map with a  $C^1$  inverse given by:  $\varphi(-t, x_0)$ ;

**Remark 7** A geometric way to think of the connection between a **flow** and the induced **dynamical system** is to say that the flow of an **ODE** gives rise to a dynamical system on  $\mathbb{R}$ .

**Remark 8** It is important to remember that the **map** of  $U \subset \mathbb{R}$  into itself may **not** be defined on all of  $\mathbb{R}$ . In this context, it might be useful to recall the distinction between partial recursive functions and total functions in classical recursion theory.

**Definition 9 Invariant set**

A set (usually compact)  $S \subset U$  is **invariant** under the flow  $\varphi(., .)$  whenever  $\forall t \in \mathbb{R}, \varphi(., .) \subset S$ .

**Definition 10 Attracting set**

A closed invariant set  $A \subset U$  is referred to as the **attracting set** of the **flow**  $\varphi(t, x)$  if  $\exists$  some neighbourhood  $V$  of  $A$ , s.t  $\forall x \in V \ \& \ \forall t \geq 0, \varphi(t, x) \in V$  and:

$$\varphi(t, x) \rightarrow A \text{ as } t \rightarrow \infty \tag{17}$$

**Remark 11** It is important to remember that in dynamical systems theory contexts the attracting sets are considered the **observable** states of the dynamical system and its flow.

**Definition 12** The basin of attraction of the attracting set  $A$  of a flow, denoted, say, by  $\Theta_A$ , is defined to be the following set:

$$\Theta_A = \cup_{t \leq 0} \varphi_t(V) \tag{18}$$

where:  $\varphi_t(.)$  denotes the flow  $\varphi(., .), \forall t$ .



**Remark 13** *Intuitively, the basin of attraction of a flow is the **set of initial conditions** that eventually leads to its attracting set - i.e., to its limit set (limit points, limit cycles, strange attractors, etc). Anyone familiar with the definition of a Turing Machine and the famous Halting problem for such machines would immediately recognise the connection with the definition of basin of attraction and suspect that my main result is obvious<sup>21</sup>.*

On the policy side, my formal assumption is that by ‘policy’ is meant ‘rules’ and my obvious working hypothesis - almost a thesis, if not an axiom - is the following:

**Claim 14** *Every rule is reducible to a recursive rule<sup>22</sup>*

**Remark 15** *This claim and the results below are valid whether by ‘rule’ is meant an element from a set of preassigned rules (i.e., the notion of policy as a fixed ‘rule’ in the ‘rules vs. discretion’ dichotomy) or a rule as a (partial recursive or total) function of the current state of the dynamics of a complex economy (discretionary policy)<sup>23</sup>.*

**Remark 16** *If anyone can suggest a rule which cannot be reduced to a recursive rule, it can only be due to an appeal to a non-algorithmic principle like an undecidable disjunction (which are routinely invoked in mathematical economics), magic, ESP or something similar.*

**Definition 17** ***Dynamical Systems capable of Computation Universality:***

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

**Proposition 18** *Dynamical systems characterizable in terms of limit points, limit cycles or ‘chaotic’ attractors, called ‘elementary attractors’, are not capable of universal computation.*

**Proposition 19** *Only dynamical systems whose basins of attraction are poised on the boundaries of elementary attractors are capable of universal computation.*

**Theorem 20** *There is no effective procedure to decide whether a given observable trajectory is in the basin of attraction of a dynamical system capable of computation universality*

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<sup>21</sup>In the same sense in which the Walrasian Equilibrium Existence theorem is obvious for anyone familiar with the Brouwer (or similar) fixed point theorem(s). The finesse, however, was to formalise the Walrasian economy topologically, in the first place. A similar finesse is required here.

<sup>22</sup>Firstly, ‘recursive’ is meant to be interpreted in its ‘recursion theoretic’ sense; secondly, this claim is, in fact, a restatement of the Church-Turing Thesis (cf. [6], p.34).

<sup>23</sup>It may be useful to keep in mind the following caveat introduced in one of the famous papers on these matters by Kydland and Prescott ([24], p.169):

"[W]e emphasize that the choice is from a [fixed] set of fiscal policy rules."

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

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

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

Therefore, by *Rice's theorem*, there is no effective procedure to decide whether any given arbitrary observed trajectory is in the basin of attraction of such recursively enumerable but not recursive basin of attraction. ■

Given this result, it is clear that *an effective theory of policy is impossible in a complex economy*. Obviously, if it is effectively undecidable to determine whether an observable trajectory lies in the basin of attraction of a dynamical system capable of computation universality, it is also impossible to devise a policy - i.e., a recursive rule - as a function of the defining coordinates of such an observed or observable trajectory. Just for the record I shall state it as a formal proposition:

**Proposition 21** *An effective theory of policy is impossible for a 'complex' economy*

**Remark 22** *The 'impossibility' must be understood in the context of effectiveness and that it does not mean specific policies cannot be devised for individual complex economies. This is similar to the fact that non-existence of general purpose algorithms for solving arbitrary Diophantine equations does not mean specific algorithms cannot and have not been found for special, particular, such equations.*

What if the realized trajectory lies outside the basin of attraction of a dynamical system capable of computation universality and the objective of policy is to drive the system to such a basin of attraction? This means the policy maker is trying to design a dynamical system capable of computational universality with initial conditions pertaining to one that does not have that capability. Or, equivalently, an attempt is being made, by the policy maker, to devise a method by which to make a Finite Automaton construct a Turing Machine, an impossibility. In other words, an attempt is being made endogenously to construct a 'complex economy' from a 'non-complex economy'. Much of this effort is, perhaps, what is called 'development economics' or 'transition economics'

and various principles of institution design are attempting the recursively impossible. Essentially, my claim is that it is recursively impossible to construct a system capable of computation universality using only the defining characteristics of a Finite Automaton. To put it more picturesquely, a *non-algorithmic step* must be taken to go from systems incapable of self-organisation to ones that are capable of it. This is why ‘development’ and ‘transition’ are difficult issues to theorise about, especially for policy purposes. It must, however, be remembered that this does not mean that the task is impossible in any absolute sense. There may well be non-recursive methods to ‘seek out the boundaries of the equivalent of the basins of attractors of dynamical systems’. There may also be *ad hoc* means by which recursive methods may be discovered for such a task. The above theorem seeks only to state that there are no *general purpose effective methods* for such a policy task. Hence the admonition by some of the pioneers of economic theory - Hayek, above all - to be modest about policy proposals for a complex economy may have been motivated by concerns about being *conned* to by pseudo-mathematical economists, trained in one kind of convenient but irrelevant mathematics.

I should emphasize that no reading of the above framework and results justifies the widespread belief that the new classical have made a formal case for rules against discretion. The framework and results above make a case for an enlightened approach to policy, where poetry and prose may well be the better guides than one-dimensional mathematics.

Perhaps this is the reason for Hayek’s lifelong scepticism on the scope for policy in economies that emerge and form spontaneous orders! It is not for nothing that Harrod’s growth path was on a knife-edge and Wicksell’s cumulative process was a metastable dynamical system, located on the boundary defined by the basins of attractions of two stable elementary dynamical systems (one for the real economy, founded on a modified Austrian capital theory; the other for a monetary macroeconomy underpinned by a pure credit system.)

When policy discussions resort to reliance on special economic models the same unease that causes disquiet when special interests advocate policies should be the outcome. Any number and kind of special dynamic economic models can be devised to justify almost anything - all the way from policy nihilism, the fashion of the day, to dogmatic insistence on rigid policies, justified on the basis of seemingly sophisticated, essentially *ad hoc*, models. Equally, studying patterns by simulating complex dynamical models and inferring structures, without grounding them on the mathematics of the computer is a dangerous pastime. *A fortiori*, suggesting policy measures on the basis of such inferred structures is doubly dangerous. Nothing in the formalism of the mathematics underlying the digital computer, the vehicle in which such investigations are conducted, and simulations by it, justifies formal inferences on implementable effective policies.

Impossibility and undecidability results do not mean paralysis. Arrow’s impossibility theorem did not mean that democratic institution design was abandoned forever; Rabin’s powerful result that even though there are *determined* classical games, it is not possible to devise effective instructions to guide the theoretical winner to implement a winning strategy has not meant that game

theory cannot be a useful guide to policy. Similarly, the results above do not mean that the poets in our profession cannot devise enlightened policies that benefit a complex economy. Perhaps the growth of the complexity of economies calls forth more than intuition based on a thorough familiarity of the institutions of an economy and its behavioural underpinnings. Neither poetry nor prose are algorithmic endeavours - either in their creation or in their appreciation; nor is policy, especially in a complex economy.

Justification for policy - either positively or negatively - cannot be sought in mathematical formalisms. One must resort to poetry and classical political economy, i.e., rely on imagination and compassion, for the visions of policies that have to be carved out to make institutions locate themselves in those metastable configurations that are defined by the boundaries in which dynamical systems capable of universal computation get characterised.

## 4 *Conning* about Rational Expectations Equilibrium and its Learning

In standard mathematical economics, *topological* fixed-point theorems have been used routinely to encapsulate and formalize *self-reference* (*rational expectations* and policy ineffectiveness), *infinite-regress* (*rational expectations*) and *self-reproduction* and *self-reconstruction* (growth), in economic dynamic contexts. This is in addition to, and quite apart from, their widespread use in proving existence of equilibria in a wide variety of economic and game theoretical contexts. The mathematical foundations of topology are, in general, sought in axiomatic set theory. Set theory, however, is only one of four branches of mathematical logic; the other three being, model theory, proof theory and recursion theory<sup>24</sup>. One can associate, roughly speaking, real analysis, non-standard analysis, constructive analysis and computable analysis with these four branches of mathematical logic. Economists, in choosing to formalize economic notions almost exclusively in terms of real analysis, may not always succeed in capturing the intended conceptual underpinnings of economic notions with the required fidelity. My claim in this section is that the use of topological fix point theorems to formalize rational expectations does not capture the two fundamental behavioural notions that are crucial in its definition: *self-reference* and *infinite-regress*. I try, therefore, to reformalize the notion of rational expectations using a recursion theoretic formalism such that fundamental theorems from this field can be invoked and utilized<sup>25</sup>. The idea of self-referential behaviour is, for example, formalized by considering the *action of a program* or an *algorithm* on its *own description*. Infinite regress is, of course, short-circuited, in the usual way, by a fix-point theorem.

Thus, I formalize the notion of *Rational Expectations Equilibria*, *REE*, recursion theoretically, eschewing all topological assumptions. The emphasis is

<sup>24</sup>Some add the *higher arithmetic* (i.e., *number theory*) as an independent fifth branch of modern mathematical logic.

<sup>25</sup>One of which is also called a *fix point theorem*.

on suggesting an alternative modelling strategy that can be mimicked for other concepts and areas of economic theory. The implicit claim is that the dominance of topological fix-point theorems in mathematical economics was - indeed, is - a particularly insidious *con*, perpetrated by the usual one-dimensional approach to mathematics and mathematical logic by the mathematical economists.

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 kind of indeterminacy is unfamiliar to economists with a mathematical bent simply because it is not common in the mathematics of real analysis. These indeterminacies are due to the generic result in computability theory, referred to and invoked in the previous section: 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 that makes it possible to claim that seeking economic theoretic foundations for policy may not be an easy task. To be categorical about policy - positively or negatively - on the basis of mathematical models is a dangerous sport.

## 4.1 Background

In a critical discussion of the use of the Brouwer fixed point theorem by Herbert Simon, [48], 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 ([3], [49], [4] and [50]), 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 appropriate 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 formalization.

There are two crucial aspects to the notion of rational expectations equilibrium - henceforth, *REE* - ([42], pp.6-10): an individual optimization problem, subject to *perceived constraints*, and a system wide, autonomous, set of constraints imposing 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 *REEs* 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  $H^{26}$ . 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,  $H^0$ . The search for fixed-points of a mapping,  $T$ , linking the individually perceived macroeconomic law of motion,  $H$ , and the actual law of motion,  $H^0$  is assumed to be given by a general functional relationship subject to the standard mathematical assumptions:

$$H^0 = T(H) \tag{19}$$

Thus, the fixed-points of  $H^*$  of  $T^{27}$ :

$$H^* = T(H^*) \tag{20}$$

determine *REEs*.

What is the justification for  $T$ ? What kind of ‘object’ 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 optimizing economic theoretical justification for it.

There is also a methodological asymmetry in the determination of  $H$  and  $H^0$ , respectively. The former has a self-referential aspect to it; the latter an infinite regress element in it. Transforming, mechanically, the former into the latter, 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 a 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<sup>28</sup>. 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 optimizing

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<sup>26</sup> 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.

<sup>27</sup> In a space of functions.

<sup>28</sup> In the strict technical sense, as suggested above, of the mathematics of *real analysis* as distinct from, say, *constructive, computable or non-standard analysis*.

agents<sup>29</sup>. It is in this second part where standard economic theory endows agents with varieties of ‘bounded rationality’ postulates, without modifying the full rationality postulates of the underlying, original, individual optimization problem.

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, [33], but from the prior work of Herbert Simon on a problem of predicting the behaviour of rational agents in a political setting, [48] and an almost concurrent economic application by Franco Modigliani and Emile Grunberg, [31]. Let me explain, albeit briefly, to the extent necessary in the context of this essay.<sup>30</sup>

Simon, in considering the general issue of the feasibility of public prediction in a social science context, formalized 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, [48], pp. 82-4; italics added.

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<sup>29</sup>Perceptive readers 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).

<sup>30</sup>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 became 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 or uncomputable 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 an 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 (cf. [63] for a rigorous demonstration of this claim).

Grunberg and Modigliani recognized, clearly and explicitly, 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 (*ad infinitum* – i.e., *infinite regress*), the aggregate outcome, but also were acutely aware of the technical difficulties of infinite regress that was inherent in such situations (cf., in particular, [31], 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 recognized by them ([31], p.468, footnote 13). Interestingly, what eventually came to be called *rational expectations* by Muth was called a *warranted expectation*<sup>31</sup> 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, the constituent functions<sup>32</sup> 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* ([42]) was able to refer to the fixed-point approach to rational expectations, referring to equation (20), above:

"A rational expectations equilibrium is a fixed point of the mapping  $T$ ."  
[42], 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

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<sup>31</sup>I am reminded that Phelps, in one of his early papers introducing the concept of the natural rate of unemployment in its modern forms ([35]), 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.

<sup>32</sup>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*." ([31], 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 *REEs*.



- and they are not to be blamed. They have been conned for so long, and bamboozled, too, by the ubiquity of fixed-point theorems in economic theory that yet another application to a domain of economic theory causes no apparent dissonance, cognitive or otherwise. After all, the same somnambulant complacency, equally due to conning by one-eyed mathematical economists, 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 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.

On the other hand, 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.

## 4.2 Recursion Theoretic Formalisms

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<sup>33</sup>. I need some unfamiliar, but elementary, formal machinery – concepts, definitions, new or alternative connotations for familiar words, etc., – not normally available to the mathematical economist.

**Definition 23** *An operator is a function:*

$$\Phi : \mathcal{F}_m \longrightarrow \mathcal{F}_n \tag{21}$$

where  $\mathcal{F}_k$  ( $k \geq 1$ ) is the class of all partial (recursive) functions from  $\mathbb{N}^k$  to  $\mathbb{N}$ .

**Definition 24**  $\Phi$  is a **recursive operator** if there is a computable function  $\phi$  such that  $\forall f \in \mathcal{F}_m$  and  $\mathbf{x} \in \mathbb{N}^m, y \in \mathbb{N}$ :

$$\Phi(f)(\mathbf{x}) \simeq y \text{ iff } \exists \text{ a finite } \theta \sqsubseteq f \text{ such that } \phi(\tilde{\theta}, \mathbf{x}) \simeq y$$

where<sup>34</sup>  $\tilde{\theta}$  is a standard coding of a finite function  $\theta$ , which is extended by  $f$ .

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<sup>33</sup>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: [10], [11], [29] and [39].

<sup>34</sup>If  $f(\mathbf{x})$  and  $g(\mathbf{x})$  are expressions involving the variables  $\mathbf{x} = (x_1, x_2, \dots, x_k)$ , 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 25** An operator  $\Phi : \mathcal{F}_m \longrightarrow \mathcal{F}_n$  is **continuous** if, for any  $f \in \mathcal{F}_m$ , and  $\forall \mathbf{x}, y$ :

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

**Definition 26** An operator  $\Phi : \mathcal{F}_m \longrightarrow \mathcal{F}_n$  is **monotone** if, whenever  $f, g \in \mathcal{F}_m$  and  $f \sqsubseteq g$ , then  $\Phi(f) \sqsubseteq \Phi(g)$ .

**Theorem 27** A recursive operator is continuous and monotone.

**Example 28** Consider the following **recursive program**,  $\bar{P}$ , (also a recursive operator) over the integers:

$$\bar{P}: F(x, y) \Leftarrow \text{if } x = y \text{ then } y + 1, \text{ else } F(x, F(x - 1, y + 1))$$

Now replace each occurrence of  $F$  in  $\bar{P}$  by each of the following functions:

$$f_1(x, y) : \text{if } x = y \text{ then } y + 1, \text{ else } x + 1 \quad (22)$$

$$f_2(x, y) : \text{if } x \geq y \text{ then } x + 1, \text{ else } y - 1 \quad (23)$$

$$f_3(x, y) : \text{if } (x \geq y) \wedge (x - y \text{ even}) \text{ then } x + 1, \text{ else undefined.} \quad (24)$$

Then, on either side of  $\Leftarrow$  in  $\bar{P}$ , we get the **identical** partial functions:

$$\forall i (1 \leq i \leq 3), f_i(x, y) \equiv \text{if } x = y \text{ then } y + 1, \text{ else } f_i(x - 1, y + 1) \quad (25)$$

Such functions  $f_i$  ( $\forall i (1 \leq i \leq 3)$ ) are referred to as **fixed-points** of the recursive program  $\bar{P}$  (recursive operator).

Note that these are fixed-points of functionals.

**Remark 29** 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  $\bar{P}$ .
- In addition,  $f_3$  is the **only** partial function with this property for  $\bar{P}$  and is, therefore called the **least fixed point of  $\bar{P}$** .

We now have the minimal 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 30** Suppose that  $\Phi : \mathcal{F}_m \longrightarrow \mathcal{F}_m$  is a recursive operator (or a recursive program  $\bar{P}$ ). Then there is a partial function  $f_\Phi$  that is the least fixed point of  $\Phi$  :

$$\Phi(f_\Phi) = f_\Phi;$$

$$\text{If } \Phi(g) = g, \text{ then } f_\Phi \sqsubseteq g.$$

**Remark 31** *If, in addition to being partial,  $f_\phi$  is also total, then it is the **unique least fixed point**. Note also that a recursive operator is characterized 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.*

**Remark 32** *Although this way of stating the (first) recursion theorem almost highlights its non-constructive aspect – i.e., the theorem guarantees the **existence** of a fix-point without indicating a way of finding it – it is possible to use a slightly stronger form of the theorem to amend this ‘defect’ (cf. [32], p.59).*

### 4.3 Recursion Theoretic *REE*

Before stating formally, as a summarizing theorem, the main result (i.e., theorem 33, below) it is necessary to formalize the rational agent and the setting in which rationality is exercised in the expectational domain in recursion theoretic formalisms, too. This means, at a minimum, the rational agent as a *recursion theoretic agent*<sup>35</sup>.

The topological fix-point theorems harnessed by a rational agent are, as mentioned previously, easily done in standard economic theory where the agents themselves are *set-theoretically* formalized. There is no dissonance between the formalism in which the rational agent is defined and the economic setting in which such an agent operates. The latter setting is also set theoretically defined.

The recursion theoretic formalism introduced in the previous sub-section presupposes that the rational agent is now recursion theoretically defined and so too the setting - i.e., the economy. Defining the rational agent recursion theoretically means defining the preferences characterizing the agent and the choice theoretic actions recursion theoretically. This means, firstly, defining the domain of choice for the agent number theoretically and, secondly, the choice of maximal (sub)sets over such a domain in a computably viable way. Such a redefinition and reformalization should mean equivalences between the rational choice of an agent over well defined preferences and the computing activities of an ideal computer, i.e., Turing Machine (or any of its own formal equivalences, by the *Church-Turing Thesis*). Since a complete formalism and the relevant equivalences are described, defined and, where necessary, rigorously proved in Chapter 3 of my *Ryde Lectures* ([59]), I shall simply assume the interested reader can be trusted to refer to it for any detailed clarification and substantiation.

It is now easy to verify that the domain over which the recursive operator and the partial functions are defined are weaker<sup>36</sup> than the conventional domains

<sup>35</sup>This should not cause any disquiet in expectational economics, at least not to those of us who have accepted the Lucasian case for viewing agents as ‘signal processors’ who use optimal filters in their rational decision processing activities (cf. [28], p.9). Agents as ‘signal processors’ is only a special variant of being ‘optimal computing units’.

<sup>36</sup>They are ‘weaker’ in a very special sense. A domain of definition that is number theoretically defined – i.e., over only the rational or the natural numbers – rather than over the whole of the real number system pose natural diophantine and combinatorial conundrums that cannot easily be resolved by the standard operators of optimization.

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 theorem 8, we know that there is a partial function - i.e., a computable function -  $f_t$  that is the least fixed point of  $T$ . Thus, we can summarize the desired result in the form of the following theorem:

**Theorem 33** *Suppose that the reaction or response function,  $T : H_m \longrightarrow H_m$  is a recursive operator (or a recursive program  $\Gamma$ ). 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  $f_t \sqsubseteq g$

**Remark 34** *Theorem 33 can be used directly to show that  $\exists$  a (recursive) program that, under any input, outputs exactly itself. It is this program that acts as the relevant reaction or response **function** for an economy in REE. The existence of such a recursive program justifies the Newclassical methodological stand on the ubiquity of rational expectations equilibria. However, since theorem 33 is stated above in its non-constructive version, finding this particular recursive program requires a little effort. Hence, the need for learning processes to find this program, unless the theorem is utilized in its constructive version. Even with these caveats, the immediate advantage is that there is no need to deal with non-recursive reals or non-computable functions in the recursion theoretic formalism. In the traditional formalism the fix-point that is the REE is, except for flukes, a non-recursive real; constructing learning processes to determine non-recursive reals is either provably impossible or formally intractable (computationally complex).*

What are the further 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 not unimportant in policy oriented economic theoretic contexts 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 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.

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

therefore, suppose that evaluating a compound expression in  $\mathfrak{S}$  is immediate: given an expression in  $\mathfrak{S}$ , say  $\lambda(\varsigma)$ , the variables in  $\lambda$ , when given specific values  $\alpha$ , are to be evaluated according to the *semantics* of  $\mathfrak{S}$ . To actually *evaluate* a compound expression,  $\lambda(\varsigma)$ , we write a *recursive program* in the language  $\mathfrak{S}$ , 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  $\mathfrak{S}$ . 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,  $\mathfrak{S}$ , 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  $\mathfrak{S}$ . Then try to convert the proof into a program in the language  $\mathfrak{S}$ , 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 naturally algorithmic. Therefore, the expressions formed from the language of economic theory, in a topological formalization, are not necessarily implementable by a program, except by flukes, appeal to magic or by illegitimate, intractable and vague approximations. Hence the need to dichotomize 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 *REEs*.

In all of these - and many more - senses, the *REE* and its learning literature are noble purveyors of indiscriminate *conning*.

## 5 Concluding Notes

"The human mind is not a purely logical entity. The complex manner in which it functions is *often at variance with the logic of mathematics*. It is not always pure logic which gives us insight, nor is it chance that causes us to make mistakes. To understand how these processes occur, both successfully and erroneously, we must formulate a distinction between the *mathematical concepts as formally*

*defined and the cognitive processes by which they are conceived."*

Tall and Vinner ([58], p.151; italics added)

What guarantee is there, then, that economic concepts can be mapped unambiguously and surjectively - to be terribly and unnecessarily mathematical about it - into mathematical concepts? The belief in the power and necessity of formalizing economic theory mathematically has thus obliterated the distinction between cognitively perceiving and understanding concepts from different domains and mapping them into each other. Whether the age-old problem of the equality between supply and demand should be mathematically formalized as a system of inequalities or equalities is not something that should be decided by mathematical knowledge or convenience. Surely it would be considered absurd, bordering on the insane, if a surgical procedure was implemented because a tool for its implementation was devised by a medical doctor who knew and believed in topological fixed point theorems? Yet, weighty propositions about policy are decided on the basis of formalizations based on ignorance and belief in the veracity of one kind of one-dimensional mathematics.

## 5.1 A Mathematical Excursion

Thus, consider Hildenbrand's attempt at characterizing 'an axiomatic theory of a certain economic phenomenon as formulated by Debreu' ([13]). This attempt leads to two precepts for formalizing in economics:

"First, the primitive concepts of the economic analysis are selected, and then, each of these primitive concepts is represented by a *mathematical object*.

....

Second, assumptions on the mathematical representations of the primitive concepts are made explicit and are fully specified. Mathematical analysis then establishes the consequences of these assumptions in the form of theorems."

ibid, p.4; italics added.

Even if we grant Hildenbrand the first precept, accepting orthodox characterizations of the 'primitive concepts of economic analysis', there are a variety of mathematics, each with different kinds of mathematical object, onto which these can be mapped. Surely, many economic concepts are naturally combinatorial. The mapping from such combinatorial 'primitive concepts of economics' to an appropriate mathematics is, even at the simplest level, as different as ordinary linear programming as against integer programming. One cannot solve an 'equivalent' linear programming problem, defining economic variables to range over the reals and, then, approximate the solution to the integer programming problem by taking the nearest integer solution to the real solution of the linear programming problem. Non-standard mathematical analysis is quite comfortable with infinitesimal objects. Constructive mathematics does not accept the Church-Turing thesis and, hence, the nature and scope of allowable algorithms

is significantly different from those accepted in recursion theory, which accepts the Church-Turing thesis. The Bolzano-Weierstrass theorem is invalid in constructive analysis; the Heine-Borel theorem has no counterpart in Computable analysis. And so on.

Nor is it the case that the ‘mathematical analysis’ which ‘then establishes the consequences of these assumptions in the form of theorems’ independent of the nature of the mathematical objects and the logic of mathematical reasoning allowed in that particular mathematical analysis. In most forms of Constructive mathematics undecidable disjunctions are eschewed, particularly when they are implications of invoking the *tertium non datur*.

Let me give a famous example from the very heart of number theory, which in its conception and formulation is also elementary: *the prime number theorem*. I choose this example to illustrate the fact that *the nature of the mathematical object*, in this case ordinary numbers, about which a conjecture was made belongs to one particular type of mathematics, i.e., number theory; but the conjecture itself belongs to a different kind of mathematics, i.e., analysis, and its proof, therefore, was sought after in the latter branch of mathematics.

The saga that led to what I like to call the ‘prime number nut’ began with the famous conjectures, independently made, by Euler, Legendre and Gauss that  $\pi(x)$ , the number of primes less than  $x$ , approaches, asymptotically, the quotient  $\frac{x}{\log x}$ , or:

$$\lim_{z \rightarrow \infty} \frac{\pi(x)}{x / \log x} = 1 \quad (26)$$

As Shanks ([46], p.16; last italics in original) observed perceptively:

"No easy proof of the [prime number theorem] is known. The fact that it took a century to prove is a measure of its difficulty. *The theorem is primarily one of analysis. Number theory plays only a small role.* That some analysis must enter is clear from [the above equation] – a *limit* is involved. The extent to which analysis is involved is what is surprising."

But surely recognising that the object about which the conjecture was made was combinatorial and, therefore, techniques of proof belonging to some variant of combinatorial mathematics may well result in a proof that was as simple as it was to state the conjecture? Consider, therefore, the following proof of a bound for  $\pi(x)$ . denote by  $p_i$  ( $\forall i = 1, \dots, m$ ), those prime numbers less than the number  $x$ . Then:

$$x = p_1^{e_1} \cdot p_2^{e_2} \cdot \dots \cdot p_m^{e_m} \quad (27)$$

Clearly each exponent is at most  $\log x$ ; i.e.,

$$e_i \leq \log x \quad \forall i = 1, \dots, m \quad (28)$$

Thus each  $e_i$  can be *effectively*<sup>37</sup> encoded by at most  $\log \log x$  bits. On the other hand, a fundamental result in algorithmic complexity theory shows that

<sup>37</sup>Of course *effectivity* in the strict mathematical sense of recursion theory.

maximally complex numbers  $x$  cannot be encoded by less than  $\log x$  bits; i.e.,

$$\pi(x) \leq \frac{\log x}{\log \log x} \quad (29)$$

This (provably simpler) proof presupposes some elementary knowledge of *algorithmic complexity theory* which, in turn, presupposes some further elementary grounding in classical recursion theory. But the proverbial buck stops there; the basis is computable and combinatorial and hence entirely consistent with the nature of the mathematical object, about which the conjecture was originally made. In the proof - called *proof by the incompressibility method* - itself I have, implicitly, exploited the fact that we can assume most numbers to be maximally descriptively complex (in a precise sense). Then the required contradiction is obtained by supposing that such a number can be (effectively) encoded by exploiting extractable algorithmic patterns. The key methodological difference between proof by the incompressible method and 'traditional' formalistic proofs (particularly *existence* and *lower-upper bound* proofs) is best described by Li and Vitanyi, the most polished exponents and expositors of the 'incompressible method':

"Traditional proofs often involve all instances of a problem in order to conclude that some property holds for at least one instance. The proof would have proceeded simpler, if only that one instance could have been used in the first place. Unfortunately, that instance is hard or impossible to find, and the proof has to involve all the instances. In contrast, in a proof by the incompressibility method, we first choose a random (that is, incompressible) individual object that is known to exist (even though we cannot construct it). Then we show that if the assumed property would not hold, then this object could be compressed, and hence it would not be random"

Li and Vitanyi ([27], p.4)

In the above (sketch of the) proof I have exploited the incompressibility of most numbers by selecting a 'typical  $x$ ', and the *selection* process is *effective*, i.e., no metaphysical *choice axioms* need be invoked at any stage of the demonstration. This remark leads me on to my next point regarding the ubiquity of the axiom of choice in the activities of the mathematical economist. But before that, to continue and close the thread of reasoning involved with the example of the prime number theorem and its proof, let me remind the reader of a wise observation made by Clower and Howitt ([8]) in the context of monetary theory with entirely obvious rational number constraints on the domain of traditional supply-demand variables. Clower and Howitt pointed out that *proofs* of propositions in monetary theory with rational number constraints:

"[N]ecessarily involve the use of number theory - a branch of mathematics unfamiliar to most economists."

ibid, p. 452.



This perceptive observation and the kind of mathematics I used to prove quite simply the prime number theorem are reminders of the fact that ‘the economic problem’ is not naturally to be viewed (always) necessarily from the point of view of classical mathematical analysis; nor need propositions have (always) to be proved by analytical methods. The more substantive examples in the main body of the paper are illustrations of these precepts and, therefore, highlight the problem of the ontology of economic concepts and entities and their unambiguous mapping into mathematical domains.

Then, there is the ubiquity of the *axiom of choice* in the activity of the mathematical economist and the way it cons us into somnambulism is far more sinister in many ways than even the blind use of fixed point theorems and reliance on classical mathematical analysis for formalizations. Each time an article or a text in economic analysis contains the unguarded assertion that ‘agents are indexed over the continuum’, and a particular ‘representative agent’ is identified, there is an implicit reliance on, and explicit invoking of, *the axiom of choice*<sup>38</sup>.

Economically evocative descriptive terms and concepts such as *choice sets*, *selectors*, etc., are associated with the use of this axiom in economics, particularly in discussions about the *foundations of rational choice*. How do mathematical economists who appreciate its formal power justify its use in deriving economic propositions despite its blatant violation of any implemental choice? Not, as one may expect, on the basis of the intuitive richness of its implications; but, peculiarly, on its apparent acceptance by mathematicians:

"The proof of [Szpilrajn's theorem on the existence of an extension that is an ordering] requires an auxiliary proposition called Zorn's lemma [that] is **not intuitively clear**, but it is **demonstrably equivalent** to an important *axiom of choice* that is accepted today by most mathematicians."

Suzumura ([57], pp. 16-7; bold emphasis added)

Have we been reduced to accepting mathematical axioms also on the basis of the simple majority rule? Are we, as economists, supposed to accept the use of a controversial axiom, to put it mildly, to derive important economic propositions, simply on the basis that is ‘accepted today by most mathematicians’? Even if

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<sup>38</sup>I had the melancholy privilege of listening to a seminar given by a Senior colleague of mine, at the University of Trento, where all sorts of silly assumptions about a continuum of agents were made. When I pointed out to him that it was, in fact, not necessary for him to assume a continuum, but that a countable infinity of agents would suffice, his response was: ‘But others using this kind of model assume a continuum of agents’! In exasperation I finally had to write him as follows (in a letter dated 17 June 2003) :

"You cannot start with a continuum assumption and restrict the simulation model to 10 or 20 or 1020 or any number of finite agents; the answers will not correspond to the solution given by the model, except by fluke, and even then one will not be any wiser in an analytical sense.  
Economists do this all the time ....."

we, again as economists, grant this should we not wonder why it was necessary, in the first place, for mathematicians and mathematical logicians to formulate such an axiom and, often, to be quite explicit about actually appealing to it - much more than the half-baked mathematical economists who more often than not do not even realise they are using it and its dubious underpinnings and implications<sup>39</sup>?

## 5.2 An Excursion into Game Theory

An important sub-field of mathematical economics that I have not dealt with must be mentioned in this context of comments about the invoking of the axiom of choice: *Game theory*, a theory which is also heavily dependent, even historically, on the use of fixed point theorems<sup>40</sup>.

In the case of game theory the subversion into its subjective vision of economic behaviour in adversarial situations was a direct consequence of the fixed point approach pioneered by von Neumann and Nash. I think I can make a strong case to substantiate this assertion and, thus, provide a further example of economists and social scientists being conned into a particular vision of economic, social and political processes<sup>41</sup>. My starting point would be Zermelo's celebrated lecture of 1912 ([68]) and his pioneering formulation of an adversarial situation into an *alternating game* and its subsequent formulation and solution as a mini-max problem by Jan Mycielski in terms of *alternating the existential and universal quantifiers*.

The Zermelo game has no subjective component of any sort. It is an entirely objective game of perfect information, although it is often considered part of the orthodox game theoretic tradition. Let me describe the gist of the kind of game considered by Zermelo, first. In a 2-player game of perfect information, alternative moves are made by the two players, say *A* and *B*. The game, say as in Chess, is played by each of the players 'moving' one of a finite number of counters available to him or her, according to specified rules, along a 'tree' - in the case of Chess, of course, on a board of fixed dimension, etc. Player *A*, say, makes the first move (perhaps determined by a chance mechanism) and places one of the counters, say  $a_0 \in A_0$ , on the designated 'tree' at some allowable position (again, for evocative purposes, say as in Chess or any other similar board game); player *B*, then, observes the move made by *A* - i.e., observes, with perfect recall, the placement of the counter  $a_1$  - and makes the second move by

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<sup>39</sup>Kuratowski and Mostowski, whose monumental text on Set Theory ([23]) is often invoked by mathematical economists, alert the reader each time a theorem is established using the axiom of choice. This is not the only classic mathematical or mathematical logic text to do so. Would that economists could also be knowledgeable and sensitive enough to do so!

<sup>40</sup>In fact, in a sense, the Arrow-Debreu formalization 'borrowed' the fixed point approach from Nash. But I am not writing a history of mathematical economics, however much such an approach would be the ideal way to discuss all the *cons* and *conning* I am interested in. That must wait for a different exercise.

<sup>41</sup>In direct analogy with the kind of observation made by Steve Smale about transforming an intrinsic equation approach to the problem of supply-demand equilibrium to one of inequalities formulated as fixed point problems.

placing, say  $b_1 \in B_1$ , on an allowable position on the ‘board’; and so on. Let us suppose these alternating choices terminate after Player B’s  $n$  – *th* move; i.e., when  $b_n \in B_n$  has been placed in an appropriate place on the ‘board’.

**Definition 35** A *play* of such a game consists of a sequence of such alternative moves by the two players

Suppose we label the alternating individual moves by the two players with the natural numbers in such a way that:

1. The even numbers, say,  $a(0), a(2), \dots, a(n-1)$  enumerate player A’s moves;
  2. The odd numbers, say,  $b(1), b(3), \dots, b(n)$  enumerate player B’s moves;
- Then, each (finite) play can be expressed as a sequence, say  $\gamma$ , of natural numbers.

Suppose we define the set  $\alpha$  as the set of plays which are wins for player A; and, similarly, the set  $\beta$  as the set of plays which are wins for player B.

**Definition 36** A *strategy* is a function from any (finite) string of natural numbers as input generates a single natural number, say  $\sigma$ , as an output.

**Definition 37** A game is **determined** if one of the players has a winning strategy; i.e., if either  $\sigma \in \alpha$  or  $\sigma \in \beta$ .

**Theorem 38** *Zermelo’s Theorem:*  $\exists$  a winning strategy for player A, whatever is the play chosen by B; and vice versa for B

**Remark 39** This is Zermelo’s version of a minimax theorem in a perfect recall, perfect information, game.

It is in connection with this result and the minimax form of it that Steinhaus observed, with considerable perplexity:

"[My] inability [to prove the minimax theorem] was a consequence of the ignorance of Zermelo’s paper ([68]) in spite of its having been published in 1913. .... J von Neumann was aware of the importance of the minimax principle (cf. [65]); it is, however, *difficult to understand the absence of a quotation of Zermelo’s lecture in his publications.*"

Steinhaus ([55], p. 460; italics added)

Why didn’t von Neumann refer, in 1928, to the Zermelo-tradition of alternating games? The tentative answer to such a question is a whole research program in itself and I will simply have to place it on an agenda and pass on. I have no doubts whatsoever that any serious study to answer this almost rhetorical question will reap a rich harvest of further cons perpetrated by the

mathematical economists, perhaps inadvertently. The point I wish to make is something else and has to do with the axiom of choice and its place in *economic conning*. So, let me return to this theme.

Mycielski (cf., [55], pp. 460-1) formulated the Zermelo minimax theorem in terms of alternating logical quantifiers as follows<sup>42</sup>:

$$\sim \left\{ \bigcup_{a_0 \in A_0} \bigcap_{b_1 \in B_1} \dots \bigcup_{a_{n-1} \in A_{n-1}} \bigcap_{b_n \in B_n} (a_0 b_1 a_2 b_3 \dots a_{n-1} b_n) \right\} \in \alpha$$

$$\implies \left\{ \bigcap_{a_0 \in A_0} \bigcup_{b_1 \in B_1} \dots \bigcap_{a_{n-1} \in A_{n-1}} \bigcup_{b_n \in B_n} (a_0 b_1 a_2 b_3 \dots a_{n-1} b_n) \right\} \notin \beta$$

Now, summarizing the structure of the game and taking into account Mycielski's formulation in terms of alternating we can state as follows:

1. The sequential moves by the players can be modelled by alternating existential and universal quantifiers.
2. The existential quantifier moves first; if the total number of moves is odd, then an existential quantifier determines the last chosen integer; if not, the universal quantifier determines the final integer to be chosen.
3. One of the players tries to make a logical expression, preceded by these alternating quantifiers *true*; the other tries to make it *false*.
4. Thus, inside the braces the win condition in any play is stated as a proposition to be satisfied by generating a number belonging to a given set.
5. If, therefore, we can extract *an arithmetical form* - since we are dealing with sequences of natural numbers - for the win condition it will be possible to discuss recursive solvability, decidability and computability of winning strategies.

The above definitions, descriptions and structures define, therefore, an *Arithmetical Game* of length  $n$  (cf. [59], pp. 125-6 for a formal definition). Stating the Zermelo theorem in a more formal and general form, we have:

**Theorem 40** *Arithmetical Games of finite length are determined.*

The more general theorem, for games of arbitrary (non-finite) length, can be proved by standard diagonalization arguments and is<sup>43</sup>:

**Theorem 41** *Arithmetical Games on any countable set or on any set which has a countable complement is determined.*

<sup>42</sup>Readers who are knowledgeable about mathematical logic - particularly recursion, proof and model theories - will recognise, in this formulation, the way Gödel derived *undecidable sentences*.

<sup>43</sup>The real time paradox of implementing an infinite play is easily resolved (cf., [55], pp. 465; [59], chapter 7).

Now, enter the axiom of choice! Suppose we allow any unrestricted sets  $\alpha$  and  $\beta$ . Then, for example if they are *imperfect sets*<sup>44</sup>, the game is not determined. If we work within ZFC, then such sets are routinely acceptable and lead to games that cannot be determined - even if we assume perfect information and perfect recall. Surely, this is counter-intuitive? For this reason, this tradition in game theory chose to renounce the axiom of choice and work with an alternative axiom that restricts the class of sets within which arithmetical games are played. The alternative axiom is the *axiom of determinacy*, introduced by Steinhaus:

**Axiom 42** *The Axiom of Determinacy: Arithmetical Games on every subset of the Baire line*<sup>45</sup> *is determined.*

The motivation given by Steinhaus ([55], pp. 464-5) is a salutary lesson for mathematically minded economists or economists who choose to accept the axiom of choice on 'democratic' principles or economists who are too lazy to study carefully the economic meaning of accepting a mathematical axiom:

"It is known that [the Axiom of Choice] produces such consequences as the decomposition of a ball into five parts which can be put together to build up a new ball of twice the volume of the old one [the Banach-Tarski paradox], a result considered as paradoxical by many scientists. There is another objection: how are we to speak of perfect information for [players] A and B if it is impossible to verify whether both of them think of the same set when they speak of [" $\alpha$ "]? *This impossibility is inherent in every set having only [the Axiom of Choice] as its certificate of birth.* In such circumstances it is doubtful whether human beings will ever play really [an infinite game].

*All these considerations impelled me to place the blame on the Axiom of Choice.* Sixty years of the theory of sets have elapsed since this Axiom was proclaimed, and some ideas have .... convinced me that a purely negative attitude against [the Axiom of Choice] would be dangerous to propose. Thus I have chosen the idea of replacing [the Axiom of Choice] by the [above Axiom of Determinacy].  
italics added.

There is a whole tradition of game theory, beginning at the beginning, so to speak, with Zermelo, linking up, via Rabin's modification of the Gale-Stewart infinite game, to recursion theoretic formulations of arithmetical games underpinned by the *axiom of determinacy* and completely independent of the *axiom of choice* and **eschewing all subjective considerations**. In this tradition notions of *effective playability*, *solvability* and *decidability* questions take on fully meaningful computational and computable form where one can investigate whether it is feasible to instruct a player, who is known to have a winning

<sup>44</sup> A set  $\mathcal{F}$  is a *perfect set* if it is a *closed set in which every point is a limit point*.

<sup>45</sup> A *Baire line* is an *irrational line* which, in turn, is a line obtainable from a continuum by removing a countable dense subset.

strategy, to actually select a sequence to achieve the win. None of this is possible in the orthodox tradition, which *cons* us into a somnambulance that there are no alternative mathematics for investigating, mathematically, adversarial situations in the social sciences.

### 5.3 A Very Brief Note on Macrodynamics

In macrodynamics, the current frontiers seem to be dominated by the new classicals who have gradually begun to refer to this sub-discipline as *Recursive Macroeconomics*<sup>46</sup>. Their vision for the nature and future of macroeconomics, a field in which computation and dynamics is almost intrinsic to its problems, is best reflected in the way the subject is characterised by a leading proponent, a Nobel Laureate of recent years<sup>47</sup>, along the lines of the predominance of ‘tools’ in determining the nature of the subject<sup>48</sup>:

"... I want to emphasize that the methodology that transformed macroeconomics is applicable to the study of virtually all fields of economics. In fact, the meaning of the word *macroeconomics* has changed to *refer to the tools being used*<sup>49</sup> rather than just to the study of business cycle fluctuations."

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<sup>46</sup>‘*Recursive*’, because the *tools* used to formalize macroeconomic concepts and entities are determined by the mathematics of: *Markov Decision Processes* (Wald), *Dynamic Programming* (Bellman) and (Kalman) Filtering, all of which have a ‘recursive’ structure. The reader should be warned that ‘*recursive*’ in this sense has nothing whatsoever to do with ‘*recursion theory*’ in any sense whatsoever.

<sup>47</sup>He goes even further when he points out, pungently and frankly:

"What I am going to describe for you is a revolution in Macroeconomics, a transformation in methodology that has reshaped how we conduct our science. Prior to the transformation, macroeconomics was largely separate from the rest of economics. Indeed, some considered the study of macroeconomics fundamentally different and thought there was no hope of integrating macroeconomics with the rest of economics, that is, with neoclassical economics. Others held the view that neoclassical foundations for the empirically determined macro relations would in time be developed. Neither view has proved correct."

Prescott ([38])

Of course, Prescott does not recognise the existence of non- neo classical economics; sometimes not even varieties of neo classical economics ([37]).

<sup>48</sup>I cannot resist the temptation to add, as a counter-weight to this sanguine view a trenchant observation made by a previous Nobel Laureate, who may not have been unsympathetic to the new classicals, when he reviewed the classic of an earlier generation, Paul Samuelson’s *Foundations of Economic Analysis* ([?], p. 605):

"... [W]ho can know what tools we need unless he knows [the material on which they will be used]."

<sup>49</sup>Prescott is, of course, referring to mathematical and computational tools. He does not realise that the mathematical framework in which his theories are couched is intrinsically uncomputable and non-constructive. In fact, the Prescott-Kydland research program, apart from resting on Lucasian foundations, is also underpinned by the framework of computable general equilibrium theory. Neither the practitioners of CGE nor the second-hand followers have ever investigated whether CGE models are actually constructive or computable. In fact they are neither (cf.,[63] )

Prescott ([37]; second set of italics, added)

Moreover, as far as the New Classicals are concerned, the mathematical foundations of neo classical economics is provided by general equilibrium theory (and, occasionally, game theory). Since their foundations are in orthodox general equilibrium theory - i.e., neoclassical microeconomics - there is no special reason for me to identify the cons that we are subject to, at their hands: they will be the same ones that emanate from the issues discussed in earlier sections of this paper, on dynamics, policy and REE. On the other hand, the new classical macrodynamic theorist is very explicit about the crucial role of the *computational experiment* in macrodynamics ([25]). The concept and contents of a computational experiment in a macrodynamic model is direct generalization of the *static computable general equilibrium framework (CGE)*, particularly the one having its origins in the work of Shoven and Whalley ([47] which, in turn, is based on the pioneering contributions to the field by Herbert Scarf ([43]):

"Shoven and Whalley ([47]) were the first to use what we call the computational experiment in economics. The model economies that they used in their experiments are static and have many industrial sectors."

Kydland & Prescott ([25], p. 69, footnote 2).

But Kydland and Prescott misrepresent the actual theoretical nature of the computational model used in [47]. That model is neither constructive nor computable, contrary to the claims in [47]. So, they could not be carrying out a consistent 'computational experiment' that could be underpinned by any theory. The hollowness of the claims of computability and constructivity - i.e., a computational experiment - is fully described and elucidated in [63].

In the case of new classical macrodynamics there is also the particular and peculiar reliance on the functional equations of Bellman. Almost the first thing an advanced undergraduate or a beginning graduate student is taught is the way of formulating any given representative agent's dynamic optimization problem in terms of the Bellman equation. The particular forte of this formulation, as claimed by most mathematical macroeconomists, is that it is in 'recursive' form and hence amenable to a fixed point approach(cf [1], p.12)! So, there we are; back to square one on the *conning* front.

However, this is not entirely correct. It is not necessary to invoke the full force of topological fixed point theorems (cf. [30], p.275) and, therefore, one is not saddled with the many whimsical conning mathematical assumptions that make the computational experiment infeasible. It is 'only' necessary to invoke the *contraction mapping theorem* in metric spaces (cf [16], pp. 166-7 & p. 177) a paper referred to at almost the very beginning of the Kydland-Prescott research program (cf. [24]), by them, to codify the idea of the *computational experiment*). But here, too, there are so many whimsical and fragile assumptions at the very recesses of the mathematical framework that I am almost reluctant to saddle the end of this already very long paper with their nature. But here is an ultra-brief

hint. The contraction mapping theorem is defined on a complete metric space. This characteristic is a generalization of the idea of **Cauchy Completeness**, which is given by the theorem:

**Theorem 43** *Every Cauchy sequence in  $\mathbb{R}$  converges to an element of  $\mathbb{R}$*

This theorem is, in turn, proved using the Bolzano-Weierstrass theorem, which contains an unconstructible - i.e., non-algorithmic and hence impossible to utilise in a consistent ‘computational experiment’ - undecidable disjunction in its proof! And so we go on and on. Somewhere, buried in the recesses of almost every mathematical result used by conventional mathematical macrodynamics, there are undecidable disjunctions that make a mockery of the idea of computational experiments. Nothing less than whimsey and fragility seem to be the ultimate disciplining criteria for much of the mathematics of new classical macrodynamics.

I shall not even begin to discuss the way recent modelling exercises by the new classicalists have relied on a *continuum of agents* in a way that makes any notion of computation completely nonsensical.

## 5.4 Final Remarks

A continuum of agents populate many whimsical and fragile mathematical macrodynamic models. Non-constructive and uncomputable fixed point theorems are invoked to prove the existence of uncomputable equilibria which are then computed by uncomputable numerical procedures. Axioms whose implications are illusory are invoked routinely at the foundations of economic theory. Such are the whimsies and fragilities of ordinary, bread-and-butter orthodox mathematical and mathematized economic theory. To weed the whimsies and fragilities out of these frameworks may not be a worthwhile exercise. To be aware of them is, on the other hand, almost imperative - so that related mistakes need not be made by a new generation of mathematically able, numerically literate, computationally able, students of economics.

I should add here, in conclusion, that there are non-orthodox varieties of economic theory with powerful mathematical underpinnings<sup>50</sup>. For the purposes of this paper I have concentrated on a few themes because it was possible to frame them and tackle them from an almost unified or homogeneous point of view. If this exercise is reasonably successful, I may undertake a broader study, incorporating orthodox and non-orthodox mathematical economics in my quest to ‘find’ the *Con* that they, too, might encapsulate.

I can do no better than conclude this paper by recalling an eloquent admonition by a distinguished mathematician who tried valiantly to construct a mathematical economics without cons:

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<sup>50</sup>I have, in the first instance, in mind Piero Sraffa’s remarkable *Production of Commodities by Means of Commodities* ([54]) and Jacob Schwartz’s *Lectures on the Analytical Method in Economics* ([44]). These two books, each in their own way, extol the virtues of non-orthodox approaches to economics using non-routine mathematical tools, concepts, frameworks and proof techniques. A first pass at studying one of them is in ([64]).



"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 prestidigitation - *a glittering deception in which some are entrapped, and some, alas, entrappers*." ([45], pp. 22-3, italics added)

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