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REFLECTIONS ON MATHEMATICAL ECONOMICS IN THE ALGORITHMIC MODE[♣]

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[♣] This paper is, essentially, a 'series' of reflections on comments by distinguished economists, who also happen to be personal friends and, more importantly, friends of ASSRU, on *Taming the Incomputable, Reconstructing the Nonconstructive and Deciding the Undecidable in Mathematical Economics* (forthcoming in **New Mathematics and Natural Computation**, 2011).

Abstract

Non-standard analysis can be harnessed by the recursion theorist. But as a computable economist, the conundrums of the Löwenheim-Skolem theorem and the associated Skolem paradox, seem to pose insurmountable epistemological difficulties against the use of algorithmic non-standard analysis. Discontinuities can be tamed by recursive analysis. This particular kind of taming may be a way out of the formidable obstacles created by the difficulties of Diophantine Decision Problems. Methods of existence proofs, used by the 'classical' mathematician – even if not invoking the axiom of choice – cannot be shown to be equivalent to the exhibition of an instance in the sense of a constructive proof. These issues were prompted by the fertile and critical contributions to this special issue.

1 Preliminaries

> "We have, therefore, a situation in which essentially discrete phenomena are modelled by continuous functions which must then be discretized for calculational purposes.
>
> Would it not make more sense to treat the discrete problem discretely in the first place?
>
> [20], p.216

In [31], I concentrated on trying to understand a *fragment* of orthodox mathematical economics from the point of view of (classical) computability theory, constructive analysis[1] and (algorithmic) undecidability. It was inevitable that there were the twin horns of a dilemma that I was not able to resolve: on the one hand, to be fair and representative with the kind of mathematics I could – and should - have invoked, from the points of view of one or the other of the three algorithmic foundations for computability theory, constructive analysis and undecidability; on the other hand, to avoid being too idiosyncratic about the chosen 'fragment' of mathematical economics.

Moreover, I left aside, for other exercises more detailed treatment of the impact of developments in computational,. algorithmic and stochastic complexity theories on economic theorizing and applied economics, particularly in their policy contexts. This was partly due to the fact that I was working, almost simultaneously, and in parallel, on a fairly comprehensive study of computational complexity theory – with subsidiary considerations of algorithmic and stochastic complexity theories – from the point of view of a computable economist (see [32]).

These are two serious lacunae that cannot be resolved adequately within the scope and natural limits of a Journal article, despite the fact that the Editors of **New Mathematics and Natural Computation** have been over-generous in the space they have granted me.

However, the six distinguished scholars[2], who have taken the time, and made the effort, to try to understand the vision of a computable economist, sympathetically and generously, have contributed entirely original visions of their own, quite independently of a strict interpretation of [31] alone. Rosser's detailed and thought-provoking interpretation is also a valuable work on understanding one aspect of the methodology of Barkley Rosser, Snr., to whose Centennial Memory [31] was originally dedicated. Duncan Foley's ingenious emphasis on the relevance and importance of approximation theory is characteristically lucid. Stephen Kinsella's own development of an algorithmic economics - its necessity and desirability - seems to have its starting point in the kind of work in [31],

[1] I did not try to be precise about the kind of constructive analysis I was adhering to, or invoking. Today I would be more specific and base myself more 'dogmatically' on Brouwerian constructivism, with its underpinning in Intuitionism.

[2] Francisco Doria's contribution to this Special Issue is an entirely independent contribution and has, ostensibly, nothing to do with the contents of [31]. However, Doria is very familiar with my work - and I with some of his - and I am sure there is some deep connection and meaning, for and with computable economics, in his fascinating contribution.

and my earlier and contemporary writings and thoughts. Stefano Zambelli's comprehensive survey of computable economics – its origins, its developments, the vicissitudes it underwent in the form of almost 'still births' and numerous teething problems – could only have been given by one who was at least a fellow pioneer in the adventure and excitement of the genesis of a wholly new research program. Cassey Lee has emphasized methodological aspects that can be culled out of [31] and, in general, in my many earlier writings, and contemporary work. Sundar Sarukkai brings the fresh and challenging perspective of a Philosopher of Mathematics, with serious and deep underpinnings and understanding of modern theoretical physics and its methodologies to an interpretation of my paper.

It is completely unnecessary for me to try to summarize or re-interpret interpretations; there is no end to such exercises. Moreover, each of the above contributions are concisely clear and need no secondary interpretations. It will also be slightly unfair of me to try to comment or correct any particular interpretation, whether I agree with it or not, since the distinguished contributors are not themselves going to respond further to my possible comments.

I welcome, therefore, the informed contributions by sympathetically critical friends, whose insights and questions allow me to clarify three issues, hopefully to dispel careless and uninformed remarks and prejudices by those intrinsically hostile to any questioning of the dominance of real analysis in mathematical economics. The three issues are: the place of non-standard analysis in mathematical economics in the algorithmic mode; the computability of discontinuous processes; and the role of proof in the theology that has become mathematical economics, following blindly a misunderstood classical mathematical analysis.

There is one minor point in Sundar Sarukkai's major and most interesting contribution which I might be allowed to clarify. Professor Sarukkai suggests that 'the central argument [in Velupillai's paper]' is '*how* mathematics is used in physics should be a model for *how* it can be used in economics.' I am afraid my examples of the Dirac delta function and the Feynman integral, and the way I may have incorporated the saga of their emergence and place in mathematical physics, surely, had the flavour of an 'evangelist'! It is entirely understandable, therefore, that Professor Sarukkai was misled into thinking that my focus was on '*how* mathematics is used in physics should be a model for *how* it can be used in economics.' Nothing could be further from my aims[3]. The felicitous examples of the saga of the Dirac delta function and the Feynman integral were harnessed to make the case against moulding a theory to fit a prior mathematics – the common practice in orthodox mathematical economics. Beauty of the mathematical formalization of a theory – as in the case of Dirac – and the minimum functionality of mathematical operators for a physically relevant mathematical physics – as in the case of Feynman – have never been the motive force for the mathematization of economic theory (with notable and rare exceptions).

[3]More recently, during a happy meeting at a Workshop in Trento I was able to clarify my stance to Sundar Sarukkai. But his contribution is of independent interest, quite separate from structuring his ingenious vision around my fictitious vision. There are many advantages in structuring interesting visions around imaginary cores.

At a recent Trento two-day Workshop on Experimental Economics, organised by the *Computable*[4] and Experimental Economics Laboratory (CEEL), I gave a talk on *The Algorithmic Revolution in the Social Sciences* ([33]). At the end of my talk one of my senior colleagues wondered whether I was a Bayesian; when I replied I preferred to be agnostic about it, at least *pro tempore*, he informed (not suggested; not questioned) me that I would be forced to admit 'continuity' if I did don a Bayesian hat in any of my statistical work. This may have been a piece of information he felt obliged to give me in view of the important part played by Stochastic Complexity Theory in my vision of the Algorithmic Social Sciences and in turn, the role played by Bayesian theory in it.

This kind of uninformed inference, every time one mentions algorithmic methods and their mathematical underpinnings in recursion theory and constructive mathematics, is very common, despite the existence (sic!) of computable or recursive analysis and constructive analysis – at least since the birth of recursion theory over 70 years ago and Brouwerian constructivism almost a century ago. The inference is that algorithmic methods are about the discrete and digital. Analysis, characterised by limit and approximation processes, is supposed to be alien to algorithmic methods, to the digital computer and to a mathematical economics based on recursion theory of constructive mathematics[5].

2 Non-Standard Analysis and Computability

> "The very first model of nonstandard analysis, due to Schmieden and Laugwitz ([22]), was in fact completely constructive. ... We emphasise that the development [in this paper] is done in compliance with Bishop's strict constructivism ([2]), and that it may indeed be formalised within Martin-Löf's type theory [see, for example, [14]], which will be the official metatheory in case of doubt. Thus *it is in principle possible to extract algorithms from all the existence results we establish.*"
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> [17], p. 233, 235; italics added.

[4] Although this laboratory is in a department of which I have been a fully fledged member for a decade, I am still mystified by the appellation 'computable' in the name of the laboratory. To the best of my knowledge, no one who has anything to do with the running of the laboratory, nor anyone of the graduate students who have done - or are doing - their doctorate under its auspices have the slightest idea of what computable economics signifies. This is despite the fact that both Stefano Zambelli and I have lectured, regularly, on Computable Economics, at the Graduate School, to which CEEL is also attached - and despite the appearance of my book ([29]), exactly ten years ago, the year I began my association with the department of economics at the University of Trento.

[5] Orthodox economists, such as Spear and Durlauf, have made remarks along these lines in articles or reviews published in so-called prestigious Journals (see, for e.g., [27]). Remarks about constructive proofs, without a clear understanding of what such a thing means, are particularly replete in game theory (see, for e.g., [16]). The first mistake is about the meaning of the domain and range of relevance in algorithmic mathematics; the second is about the nature of proof in one kind of algorithmic mathematics.

Barkley Rosser[6] questioned, perceptively, my stand on non-standard analysis and had some incisive observations on the link between non-standard analysis and constructive mathematics. Just to make my position on the role, indeed the importance of non-standard analysis, in algorithmic economics clear, I shall quote an early stance I took on this important issue. In [30], I pointed out (footnote 1, p. 587):

> Although it may appear paradoxical, I am of the opinion that *non-standard analysis* should be placed squarely in the constructive tradition - at least from the point of view of practice. Ever since Leibniz chose a notation for the differential and integral calculus that was conducive to *computation*, a notation that has survived even in the quintessentially *non-computational* tradition of classical real analysis, the practice of non-standard analysis has remained firmly rooted in applicability from a computational point of view. Indeed, the first *modern* rejuvenation of the non-standard tradition in the late 50s and early 60s, at the hands of Schmieden and Laugwitz (cf. [22]), had constructive underpinnings. I add the caveat 'modern' because Veronese's sterling efforts (cf.[35]) at the turn of the 19th century did not succeed in revitalising the subject due to its unfair dismissal by Peano and Russell, from different points of view. The former dismissed it, explicitly, for lacking in 'rigour'; the latter, implicitly, by claiming that the triple problems of the *infinitesimal, infinity* and the *continuum* had been 'solved'.

However, I have not made - as yet - a sustained case for non-standard analytic computable economics. The reason is that I have not been able to come to terms with the full philosophical and epistemological force of the *Löwenheim-Skolem theorem* and the *Skolem paradox*[7]. Skolem gave two proofs of what is now called the Löwenheim-Skolem theorem, one in 1920, where he used the axiom of choice and, later, in 1922, without the axiom of choice. He concluded the beautiful 1922 Lecture[8] with an observation that is of particular relevance for the way I believe algorithmic mathematical economics should be formalized. viz., without any reliance or foundations in set theory (in particular, ZFC):

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[6] I shall not try to distinguish between Rosser Snr., and Rosser Jnr., in any pedantic way, in the main text!

[7] For the absolute novice, a loose statement of the *Löwenheim-Skolem theorem* goes something like this: *Every formal system expressed in the first order functional calculus has a denumerable model.* The *Skolem paradox*, on the other hand, although not a 'genuine' paradox in the same sense of the other logical antinomies, is also philosophically and epistemologically disturbing. Again, informally phrased, the Skolem paradox states that there is a first order theory, such that if it has an intended model, it has both a countable and an uncountable model. Hunter ([10], Part Three), is a reasonably clear and accessible reference to a formal approach to these important issues. Shapiro ([24]), on the other hand, has a clear, albeit concise, discussion of the paradoxical implications of the theorem and the paradox. The rigorous versions of the theorem and the paradox, together with a characteristically illuminating discussion, can be found in *the Kleene 'classic'* ([11], especially, pp. 425-7).

[8] Delivered to the Fifth Congress of Scandinavian Mathematicians, held in Helsinki, in August, 1922.

> "....I believed that it was so clear that *axiomatization in terms of sets was not a satisfactory ultimate foundation of mathematics*, that mathematicians would, for the most part, not be very much concerned with it. But in recent times I have seen to my surprise that so many mathematicians think that these axioms of set theory provide the ideal foundation for mathematics; therefore it seemed to me that the time had come to publish a critique. "
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> [25], pp. 300-1; italics added.

It was not that Skolem was sceptical about the need for – and the possibility of – foundations for mathematics; but he desired it to be 'recursive', possibly based on inductive definitions - he did not think the foundations could be found, or should be sought, in axiomatic set theory. Orthodox mathematical economics – even when using non-standard analysis – seems to pride itself in the fact that the mathematics it uses is founded on *ZFC*.

I have not had time to sort these issues in a clear and simple way, so far, but hope to come to a view in the near future. Till, then, I have confined myself to using non-standard analysis in bridging the gap between the use of *ad hoc* discontinuities in standard nonlinear dynamics and rigorous continuity in non-standard analysis, using infinitesimals imaginatively, even on digital computers.

Related to this is my particular personal satisfaction to note that the *non-standard* proof of Peanos' existence theorem for ODE's avoids the use of the Ascoli lemma. In the main paper, published in this Issue, I mentioned the non-constructive nature of the Ascoli lemma and its suspicious use in the *standard* proof of Peano's existence theorem for ODEs (see, the last paragraph just before the beginning of § 3.1)[9].

I would like to take this opportunity to add a very 'personal note' on the way I came to become familiar with non-standard analysis, particularly because mathematical economists seem to think the revival of the noble tradition of infinitesimals owes everything to Abraham Robinson's undoubted contributions. Moreover, very few mathematical economists can even imagine that the infinitesimals of non-standard analysis make it possible to dispel with the ad-hoc discontinuities even in so-called rigorous non-linear dynamics, via relaxation oscillations (see [34] on the non-standard analysis of the existence (sic!) of multiple limit cycles in the van der Pol equation, ubiquitous in endogenous business cycle theories).

Economists routinely reason in terms of infinitesimals, without, of course, realizing it. Every time mathematical economists cavalierly invoke 'price taking' behaviour due to the insignificance of individual agents in a perfectly competitive market, they are invoking poor old Archimedes, too. My own realization of his immanent presence in the mathematics I was using came about entirely accidentally, but felicitously.

A completely accidental find, at a Cambridge antiquarian bookshop, of Max Newman's copy of Hobson's classic text on real analysis, [8], during what turned

[9] The elegant exposition of the non-standard proof is given on pp. 30-1, Theorem 1.5.1, in [1].

out, subsequently, to be a melancholy visit to that city in late 1977, was the beginning of my initiation into non-Archimedean mathematics. It so happened that I was spending that academic year as a Research Fellow at *C.O.R.E*, in Louvain-La-Neuve and my neighbouring office was occupied by Robert Aumann. I found Hobson's book eminently readable – all 770 pages of it, in that first edition format I was reading; it later expanded into double that size in later editions. However I was perplexed by the fact, clearly pointed out in the book, that Hobson referred to Giuseppe Veronese as the modern 'resurrector' of the older Leibniz-Newton notion of infinitesimals and his – Veronese's – development of a calculus devoid of the Archimedean assumption, ([8], pp.54-6). The perplexity was, of course, that none of the historical allusions to the founding fathers of nonstandard analysis even remotely referred to Veronese as one of them. There were the great originators: Leibniz and Newton; then there was the great resurrection by Skolem; and, finally, the 'quantum' jump to Abraham Robinson. Neither Peirce, nor Veronese, both of whom explicitly and cogently denied the Archimedean axiom in their development of analysis, were ever referred to, at least in the 'standard' texts on nonstandard analysis.

Aumann, who had done much to make continuum analysis of price taking behaviour rigorous in mathematical economics was my neighbour. One morning I dropped by at his office and showed him the pages in Hobson's book, referring to Veronese's nonstandard analysis, and asked him whether it was not a proper precursor to Abraham Robinson's work and a clear successor to Leibniz and Newton, at least with respect to infinitesimals and the (non-) Archimedean axioms? He promised to read it carefully, borrowed my book, and disappeared, as he usually did, on a Friday. He returned on the following Monday, gave me back my copy of Hobson with a cryptic, but unambiguous, remark: 'Yes, indeed, this work by Veronese appears to be a precursor to Abraham Robinson'.

Why had Veronese's modern classic, [35], 'disappeared' from orthodox histories of nonstandard analysis, at least at that time? Some rummaging through the historical status of Veronese's work on non-Archimedean analysis, particularly in Italy, gave me a clue as to what had happened. It was Veronese's misfortune to have published his work on nonstandard analysis just as his slightly younger great Italian mathematical contemporary, Giuseppe Peano, was beginning his successful crusade to consolidate the movement to make standard real analysis rigorous. Veronese's book was severely criticised[10] for falling foul of the emerging *orthodox standards of 'rigour'* and fell off the backs of the official mathematical community like water off of a *duck*'s back. If only they knew what *nonstandard ducks* would eventually be shown to be capable of, just in the study of the van der Pol equation alone(see [34]!

[10]See, in particular, Peano's 'open letter' to Veronese, [18], in the very first volume of the Journal Peano founded in 1891, *Rivista di Matematica*. The hands of fate have a way of making confluences toll heavily in one direction than another! I may add that my interest, as a Trento economist, has a regional patriotic flavour in favour of Veronese. He was from Chioggia, 'here' in the Northeast of Italy; Peano was from, Spinetta, near Cuneo, at the other end of the horizontal divide of Italy, the Northwest!

3 Computably Approximating Discontinuities

> "Algorithms are becoming worthy of analysis in their own right, not merely as a means to solve other problems. This I am suggesting that as the study of an equation (e.g., manifold) played such an important role in 20th-century mathematics, the study of *finding the solutions* (e.g., *an algorithm*) may play an equally important role in the next century."
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> [26], p.8; italics added.

Duncan Foley's concise, but characteristically focused contribution centres on the importance of the notion of, and the theory of, approximations, and its almost complete neglect in orthodox economic theory. I believe I have, in my contribution (but see also [32]) — perhaps more between the lines than explicitly — underlined the importance of a rigorous theory of approximations in every kind of algorithmic theory, but very particularly so in computable and constructive analysis.

Here, I would like to take the opportunity to clarify a vague, and apparently 'throwaway' remark, and use it to develop a theme about the computability of 'discontinuous' equations, thus continuing (sic!) a theme broached in the previous section.

I stated, in footnote 17, of the main paper in this Issue, as follows:

> "The generalization of the function concept, inspired by the needs of theoretical physics as envisaged by a supremely *intuitively rigorous* theoretical physicist, that resulted in 'Distributions', had the important 'computational' purpose of being able to work with functions that were *always* differentiable. After the Weierstrassian 'monstrosities' had been discovered, *first by Takagi*, in 1903, in an explicit example, (cf. [28]), of *functions* that were 'continuous everywhere but differentiable nowhere', there was a hiatus waiting to be filled: a hiatus that could have been filled either by re-interpreting the notion of function or generalizing it. The latter path resulted - but not because it was an exercise in 'gratuitous generalization' ([23], p. 211); but because it was motivated by the needs of a theoretical problem solver."

Any careful reader would have found the incongruence between 'after' and 'first' in the phrase 'After the Weierstrassian monstrosities were discovered, first by Takagi, in 1903, ...'! A comprehensive 'story' of the early saga of these 'monstrosities' can be found in [9], especially §271, pp. 401-4. What I meant to say was that Takagi's example was constructed about thirty years before the more famous, pedagogically popular, example of Van der Waerden[11]. My reference to the Takagi example had an 'ulterior' motive, but I was not able to

[11] I myself learned the analytical details of the 'existence of everywhere-continuous, nowhere differentiable functions' from Landau's classic, elementary textbook on 'Differential and Integral Calculus' ([12], Theorem 100, p. 73). Landau, ironical as ever, stated in the 'Preface To

develop it further at the time I wrote [31], simply because I was not technically equipped to do it. Work I have done since then, in particular to try to give a computable solution and interpretation to the 'Finance Function' in [4] (pp. 451-2 & Figure 2), built up from a 'saw-tooth' like time-paths of the inventories of the relevant variables, all of which had been assumed to be integers or rational numbers.

In essence I was looking for a way to understand the computability of a discontinuous function that was built up from a mapping of rational variables to rational variables. Till about one year ago, I felt the only way to handle such formulations was to view them as Diophantine equations and, then, appeal to results on the *negative solution to Hilbert's Tenth Problem*.

I knew that the Takagi function can be shown to map rationals to rationals; I was also aware - as many have been, for years - that the Takagi function is an example of a 'Weierstrassian monstrosity' – i.e., it is provably *a continuous-everywhere, differentiable nowhere function*. Now, the Takagi function is as follows:

$$\tau(x) := \sum_{i=1}^{\infty} \int_0^x r_k(t)\, dt$$

Where:
$x \in [0, 1]$
$\forall k, r_k(t) = (-1)^{\lfloor 2^k t \rfloor}$ is the kth *Rademacher function*[12].

The Rademacher function, in turn is a kind of sequence of step functions, the kind that appears in the remarkable Clower-Howitt analysis of the Transactions Theory of the Demand for Money. Now, the Takagi function maps rationals to rationals, viz:

Theorem 1 *If* $x \in \mathbb{Q}$, *then* $\tau(x) \in \mathbb{Q}$

I had to understand how to interpret computability of the discontinuous, step-like, Rademacher function, so that I could find a way to apply it to the Clower-Howitt 'Finance Function' - and, thereby, crack the eternal nut of rational valued economic mappings, avoiding the conundrums of Diophantine Decision problems. It was at this point, a few months ago, I discovered the remarkable work of what I shall call, for want of a better name, the 'Yasugi school' at Kyoto Sangyo University (cf., for e.g., [36]). They had applied the framework

The (First) German Edition', (op.cit, p.2):

> Some mathematicians may think it unorthodox to give as the second theorem after the definition of the derivative, Weierstrass' theorem on the existence of functions which are continuous everywhere but differentiable nowhere. To them I would say that while there are very good mathematicians who have never learned any proof of that theorem, it can do the beginner no harm to learn the simplest proof to date right from his textbook, and it may serve as a useful illustration which will enhance his understanding of the concept of derivative.

[12] An excellent introduction to *Rademacher functions* can be found in [5], §8.6.

that had been developed by Pour-El & Richards ([19]) to interpret the computability of discontinuous functions, embedded in a Banach Space. It is here that approximation enters digital computation quite precisely and allows one to define the computability of discontinuous functions. The main idea is to consider a function – even, indeed, especially, discontinuous ones – as computable if they can be *effectively approximated* by a computable sequence of continuous functions, with respect to the *norm* of the space.

In a series of serendipities, I am now able to complete one key aspect of the research program in Algorithmic Mathematical Economics. This is the need to constrain economic variables to the domain and range of rational numbers, while making it possible to interpret economically plausible mappings computably. When I chose to mention the Takagi function, instead of referring to the more 'popular' Van der Waerden example, as a repository of Weierstrassian monstrosities, my hunch was that it was going to provide the key links between, approximations, discontinuities – displayed by step-like functions – their possible computabilities, due to the place occupied by the Rademacher functions in the definition of the Takagi function and the desirable property of $\tau(x)$ mapping rationals to rationals.

In constructive mathematics, particularly of the Brouwerian variety, all constituent functions are, *ab initio*, continuous. But this is not so in computable analysis. It is this 'wedge' that seemed impossible to crack and, therefore, I chose refuge in the indeterminacies of Diophantine Decision Problems. By means of a deft use of, and exploiting the power of, *rigorous approximation theory in effective contexts*, Algorithmic Mathematical Economics gains an enormous richness.

4 Whither Mathematical Economics

> "Mathematical methods are more fashionable than ever before. Witness the surge of interest in mathematical logic, mathematical biology, *mathematical economics*, mathematical psychology – in mathematical investigations of every sort. The extent to which many of these investigations are premature or unrealistic indicates the deep attraction mathematical exactitude holds for the contemporary mind."
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> [2], p. vii; italics added.

I have not discussed the issue of 'proof' and the validity or relevance of various methods of 'proof' in orthodox mathematical economics. I wish to take a categorical stance on this aspect, as far as the methodology of Mathematical Economics in the Algorithmic Mode is concerned. These are issues dealt with in the penetrating, but concise, contributions by Dr Stephen Kinsella and Professor Cassey Lee, both implicitly and explicitly.

Stefano Zambelli has observed, correctly and with much generosity, the path from my reliance on recursion theory to one, more recently, where my inclinations are more towards constructive mathematics, to define and characterise conputable economics. I am not sure my reasons are technical. I think one of the reasons for this subtle shift, taken over a period of several years, has been an increasing awareness that the Church-Turing Thesis is unnecessary as a starting point for algorithmic mathematics. A second, and more important reason, may well be more specifically epistemological. I have come to realise the importance of Brouwer's *Choice Sequences* and his intuitive concept of the *Ideal Mathematician*; this, coupled to a Husserlian phenomenological vision, feels a more comfortable as an epistemological position for an economist who wants to dethrone the kind of teleology that is pervasive in economic theory, especially in its mathematical mode.

Moreover, I have come to believe that economists have no business proving anything in any way except by means of methods sanctioned by Brouwerian constructivists, underpinned by intuitionism. At the height of the Brouwer-Hilbert controversies, in the late 1920s and early 1930s, there were attempts by Hilbertian proof theorists to show that:

> Is it possible to prove that all proofs of existence claims, with the exception of those obtained by means of the axiom of choice, can be shown to rely on an implicit exhibition of an instance. ... A proof of the conjecture would have implied that most of classical mathematics – the only exception being those parts relying on the axiom of choice – is already 'constructive'.
>
> [13], p.350

Of course, the conjecture was never 'proved'. If this hopeless effort is not enough evidence to dissuade orthodox mathematical economists from continuing their theological proof activities, then I don't know what will be persuasive. 'Classical mathematics' tries, in vain, to become 'constructive mathematics'. The orthodox mathematical economist continues, with princely unconcern for the meaning and relevance of methods of proof, using mysterious methods of demonstrating the existence of algorithmically undefinable, uncomputable, entities. These entities are, then, harnessed for use in momentous policy debates, as if they have a material existence, and not just a phantom appearance in the minds of orthodox mathematical economists.

Even more importantly – as I have now come to think and believe – mathematical economists, as part of their routine graduate training should, therefore, be taught, not only technique but also the meaning and relevance of valid methods of proof. How many orthodox mathematical economists have ever been taught that the Bolzano-Weierstrass theorem is underpinned by undecidable disjunctions and, hence, any theorem depending on it cannot be algorithmized. If they were taught this fact, would they have any reason to be fascinated by equilibrium existence proofs? Eschewing *reductio ad absurdum*, the law of double negation or the *tertium non datur* is not a matter of aesthetic sensibilities – although that aspect, too, is important; but it is a serious question of practical relevance, when used indiscriminately to derive momentous policy-pregnant propositions.

I shall end my ‘reflections’ by making an almost propaganda-like case for mathematical economics in the algorithmic mode.

An advanced text book on *Diffusions, Markov Processes and Martingales* ([21], p.1), defines, on the first page, Brownian motion:

Definition 2 *A real-valued stochastic process* $\{B_t : t \in \mathbb{R}^+\}$ *is a Brownian motion if it as the properties*

1. $B_0(\omega) = 0, \forall \omega$;
2. the map $t \mapsto B_t(w)$ is a continuous function of $t \in \mathbb{R}^+$ for all ω;
3. for every $t, h \geqslant 0, B_{t+h} - B_t$ is independent of $\{B_u : 0 \leqslant u \leqslant t\}$, and has a Gaussian distribution with mean 0 and variance h.

The authors, then, go on to ask, and give four answers to the question, ’Why study it?’ (*ibid, p.1)*:

> "(i) Virtually every interesting class of processes contains Brownian motion – Brownian motion is a martingale, a Gaussian process, a Markov process, a diffusion, a Lévy process, ...;
> (ii) Brownian motion is sufficiently concrete that one can do explicit calculations, which are impossible for more general objects;
> (iii) Brownian motion can be used as a building block for other processes Indeed, a number of the most important results on Brownian motion state that the most general process in a certain class can be obtained from Brownian motion by some sequence of transformations);
> (iv) last, but not least, Brownian motion is a rich and beautiful mathematical object in its own right."

Suppose I now defined a Turing Machine and made the same four claims and ask an economist to choose between the two formulations?[13] Which of the two formulations should the economist choose, for modelling economic processes? What kind of considerations should the economist take into account before deciding for one or the other of the two formulations to model economic phenomena? I would urge the economist, in his or her deliberation phase to remember the nature of the domain and range over which economic variables can, at best, be defined. I would also urge the economist to keep in mind the results of the previous section on the Takagi function, which, like Brownian motion, is a Weierstrassian monster - i.e., *continuous-everywhere, differentiable nowhere*. I would also plead with the economist to read carefully the considered opinion of an outstanding applied mathematician, who was also one of the pioneers of mathematical finance theory, Maury Osborne ([15]):

[13] To be quite rigorous and complete, I should add the usual formal definition of a formal dynamical system (cf.,[3] or [7], pp.159-160) and consider the ‘coupled’ system of the Turing Machine and the formal dynamical system. Such a coupled system can satisfy the four claims as easily, if not also capable of displaying itself as a more ‘beautiful mathematical object in its own right’.

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

[15], p.34; italics added.

In other words, Osborne emphasizes, in a whimsical way, the fact that economic and financial data can only be graphed as step-functions. What of continuity, then? What are the domains and ranges of typical economic and financial variables?

Even more interestingly, another renowned applied mathematician, who was also a pioneer information theorist, made some observations that almost summarise my discussions. I shall, therefore, quote extensively, to complete this 'manifesto', invoking Richard Hamming's powerful summary:

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

What are all these uncountably many non-computable numbers that the conventional real number system includes?....

The intuitionists, of whom you seldom hear about in the process of getting a classical mathematical education, have long been articulate about the troubles that arise in the standard mathematics, have long been articulate about the troubles that arise in the standard mathematics, including the paradoxes, in the usual foundations of mathematics. One such is the Skolem-Löwenheim paradox which asserts that any finite number of postulates has a realization that is countable. This means that no finite number of postulates can uniquely characterise the accepted real number system. .... Again, the Banach-Tarski paradox, ... suggests that *we must be wary of using such kinds of mathematics in many real world applications including probability theory. These statements warn us that we should not use the classical real number system without carefully thinking whether or not it is appropriate for new applications to probability.* ....

What are we to think of this situation? What is the role in *probability theory* for these numbers which can never occur in practice?"

[6], pp. 90-1; italics added.

How could the economist not opt for the algorithmic alternative?

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