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TOWARDS AN ALGORITHMIC *REVOLUTION* IN ECONOMIC THEORY

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Abstract. This is an attempt to tell a coherent story of a possible path towards an algorithmic revolution in economic theory, based on foundational debates in mathematics. First, by exposing the non-computational content of classical mathematics, and its foundations, it is shown that both *set theory* and the *tertium non datur* can be dispensed with, as foundational concepts. Next, then, as a natural sequel, it follows that every kind of economic theory that bases its theoretical underpinning on classical mathematics can be freed from these foundations and can be made naturally algorithmic. This will make the subject face, in an all pervasive way, absolutely (algorithmically) undecidable decision problems. The thrust of the path towards an algorithmic revolution in economics lies, then, in pointing out that only a radically new mathematical vision of microeconomics, macroeconomics, behavioural economics, game theory, dynamical systems theory and probability theory can lead us towards making economic theory a meaningfully applied science and free of mysticism and subjectivism.

Keywords. Algorithmic economics; Computable economics; Diophantine decision problems; Mathematical proof

1. A Foundational Preamble

> *Hilbert's vision of a universal algorithm* to solve mathematical theorems[1] required a unification of Logic, Set Theory and Number Theory. This project was initiated by Frege, rerouted by Russell, repaired by Whitehead, derailed by Gödel, restored by Zermelo, Frankel, Bernays and von Neumann, shaken by Church and *finally demolished by Turing*. Hence, to say that the interest in algorithmic methods in mathematics or the progress in logic was engendered by the computer is wrong way around. For these subjects it is more correct to observe *the revolution in computing that was inspired by mathematics*.
>
> Cohen (1991, p. 324; italics added)

It is in the above sense – of a 'revolution in computing that was inspired by mathematics' – that I seek to advocate an 'algorithmic revolution in economic theory inspired by mathematics'. I have argued elsewhere, (see Velupillai, 2010a), that a strong case can be made to the effect that 'the revolution in computing was inspired by mathematics' – more specifically by the *debates in the foundations of mathematics*, in particular those brought to a head by the *Grundlagenkrise* (see Section 3, below for a partial summary, in the context of the aims of this paper). Although this debate had the unfortunate by-product of 'silencing' Brouwer, *pro tempore*, it did bring about the 'derailing by Gödel', the 'shaking by Church' and the 'final demolition by Turing' of the Hilbert project of a *Universal Algorithm* to solve all mathematical problems.[2]

Before I proceed any further on a 'foundational preamble', let me make it clear that the path *towards an algorithmic revolution in economics* is not envisaged as one on Robert Frost's famous '*Roads Not Taken*'. Algorithmic behavioural economics, algorithmic statistics, algorithmic probability theory, algorithmic learning theory, algorithmic dynamics and algorithmic game theory[3] have already cleared the initial roughness of the path for me.

In what sense, and *how*, did 'Turing demolish Hilbert's vision of a universal algorithm to solve mathematical [problems]'? – in the precise sense of showing, via the recursive unsolvability of the *Halting Problem for Turing Machines*, the impossibility of constructing any such universal algorithm to solve any given mathematical problem. This could be viewed as an example of *a machine demonstrating the limits of mechanisms*, but I shall return to this theme in the next section.

The resurgence of interest in *constructive mathematics*, at least via a far greater awareness that one possible rigorous – even if not entirely practicable – definition of an algorithm is in terms of its equivalence with a *constructive proof*, may lead to an alternative formalization of economic theory that could make the subject intrinsically algorithmic.

Equally inspirational, from what may, with much justification, be called a resurgence of interest in the possibilities for 'new' foundations for mathematical analysis – in the sense of replacing the 'complacent' reliance on set theory, supplemented by ZFC (Zermelo-Frankel plus the Axiom of Choice) – brought about by the development of *category theory* in general and, in particular, a category called a *topos*. The underpinning logic for a *topos* is entirely consistent with *intuitionistic logic* – hence no appeal is made to either the *tertium non datur* (the *law of the excluded middle*) or the *law of double negation* in the proof procedures of *topoi.*[4] This fact alone should suggest that *categories* are themselves intrinsically computational in the sense of constructive mathematics and the implications of such a realization is, I believe, exactly encapsulated in Martin Hyland's enlightened call for at least a 'radical reform' in mathematics education:

> Quite generally, the concepts of classical set theory are inappropriate as organising principles for much modern mathematics and *dramatically so for computer science*. The basic concepts of *category theory* are very flexible and prove more satisfactory in many instances.....[M]uch *category theory is essentially computational* and this makes it particularly appropriate to the conceptual demands made by computer science........[T]he IT revolution has transformed [category theory] into a serious form of applicable mathematics. In so doing, it

has revitalized logic and foundations. The old complacent security is gone. *Does all this deserve to be called 'a revolution' in the foundations of mathematics*? If not maybe it is something altogether more politically desirable: *a radical reform*.

J.M.E. Hyland (1991, pp. 282–3; italics and quotes added)

As for independence from any reliance on the *tertium non datur*, this is a desirable necessity, not an esoteric idiosyncrasy, in any attempt to algorithmize even orthodox economic theory – whatever definition of algorithm is invoked. For example, the claims by computable general equilibrium theorists that they have devised a constructive algorithm to compute the (provably uncomputable) Walrasian equilibrium *is false* due to an appeal to the Bolzano–Weierstrass theorem[5] which, in turn, relies on an undecidable disjunction (i.e. an appeal is made to the *tertium non datur* in an infinitary context).

To place this last observation in its proper historical context, consider the following. There are at least 31 propositions[6] in Debreu's *Theory of Value* (Debreu, 1959), not counting those in chapter 1, of which the most important[7] are theorems 5.7 (existence of equilibrium), 6.3 (the 'optimality' of an equilibrium) and 6.4[8] (the 'converse' of theorem 6.3). None of these are algorithmic and, hence, it is impossible to implement their proofs in a digital computer, even if it is an ideal one (i.e. a Turing Machine, for example). As a matter of fact *none* of the proofs of the 31 propositions are algorithmic. On the contrary, there are at least 22 propositions in Sraffa (1960), and all – except possibly one – are endowed with algorithmic[9] proofs (or hints on how the proofs can be implemented algorithmically). In this sense one can refer to the theory of production in this slim classic as an algorithmic theory of production. The book, and its propositions are rich in algorithmic – hence, numerical and computational – content.[10]

Contrariwise, one can refer to the equilibrium existence theorem in Debreu (1959) as an uncomputable general equilibrium and *not* a Computable General Equilibrium (CGE) model; one can go even further: it is an *unconstructifiable* and *uncomputable* equilibrium existence theorem (cf. Velupillai, 2006, 2009). There is no numerical or computational content in the theorem.

Greenleaf (1991) summarized the 'insidious' role played by the *tertium non datur* in mathematical proofs[11]:

Mathematicians use algorithms in their proofs, and many proofs are totally algorithmic, in that the triple [*assumption, proof, conclusion*] can be understood in terms of [*input data, algorithm, output data*]. Such proofs are often known as *constructive*, a term which provokes endless arguments about ontology.

To 'understand' [any mathematical] theorem 'in algorithmic terms', represent the assumptions as *input data* and the conclusion as *output data*. Then try to convert the proof into an algorithm which will take in the input and produce the desired output. If you are unable to do this, it is probably because the proof relies essentially on the *law of the excluded middle*.

Greenleaf (1991, pp. 222–3; quotes added)

What is the point of mathematizing economic theory in non-numerical, computationally meaningless, mode, as practised by Debreu and a legion of his followers – and all and sundry, of every school of economic thought? Worse, what is the justification of then claiming computational validity of theorems that are derived by non-constructifiable, uncomputable, mathematical formalisms?

The most serious and enduring mathematization of economic theory is that which took place in the wake of von Neumann's two pioneering contributions, in 1928 and 1938 (von Neumann, 1928, 1938), and in the related, *Hilbert-dominated*, mathematical activities of that period. To be sure, there were independent currents of mathematical economic trends – outside the confines of game theory and mathematical microeconomics – that seemed to be part of a *zeitgeist*, at least viewed with hindsight. Thus the germs and the seeds of the eventual mathematization of macroeconomics, the emergence of econometrics, and the growth of welfare economics and the theory of economic policy, in various *ad hoc* mathematical frameworks, were also 'planted' in this period.

Yet, in spite of all this, what has come to be the dominant mathematical methodology in economic theorizing is the intensely non-algorithmic, non-constructive, uncomputable one that is – strangely – the legacy of von Neumann. 'Strangely', because, after all, von Neumann is also one[12] of the pioneering spirits of the stored program digital computer. By aiming towards an algorithmic revolution in economic theory I am suggesting that there is much to be gained by shedding this legacy and its iron clasps that tie us to a non-numerical, computationally vacuous, framework of mathematical theorizing in economics.

What exactly is to be gained by this suggestion of adopting an alternative, algorithmic, mathematical framework for economic theorizing? There are at least three answers to this question. First, from an epistemological point of view, one is able to be precise about the limitations of mechanisms that can underpin knowledge, its acquisition and its utilization. Secondly, from a philosophical point of view, it will enable the economic theorist to acknowledge the limits to mathematical formalization and, hopefully, help return the subject to its noble humanitarian roots and liberate itself from the pseudo-status of being a branch of pure mathematics. Thirdly, methodologically, the algorithmic framework will not perpetuate the schizophrenia between an economic theorizing activity that is decidedly non-constructive and uncomputable and an applied, policy oriented, commitment that requires the subject to be uncompromisingly numerical and computational.

With these issues in mind, the paper is structured as follows. In the next section, largely devoted to definitional issues, an attempt is made to be precise about the relevant concepts that should play decisive roles in an algorithmic economics. Section 3 is devoted to an outline of the background to what I have called the von Neumann legacy in mathematical economic theorizing and the eventual, regrettable, dominance of 'Hilbert's Dogma' over Brouwer's algorithmic visions. There is much excitement about something called algorithmic game theory, these days; this is the obverse of computable general equilibrium theory. Both are plagued by the schizophrenia of doing the theory with one kind of mathematics and trying to compute the uncomputable and construct the non-constructive with another kind of mathematics. But there is also a genuinely alogorithmic statistics, free of schizophrenia, up to a point. And, there is also the noble case of classical behavioural economics, from the outset uncompromisingly algorithmic in the sense of computability theory. These issues are the subject matter of Section 4, the concluding section. It is also devoted to an outline of how I think *we should educate* the current generation of graduate students in economics so that they can become the harbingers of the algorithmic revolution in economics.

2. Machines, Mechanisms, Computation and Algorithms

> There are several different ways of arriving at [the precise definition of the concept of *finite procedure*], which, however, all lead to exactly the same concept. The most satisfactory way, in my opinion, is that of reducing the concept of finite procedure to that of a machine with a finite number of parts, as has been done by the British mathematician *Turing*.
>
> Gödel (1951/1995), pp. 304–5; italics added.

I should have included a fifth concept, *mind*, to the above quadruple, machines, mechanisms, computation and algorithms, especially because much of the constructive and computable basis for the discussion in this section originates in what Feferman (2009, p. 209) has called Gödel's dichotomy:

> [I]f the human mind were equivalent to a finite machine, then objective mathematics not only would be incompleatable in the sense of not being contained in any well-defined axiomatic system, but moreover there would exist *absolutely* unsolvable diophantine problems .... where the epithet 'absolutely' means that they would be undecidable, not just within some particular axiomatic system, but by *any* mathematical proof the human mind can conceive. So the following disjunctive conclusion is inevitable: *Either mathematics is incompleatable in this sense, that its evident axioms can never be compromised in a finite rule, that is to say the human mind (even within the realm of pure mathematics) infinitely surpasses the powers of any finite machine, or else there exist absolutely unsolvable Diophantine problems* ... (where the case that both terms of the disjunction are true is not excluded, so that there are, strictly speaking, three alternatives).
>
> Gödel (1951/1995, p. 310; italics in the original)

It goes without saying that I subscribe to the view that 'there exist absolutely unsolvable Diophantine problems', especially because I have maintained that Diophantine decision problems are pervasive in economics, from the ground up: basic supply–demand analysis, classical behavioural economics, economics dynamics and game theory. The nature of the data types in economics make it imperative that the natural mathematical modelling framework, from elementary supply–demand analysis to advanced decision theoretic behavioural economics, of the kind practised by Herbert Simon all his intellectual life, should be in terms of Diophantine decision problems.[13]

A 'machine[14] with a finite number of parts', in common sense terms, embodies a *mechanism*. Is it possible to envision, or imagine, a mechanism not *embodied* in a machine? This is neither a frivolous question, nor analogous to the deeper question whether the mind is embodied in the brain. I ask it because there is a respectable theory of mechanisms in economics without any implication that the economic system, its institutions or agents, are machines that embody it.

An *algorithm* is a finite procedure in the precise mathematical sense of the formalism of a *Turing Machine*, in terms of one kind of mathematics: *recursion* or *computability* theory. The 'several different ways of arriving at the precise definition of the concept of finite procedure', which all lead to 'the same concept' is summarized in the form of the *Church–Turing thesis*.

But there is another kind of mathematics, *constructive* mathematics, where finite proof procedures encapsulate, rigorously, the notion of an algorithm.[15] However, in constructive mathematics there is *no* attempt, formally or otherwise, to work with a 'precise definition of the concept of a finite procedure'. Yet:

> The interesting thing about [Bishop's *Constructive* mathematics] is that it reads essentially like ordinary mathematics, yet *it is entirely algorithmic in nature* if you look between the lines.[16]
>
> Donald E. Knuth (1981, p. 94)

However, unlike recursion theory with its non-embarrassment of reliance on classical logic and free-swinging set theoretic methods,[17] the constructive mathematician's underlying logic satisfies first-order *intuitionistic* or *constructive* logic and hence explicitly denies the validity of the *tertium non datur*. This elegant philosophy of a mathematics where the *proof-as-algorithm* vision is underpinned by a logic free of any reliance on the *tertium non datur* should be contrasted with the ruling mathematical paradigm based on *Hilbert's Dogma* – i.e. *proof-as-consistency* = *existence* – and unrestricted appeal to the *tertium non datur*.

The question of why economic theory, in its mathematical mode, shunned the proof-as-algorithm vision, underpinned by an intuitionistic logic, is addressed in the next section. Here my restricted aim is only to outline the implications of adopting the proof-as-algorithm vision, coupled to an adherence to intuitionistic logic, from the point of view of formal notions of *machines*, *mechanisms* and *computation*, thus linking it with computability theory, even if their underpinning logics are different. I believe that considerations of such implications are imperative for a sound mathematical basis for the path towards an algorithmic revolution in economics is to be constructed. This is especially so because the existing successes on paving paths towards algorithmic revolutions in probability, statistics, learning, induction and dynamics are based, almost without exception, on the foundations of computability theory in its recursion theoretic mode.

I want to ask four questions: *what* are *machines*, *mechanisms*, *computations* and *algorithms*? *How* interdependent are any answers to the questions? What are the *limitations* of mechanisms? Can a machine, encapsulating mechanisms, *know* its limitations. I ask these questions – and seek answers – in the spirit with which Warren McCulloch asked and answered his famous *experimental* epistemological question: *What is a Number, that a Man May Know It, and a Man, that He May Know a Number* (McCulloch, 1961/1965).

Before I continue in the 'McCulloch mode', finessed (I hope) by *Kant's deeper questions* as a backdrop to my suggested answers, two apparently 'simple', almost straightforward, questions must be faced squarely: '*What is a Computation?*' and '*What is an Algorithm?*'

The first of these two questions, 'What is a Computation?', was answered with exceptional clarity and characteristic depth and conviction, in the spirit and philosophy with which this essay is written, by that modern master of computability theory: Martin Davis. His elegant answer to the question is given in Davis (1978) (and my embellishment to that answer is detailed in Velupillai and Zambelli (2010)):

> What Turing did around 1936 was to give a cogent and complete logical analysis of the notion of 'computation'. Thus it was that although people have been computing for centuries, it has only been since 1936 that we have possessed a satisfactory answer to the question: 'What is a computation'?.....
>
> Turing's analysis of the computation process led to the conclusion that it should be possible to construct 'universal' computers which could be programmed to carry out any possible computation. The existence of a logical analysis of the computation process also made it possible to show that certain mathematical problems are incapable of computational solution, that they are, as one says, undecidable.
>
> Davis (1978, pp. 241–242; italics in the original)

At this point I could answer the second question – '*What is an Algorithm*'? – simply by identifying it with the (computer) *programme* which *implements* a computation on a (Universal) Turing Machine, but I shall not do so.[18] However, instead of the 'programme-as-algorithm' paradigm, I shall choose the 'proof-as-algorithm' route for a definition, mainly because my ultimate aim is a basis for economic theory in constructive mathematics.

In a series of important and exceptionally interesting – even with an unusual dose of humour, given the depth of the issues discussed in them – articles, Yiannis Moschovakis (1998, 2001) and Moschovakis and Paschalis (2008, p. 87) have proposed an increasingly refined, set-theoretic, notion of algorithm, with the aim 'to provide a traditional foundation for the theory of algorithms, . . . within axiomatic set theory on the basis of the set theoretic modelling of their basic notions'. In my reading, and understanding, of this important line of research, it is closely related to the attempt in Blum *et al.* (1998), where algorithms are defined within a 'model of computation which postulates exact arithmetic on real numbers'. Because my twin aims are to found a notion of algorithms consistent with constructive mathematics and its proof-as-programme vision, underpinned by an intuitionistic logic which eschews any reliance on the *tertium non datur*, I shall by-pass this path towards a definition of a mathematical notion of algorithm. Moreover, I would also wish to respect the natural data-types that we are faced with in economics, in any definition of algorithms, and, hence, seek also some sort of *modus vivendi* with the notion that arises in recursion theory.

I am, on the other hand, somewhat relieved that the view of algorithms-as-(constructive) proofs is not entirely dismissed by Moschovakis, even if he does have serious doubts about any success along this path. His views on this matter are worth quoting in some detail, for they are the path I think economists should choose, if we are to make the subject seriously algorithmic with a meaningful grounding also in computability theory. In subsection '3.4 (IIb) Algorithms as Constructive Proofs', Moschoavakis (1998, pp. 77–78; italics in the original), points out that:

> Another, more radical proposal which also denies independent existence to algorithms is the claim that *algorithms are implicitly defined by constructive proofs* ......
>
> Although I doubt seriously that algorithms will ever be eliminated in favour of constructive proofs (or anything else for that matter), I think that this view is worth pursuing, because it leads to some very interesting problems. With specific, precise definitions of algorithms and constructive proofs at hand, one could investigate whether, in fact, every algorithm can be extracted (in some concrete way) from some associated, constructive proof. Results of this type would add to our understanding of the important connection between *computability and constructivity*.

In an 'aside' to the above observation, as a footnote, Moschovakis also points out that there is the possibility simply to 'define "algorithm" to be [a] constructive proof', but goes on to remark that he 'cannot recall seeing this view explained or defended'. It is this view that I subscribe to, especially because it is in line with the way, for example, Bishop (1967) is written, as observed by Knuth (1981), which I have quoted earlier in this section.

In passing, it may be apposite to point out that Moschovakis (2001, p. 919, footnote 2) refers to Knuth's monumental work on *The Art of Computer Programming* (Knuth, 1973) as 'the only standard reference [he knows] in which algorithms are defined where they should be, in Sect.1.1'. Somehow, Moschovakis seems to have overlooked Knuth's handsome acknowledgement (Knuth, 1973, p. 9) that his – i.e. Knuth's 'formulation [definition of algorithms] is virtually the same as that given by A.A. Markov in 1951, in his book *The Theory of Algorithms*'. This is doubly interesting, in the current context. First of all, Markov 'defines' algorithms even before 'Sect. 1.1' of his book, in fact in the Introduction to his classic book. Secondly, Markov endorses, although at that embryonic stage of the resurgence of constructive foundations for mathematics it could only have been a 'hope', the nexus 'algorithms' – constructive proof quite explicitly (Markov, 1954/1961):

> The entire significance for mathematics of rendering more precise the concept of algorithm emerges, however, in connection with the problem of a constructive foundation for mathematics. On the basis of a more precise concept of algorithm one may give the constructive validity of an arithmetical expression. On its basis one may set up also a constructive mathematical logic – a constructive propositional calculus and a constructive predicate calculus. Finally, the main field of application of the more precise concept of algorithm will undoubtedly be constructive analysis – the constructive theory of real numbers and functions of a real variable, which are now in a stage of intensive development.

These Markovian thoughts and suggestions were the embryonic *algorithmic visions* from which what came to be called *Russian Constructive Mathematics* (cf. chapter 3 of Bridges and Richman, 1987) and the influential work of Oliver Aberth (1980, 2001) emerged.[19]

I return now to the spirit of Warren McCulloch, deepened by Kant's famous themes. Kant's deeper question was: *What is man*, which he then proceeded to answer by subdividing it into three more limited queries: *What can I know*? *What must I do*? *What may I hope*? If I substitute, not entirely fancifully, machine for man, in Kant's question, then, the issues I try to discuss in terms of McCulloch's epistemological vision, must come to terms with at least the following: *What can a machine know about the limitations of the mechanisms it embodies*? The answer(s) depend crucially on Gödel's incompleteness theorems, the Turing Machine and Turing's famous result on the Unsolvability of the Halting Problem for Turing Machines.

However, in terms of *any* mathematical formalism, validity of mathematical theorems are claimed on the basis of *proof*, which are, in turn, the only *mechanism* for expressing *truth* effectively – in the precise sense of recursion theory – in mathematics. Then, with Kant:

- The mathematician can *hope* all *provable* mathematical statements are *true*;
- Conversely, the mathematician can also *hope* that all – and only – the true statements are provable;
- And, following Hilbert's vision, the mathematician's task – Kant's 'what must I do' – is to build a machine to *discover* – Kant's 'what must I know' – valid proofs of every possible theorem in any given formal system.

The first two hopes were 'derailed' by Gödel's incompleteness theorems, by the demonstration that in any reasonably strong formal system there are *effectively* presentable mathematical statements that are *recursively undecidable* – i.e. neither *algorithmically* provable nor unprovable. The third was 'shaken by Church and *finally demolished by Turing*', i.e. that no such machine can be 'built', shown in a precisely effective way.

Because, however, Gödel's theorems were presented recursively and proved constructively – hence within the proof-as-algorithm paradigm – it must be possible to build a *machine*, with an effective *mechanism*, to *check* the validity of the existence of *undecidable* statements. This, then, will be an instance of a mechanical verification of Gödel's proof and, hence, a demonstration that *a machine can establish the limitations of its own mechanism*.[20]

This is where computation, computability theory and constructive mathematics intersect and interact felicitously, via the Turing Machine, to unify the four notions of machines, mechanisms, computations and algorithms. The mechanism encapsulated in the Turing Machine implements the effective (finite) procedure that is an algorithm in its proof-as-algorithm role. Finally, because all the effectivizations are in terms of Gödel's arithmetization – i.e. via Gödel numberings – the implementations are all in number-theoretic terms and, thus, within the domain of computability theory.

What exactly is such a mechanism? And, given any such mechanism, does it have a universal property? By this is meant whether there are effectively definable – and constructible – alternative mechanisms, in machine mode or otherwise, that are as 'powerful' in some precise sense? For example, is there a closure property such that all calculable number-theoretic functions can be evaluated by one such mechanism? The answer to this question is given by the Church–Turing thesis (cf. Velupillai, 2000, for a precise statement of this notion).

Surely, the obvious question an economist should ask, in view of these results, is the following: if the economic system is a mechanism, can the machine which encapsulates it demonstrate its set of undecidable statements? A frontier topic in economics, particularly in its mathematical mode and policy design variants, is *mechanism theory*. Strangely, though, mechanism theory has completely ignored the whole of the above development. *A fortiori*, therefore, the *Limitations of Mechanisms*, whether in thought processes, which lead up to theory building – in the sense in which Peirce used the term *abduction* or *retroduction* – or in the actual analysis of so-called *economic mechanisms*, are not explicitly considered.[21]

3. The Legacy of *Hilbert's Dogma* in Mathematical Economics

> [Hilbert] won politically. . . . . Brouwer was devastated, and his active research career effectively came to an end.
>
> [Hilbert] won mathematically. Classical mathematics remains intact, intuitionistic mathematics was relegated to the margin. . . . . .
>
> And [Hilbert] won polemically. Most importantly . . . Hilbert's agenda set the context of the controversy both at the time and, largely, ever since. Carl J. Posy (1998, pp. 292–293)

Suppose economics, in particular game theory, had been mathematized, say by von Neumann, in 1928 (von Neumann, 1928), in the *constructive* mode that was being vigorously advocated by Brouwer just in those years; or, in terms of *recursion theory*, which came into being, as a result of the pioneering works by Gödel, Church, Turing, Post, Rosser and Kleene, just as von Neumann's growth model (von Neumann, 1938) was made known to the wider mathematical and economics academic world, in 1936. What would we now, some eighty years later, be teaching as mathematics for economics to our graduate students?

To answer this obviously counterfactual question, let me backtrack a little, but on the basis of a strangely unscholarly remark made in a recent, respectable, almost encyclopaedic tract on *Real Analysis with Economic Applications* (Ok, 2007):

> It is worth noting that in later stages of his career, he [Brouwer] became the most forceful proponent of the so-called intuitionist philosophy of mathematics, which not only forbids the use of the Axiom of Choice but also rejects the axiom that a proposition is either true or false (thereby disallowing the method of proof by contradiction). The consequences of taking this position are dire. For instance, an intuitionist would not accept the existence of an irrational number! In fact, in his later years, Brouwer did not view the Brouwer Fixed Point Theorem as a theorem. (he had proved this result in 1912, when he was functioning as a 'standard' mathematician).
>
> If you want to learn about intuitionism in mathematics, I suggest reading – *in your spare time, please* – the four articles by Heyting and Brouwer in Benacerraf and Putnam (1983).
>
> Efe. A. Ok (2007), p. 279; italics added.

The von Neumann (1928) paper introduced, and etched indelibly, to an unsuspecting and essentially non-existent Mathematical Economics community and tradition what has eventually come to be called Hilbert's Dogma,[22] 'consistency $\Leftrightarrow$ existence'. This became – and largely remains – the mathematical economist's credo and hence, the resulting inevitable schizophrenia of 'proving' existence of equilibria first, and looking for methods to *construct* or *compute* them at a second, entirely unconnected, stage. Thus, too, the indiscriminate appeals to the *tertium non datur* – and its implications – in 'existence proofs', on the one hand, and the ignorance about the nature and foundations of constructive mathematics or recursion theory, on the other.

But it was not as if von Neumann was not aware of Brouwer's opposition to Hilbert's Dogma, even at that early stage, although there is reason to suspect – given the kind of theme I am trying to develop in this paper – that something peculiarly 'subversive' was going on. Hugo Steinhaus observed, with considerable perplexity (Steinhaus, 1965, p. 460; italics added):

> [My] inability [to prove the minimax theorem] was a consequence of the ignorance of Zermelo's paper in spite of its having been published in 1913..... J von Neumann was aware of the importance of the minimax principle [in vonNeumann (1928)]; it is, however, *difficult to understand the absence of a quotation of Zermelo's lecture in his publications*.

Why did not von Neumann refer, in 1928, to the Zermelo-tradition of (alternating) arithmetical games? van Dalen, in his comprehensive, eminently readable, scrupulously fair and technically and conceptually thoroughly competent biography of Brouwer, (van Dalen, 1999, p. 636; italics added), noted, without additional comment that:

> In 1929 there was another publication in the intuitionistic tradition: an intuitionistic analysis of the game of chess by Max Euwe. It was a paper in which the game was viewed as a spread (i.e., a tree with the various positions as nodes). Euwe carried out *precise constructive estimates* of various classes of games, and considered the influence of the rules for draws. When he wrote his paper he was not aware of the earlier literature of Zermelo and Dénès König. *Von Neumann called his attention to these papers*, *and in a letter to Browuer von Neumann sketched a classical approach to the mathematics of chess, pointing out that it could easily be constructivized*.

Why did not von Neumann provide this 'easily constructivized' approach – then, or later?

Perhaps it was easier to derive propositions appealing to the *tertium non datur*, and to Hilbert's Dogma, than to do the hard work of constructing estimates of an algorithmic solution, as Euwe did?[23] Perhaps it was easier to continue using the axiom of choice *than to construct new axioms* – say the axiom of determinacy.[24] Whatever the reason, the fact remains, that von Neumann's legacy was, indisputably, a legitimization of *Hilbert's Dogma* (and the indiscriminate use of the axiom of choice in mathematical economics).

This is worth emphasizing, in the context of a discussion on an *Algorithmic Revolution in Economics*, especially because Walras and Pareto, Marshall and Edgeworth, Wicksell and Irving Fisher, strived to find methods to construct solutions than to prove existence via an appeal to consistency. Paradigmatic examples of this *genre* are, of course, *tâtonnement* as a device to solve a system of equations, the appeal to *the market as a computer* – albeit an *analogue* one – to solve large systems of equations by Pareto (and, later, taking centre stage in the *socialist calculation debate*), Irving Fisher's construction of an (analogue) hydraulic computer to measure and calibrate utility functions, and so on. I shall return to this theme in the concluding section.

It is against such a background that one must read, and not be surprised, at the kind of preposterously ignorant and false assertions in Ok's above observations and claims. These are made in a new advanced text book on mathematics for graduate (economic) students, published under the imprint of an outstanding publishing house – *Princeton University Press* – and peddled as a text treating the material it does contain 'rigorously', although the student is not warned that there are many yardsticks of 'rigour' and that which is asserted to be 'rigorous' in one kind of mathematics could be considered 'flippant' and slippery' in another kind (see van Dalen's point in footnote 22, above).

Yet, every one of the assertions in the above quote is false, and also severely misleading. Brouwer did not 'become the most forceful proponent of the so-called intuitionist philosophy of mathematics *in later stages of his career*'; he was an intuitionist long before he formulated and proved what came, later, to be called the Brouwer fix-point theorem (cf. Brouwer, 1907,[25] 1908a, b); for the record, even the fixed-point theorem came earlier than 1912. It is nonsensical to claim that Brouwer did not consider *his* 'fixed point theorem as *a theorem'*; he did *not* consider it a valid theorem in intuitionistic constructive mathematics, and he had a very cogent reason for it, which was stated with admirable and crystal clarity when he finally formulated and proved it, forty years later, *within* intuitionistic constructive mathematics (Brouwer, 1952). On that occasion he identified the reason why his original theorem was unacceptable in intuitionistic constructive – indeed, in almost any kind of constructive – mathematics, for example, in Bishop-style constructivism, which was developed without any reliance on a philosophy of intuitionism:

> [T]he validity of the Bolzano–Weierstrass theorem [in intuitionism] would make the classical and the intuitionist form of fixed-point theorems equivalent.
>
> Brouwer (1952, p. 1)

Note how Brouwer refers to a 'classical . . . form of the fixed-point theorem'. The invalidity of the Bolzano–Weierstrass theorem in any form of constructivism is due to its reliance on the law of the excluded middle in an infinitary context of choices (cf. also, Dummett, pp. 10–12). The part that invokes the Bolzano–Weierstrass theorem entails *undecidable disjunctions* and as long as any proof invokes this property, it will remain *unconstructifiable* and *non-computable*.

It is worse than nonsense – if such a thing is conceivable – to state that 'an intuitionist would not accept the existence of an irrational number'. Moreover, the law of the excluded middle is not a mathematical axiom; it is a logical law, accepted even by the intuitionists so long as meaningless – precisely defined – infinities are not being considered as alternatives from which to 'choose'[26] This is especially to be remembered in any context involving intuitionism, particularly in its Brouwerian variants, because he – more than anyone else, with the possible exception of Wittgenstein – insisted on the *independence of mathematics from logic*.

As for the un-finessed remark about the axiom of choice being forbidden, the author should have been much more careful. Had this author done his elementary mathematical homework properly, Bishop's deep and thoughtful clarifications of the role of a choice axiom in varieties of mathematics may have prevented the appearance of such nonsense (Bishop, 1967, p. 9):

> When a classical mathematician claims he is a constructivist, he probably means he avoids the axiom of choice. This axiom is unique in its ability to trouble the conscience of the classical mathematician, but in fact it is not a real source of the unconstructivities of classical mathematics. A choice function exists in constructive mathematics, because a choice is implied by the very meaning of existence.[27] Applications of the axiom of choice in classical mathematics either are irrelevant or are combined with a sweeping appeal to the principle of omniscience.[28] The axiom of choice is used to extract elements from equivalence classes where they should never have been put in the first place.

4. Reconstructing Economic Theory in the Algorithmic Mode[29]

> I am sure that the power of vested interests is vastly exaggerated compared with the gradual encroachment of ideas. Not, indeed, immediately, but after a certain interval; for in the field of economic and political philosophy *there are not many who are influenced by new theories after they are twenty-five or thirty years of age*, so that the ideas which civil servants and politicians and even agitators apply to current events are not likely to be the newest. But, soon or late, it is ideas, not vested interests, which are dangerous for good or evil.
>
> J. Maynard Keynes (1936, pp. 3833–3834; italics added)

I believe, alas, in this melancholy observation by the perceptive Keynes and I think only a new generation of graduate students can bring forth an algorithmic revolution in economics. Hence, this concluding section is partly a brief retrospective on what has been achieved 'towards an algorithmic revolution in economics' and partly a manifesto, or a program – decidedly not an algorithm – for the education of a new generation of graduate students in economics who may be the harbingers of the revolution. I do not pretend to ground my 'manifesto' for an educational effort in any deep theory of 'scientific revolution', inducement to a 'paradigm shift', and the like.

I should begin this concluding section with the 'confession' that I have not dealt with the notion of algorithm in numerical analysis and so-called 'scientific computation' in this paper. In relation to the issues raised in this paper, the most relevant reference on founding numerical analysis in a model of computation is the work of Smale and his collaborators. An excellent source of their work can be found in (Blum *et al.*, 1998). My own take on their critique of the Truing Machine Model as a foundation for 'scientific computation' is reported in (Velupillai, 2009a; Velupillai and Zambelli, 2010). It may, however, be useful – and edifying – to recall what may be called the defining theme of *Complexity and Real Computation* (Blum *et al.*, 1998, p. 10): 'Newton's Method is the "search algorithm" sine qua non of numerical analysis and scientific computation'. Yet, as they candidly point out (Blum *et al.*, 1998, p. 153; italics added): '. . . even for a polynomial of one complex variable we cannot *decide* if Newton's method will converge to a root of the polynomial on a given input'. The 'decide' in this quote refers to recursive or algorithmic decidability.

At least six 'dawns' can be discerned in the development of the algorithmic social sciences, all with direct ramifications for the path towards an algorithmic revolution in economics: algorithmic behavioural economics,[30] algorithmic probability theory, algorithmic finance theory,[31] algorithmic learning theory,[32] algorithmic statistics,[33] algorithmic game theory[34] and algorithmic economic dynamics. Yet, there is no recognisable, identifiable, discipline called algorithmic economics. Why not?

Before I try to answer this question let me clarify a couple of issues related to *Algorithmic Statistics*, *Algorithmic Game Theory*, the theory of *Algorithmic Mechanism Design* and *Algorithmic Economic Dynamics*.

To the best of my knowledge 'Algorithmic Statistics' was so termed first by Gács, Tromp and Vitányi (Gács *et al.*, 2001, p. 2443; italics and quotes added):

> While Kolmogorov complexity is the expected absolute measure of information content of an individual finite object, a similarly absolute notion is needed for the relation between an individual data sample and an individual model summarizing the information in the data, for example, a finite set (or probability distribution) where the data sample typically came from. *The statistical theory based on such relations between individual objects can be called 'algorithmic statistics'*, in contrast to classical statistical theory that deals with relations between probabilistic ensembles.

Algorithmic statistics, still an officially young field, is squarely founded on recursion theory, but not without a possible connection with intuitionistic or constructive logic, at least when viewed from the point of view of Kolmogorov complexity and its foundations in the kind of frequency theory that von Mises tried to axiomatize. This is a chapter of intellectual history, belonging to the issues discussed in Section 2, above, on the (constructive) proof-as-program vision, with an underpinning in intuitonistic logic. An admirably complete, and wholly sympathetic, account of the story of the way an algorithmic foundations for the (frequency) theory of probability was subverted by orthodoxy wedded to the Hilbert Dogma is given in van Lambalgen (1987).

I began to think of Game Theory in algorithmic modes – i.e. *Algorithmic Game Theory* – after realizing the futility of algorithmizing the uncompromisingly subjective von Neumann–Nash approach to game theory and beginning to understand the importance of Harrop's theorem (Harrop, 1961), in showing the indeterminacy of even finite games. This realization came after an understanding of *effective playability in arithmetical games*, developed elegantly by Michael Rabin more than fifty years ago (Rabin, 1957). This latter work, in turn, stands on the tradition of *alternative games* pioneered by Zermelo (1913), and misunderstood, misinterpreted and misconstrued by generations of orthodox game theorists.

The brief, rich and primarily recursion theoretic framework of Harrop's classic paper requires a deep understanding of the rich interplay between recursivity and constructive representations of *finite sets* that are recursively enumerable. There is also an obvious and formal connection between the notion of a *finite combinatorial object*, whose complexity is formally defined by the uncomputable Kolmogorov measure of complexity, and the results in Harrop's equally pioneering attempt to characterize the recursivity of finite sets and the resulting indeterminacy – undecidability – of a Nash equilibrium even in the finite case. To the best of my knowledge this interplay has never been mentioned or analysed. This will be an important research theme in the path towards an algorithmic revolution in economics.

However, *algorithmic game theory*, at least so far as such a name for a field is concerned, seems to have been first 'defined' by Christos Papadimitriou (2007, pp. xiii–xiv; italics added):

> [T]he Internet was the first computational artefact that was not created by a single entity (engineer, design team, or company), but emerged from the strategic interaction of many. Computer scientists were for the first time faced with an object that they had to feel the same bewildered awe with which economists have always approached the market. And, quite predictably, they turned to game theory for inspiration – in the words of Scott Shenker, a pioneer of this way of thinking . . . 'the Internet is an equilibrium, we just have to identify the game'. A fascinating fusion of ideas from both fields – game theory and algorithms – came into being and was used productively in the effort to illuminate the mysteries of the Internet. It has come to be called algorithmic game theory.

But, alternative games were there, long before the beginning of the emergence of recursion theory, even in the classic work of Gödel, later merging with the work that led to Matiyasevich's decisive (negative) resolution of *Hilbert's Tenth Problem* (Matiyasevich, 1993). Hence, the origins of algorithmic game theory, like those of algorithmic statistics, lie in the intellectual forces that gave rise to the *grundlagenkrise* of the 1920s. Simply put, in the battle between alternative visions on proof and logic (see, for example, the references to Max Euwe in Section 3, above).

The two cardinal principles of what I think should be called algorithmic and arithmetical games are *effective playability and (un)decidability*, even in *finite* realizations of such games, and *inductive inference from finite sequences* for algorithmic statistics. These desiderata cannot be fulfilled either by what has already become orthodox algorithmic game theory and classical statistical theory.

In footnote 22, above, and in the related, albeit brief text to which it is a note, I have indicated why the theory of mechanism design, a field at the frontiers of research in mathematical economics, may have nothing whatsoever to do with the notion of algorithm, and its underpinning logic, that is the focus in this paper. The concept of *algorithmic mechanism design* is defined, for example, in Nisan (2007, p. 210). The kind of algorithms required, to be implemented on the economic mechanisms in such a theory, are those that can compute the uncomputable, decide the undecidable and are underpinned by a logic that 'completes' the 'incompleatable'.

At least since Walras devised the *tâtonnement* process and Pareto's appeal to the market as a computing device (albeit an *analogue* one, Pareto, 1927), there have been sporadic attempts to find mechanisms to solve a system of supply–demand equilibrium equations, going beyond the simple counting of equations and variables. But none of these enlightened attempts to devise mechanisms to solve a system of equations were predicted upon the elementary fact that the data types – the actual numbers – realized in, and used by, economic processes were, at best, rational numbers. The natural equilibrium relation between supply and demand, respecting the elementary constraints of the equally natural data types of market – or any other kind of economy – should be framed as a *Diophantine decision problems* (cf. Velupillai, 2005), in the precise sense in which Gödel refers to such things (see, above, Section 2) and the way arithmetic games are formalized and shown to be effectively unsolvable in analogy with *Hilbert's Tenth Problem* (cf. Matiyasevich, 1993).

The Diophantine decision theoretic formalization is, thus, common to at least three kinds of algorithmic economics: classical behavioural economics (cf. Velupillai, 2010b), algorithmic game theory in its incarnation as arithmetic game theory (cf. Chapter 7 in Velupillai, 2000) and elementary equilibrium economics. Even those, like Smale (1976, 1981), who have perceptively discerned the way the problem of finding mechanisms to solve equations was subverted into formalizations of inequality relations which are then solved by appeal to (unnatural) non-constructive, uncomputable, fixed point theorems did not go far enough to realize that the data types of the variables and parameters entering the equations needed not only to be constrained to be non-negative, but also to be rational (or integer) valued). Under these latter constraints economics in its behavioural, game theoretic and microeconomic modes must come to terms with absolutely (algorithmically) undecidable problems. This is the cardinal message of the path towards a revolution in algorithmic economics.

Therefore, if orthodox algorithmic game theory, algorithmic mechanism theory and computable general equilibrium theory have succeeded in computing their respective equilibria, then they would have to have done it with algorithms that are not subject to the strictures of the Church–Turing thesis or do not work within the (constructive) proof-as-algorithm paradigm. This raises the mathematical meaning of the notion of algorithm in algorithmic game theory, algorithmic mechanisms theory and computable general equilibrium theory (and varieties of so-called computational economics). Either they are of the kind used in numerical analysis and so-called 'scientific computing' (as if computing in the recursion and constructive theoretic traditions are not 'scientific') and, if so, *their* algorithmic foundations are, in turn, constrained by either the Church–Turing thesis (as in Blum *et al.*, 1998) or the (constructive) proof-as-algorithm paradigm; or, the economic system and its agents and institutions are computing the formally uncomputable and deciding the algorithmically undecidable (or are formal systems that are inconsistent or incomplete).

The only way I know, for now, to link the two visions of algorithms – short of reformulating all the above problems in terms of analogue computing models (see also the next footnote) – is through Gandy's definition of mechanism so that his characterizations, when not satisfied, imply a mechanism that can compute the uncomputable. Gandy enunciates four set-theoretic '*Principles for Mechanisms*' (Gandy, 1980) to describe discrete deterministic machines[35]:

1. The form of description;
2. The principle of limitation of hierarchy;
3. The principle of unique reassembly and
4. The principle of local causality.

He then derives the important result, as a theorem, that any device which satisfies the four principles jointly generates successive states that are computable, and conversely, any formal weakening of any of the above four principles, then there exist mechanisms that compute the uncomputable (Gandy, 1980, p. 123; italics in the original):

> It is proved that that if a device satisfied the [above four] principles [simultaneously] then its successive states form a computable sequence. Counter-example are constructed which show that if the principles be weakened in almost any way, then there will be devices which satisfy the weakened principles and which can calculate *any* number-theoretic function.

It is easy to show that market mechanisms and, indeed, all orthodox theoretical resource allocation mechanisms violate one or more of the above principles. Therefore, there is a generic impossibility result, similar to that of Arrow's in Social Choice Theory, inherent in mechanism theory, when analysed from the point of view of algorithmic economics. Hence any claims about *constructing* mechanisms to depict the efficient functioning of a market economy, the rational behaviour of agents and the rational and efficient organization of institutions and the derivation of efficient policies, as made by orthodox economic theorists in micro and macro economics, IO theory and game theory, are based on non-mechanisms in the precise sense of algorithmic economics. The immediate parallel would be a claim by an engineer to have built a perpetual motion machine, violating the (phenomenological) laws of thermodynamics.

Finally, I come to the topic of *algorithmic economic dynamics*. In the same spirit of respecting the constraints on economic data types, I have now come round to the view that it is not sufficient to consider just computable economic dynamics, theorized in terms of the theory of one or another variety of computable or recursive analysis. This meant, in my work thus far, the dynamical system equivalent of a Turing Machine had to be a discrete dynamical system acting on rational numbers (or the natural numbers).

Even if such is possible – i.e. constructing a discrete dynamical system acting on rational numbers – the further requirement such a dynamical system must satisfy is the capability of encapsulating three additional properties:

- The dynamical system should possess a relatively simple global attractor;
- It should be capable meaningfully and measurably long – and extremely long – transients;[36]
- It should possess not just ordinary sensitive dependence on initial conditions (SDIC) that characterize 'complex' dynamical systems that generate strange attractors. It should, in fact, possess *Super Sensitive Dependence on Initial Conditions* (*SSDIC*). This means that the dynamical system *appears* to possess

the property that distances between neighbouring trajectories diverge too fast to be encapsulated by even partial recursive functions.

Is it possible to construct such rational valued dynamical systems or, equivalently, algorithms that imply such dynamical systems? The answer, mercifully, is yes. In Velupillai (2010c), I have discussed how, for a Clower-Howitt 'Monetary Economy' (cf. Clower-Howitt, 1978), with rational valued, saw-tooth like monetary variables, it is possible to use the '*Takagi function*' to model its dynamics, while preserving its algorithmic nature. But in this case, it is necessary to work with computable – or recursive – analysis. It would be more desirable to remain within classical algorithmic formalizations and, hence, working with rational- or integer-valued dynamical systems that have a clear algorithmic underpinning.

I believe *Goodstein's algorithm* (cf. Goodstein, 1944) could be the paradigmatic example for modelling rational – or integer – valued algorithmic (nonlinear) economic dynamics (Paris and Tavakol, 1993). In every sense in which the notion of algorithm has been discussed above, for the path towards an algorithmic revolution in economics, is most elegantly satisfied by this line of research, a line that has by passed the mathematical economics and nonlinear macrodynamics community. This is the only way I know to be able to introduce the algorithmic construction of an integer-valued dynamical system possessing a very simple global attractor, and with immensely long, effectively calculable, transients, whose existence is *unprovable* in Peano arithmetic. Moreover, this kind of nonlinear dynamics, subject to super sensitive dependence on initial conditions (SSDIC), ultra-long transients and possessing simple global attractors whose existence can be encapsulated within a classic Gödelian, Diophantine, decision theoretic framework, makes it also possible to discuss effective *policy* mechanisms (cf. Kirby and Paris, 1982).

Diophantine decision problems emerge in the unifying theoretical framework, and the methodological and epistemological bases, for the path towards an algorithmic revolution in economics. Algorithmic economics – in their (classical) behavioural), microeconomic, macroeconomic, game theoretic, learning, finance and dynamic theoretical frameworks when the constraints of the natural data types of economics is respected – turns out to be routinely faced with absolutely undecidable (algorithmic) problems, at every level. In the face of this undecidability, indeterminacy of a kind that has nothing to do with a probabilistic underpinning for economics at any level, is the rule. Unknowability, undecidability, uncomputability, inconsistency and incompleteness endow every aspect of economic decision making with algorithmic indeterminacy.

The completion of the epistemological and methodological basis for economics given by the framework of Diophantine decision theoretic formalization, in the face of absolutely (algorithmically) undecidable problems, should, obviously, require a sound philosophical grounding, too. This, I believe, is most naturally provided by harnessing the richness of Husserlian phenomenology for the philosophical underpinning of algorithmic economics. This aspect remains an entirely virgin research direction, in the path towards an algorithmic revolution in economics, where indeterminacy and ambiguity underpin perfectly rational decision making.

How, then, can a belief in the eventual desirability and necessity of an algorithmic revolution in economics be fostered and furthered by educators and institutions that may not shy away from exploring alternatives? After all, whatever ideological underpinnings the mathematization of economics may have had, if not for the possibility of mathematical modelling of theoretical innovations, we would, surely, not have had any of the advances in economic theory at any kind of policy level?

In this particular sense, then, I suggest that a program of graduate education – eventually trickling downwards towards a reformulation of the undergraduate curriculum, too – is devised, in a spirit of adventure and hope, to train students in economics, finance and business in the tools, concepts and philosophy of algorithmic economics. It is easy enough to prepare a structured program for an intensive doctoral course in algorithmic economics, replacing traditional subjects with economic theory, game theory, behavioural economics, finance theory, nonlinear dynamics, learning and induction, stressing education – learning and teaching – from the point of view of algorithmic mathematics, methodologically – in the form of mathematical methods – and epistemologically – in the sense of knowledge and its underpinnings. Given that the nature of algorithmic visions is naturally dynamic, computational and experimental, these aspects would form the thematic core of the training and education program.

No mathematical theorem would be derived, in any aspect of economics, without explicit algorithmic content, which automatically means with computational and dynamic content, naturally amenable to experimental implementations. The schizophrenia between one kind of mathematics to devise, derive and prove theorems in economic theory and another kind of mathematics when it is required to give the derived, devised and proved results numerical and computational content, would forever be obliterated – at least from the minds of a new and adventurous generation of economists.

A decade ago, after reading my first book on computable economics (Velupillai, 2000), Herbert Simon wrote, on 25 May, 2000, to one of my former colleagues as follows:

> I think the battle has been won, at least the first part, although it will take a couple of academic generations to clear the field and get some sensible textbooks written and the next generation trained.

The 'battle' that 'had been won' against orthodox, non-algorithmic, economic theory had taken Simon almost half a century of sustained effort in making classical behavioural economics and its algorithmic foundations the centrepiece of his research at the theoretical frontiers of computational cognitive science, behavioural economics, evolutionary theory and the theory of problem solving. Yet, he still felt that more time was needed, in the form of 'two [more] academic generations to clear the field and get some sensible textbooks written and the next generation trained'.

For the full impact of a complete algorithmic revolution in economics, I am not sure orthodoxy will permit 'the clearing of the field', even if 'sensible textbooks' are written to get the 'next generation trained'. All the same, it is incumbent upon us to make the attempt to prepare for an algorithmic future, by writing the 'sensible textbooks' for the next – or future – generations of students, who will be the harbingers of the algorithmic revolution in economics.

There are no blueprints for writing textbooks for the harbingers of revolutions. Paul Samuelson's *Foundations of Economic Analysis* brought forth a serious revolution in the training of students with a level of skill on mathematics that was an order of magnitude much greater than previous generations – much sooner than did *The Theory of Games and Economic Behaviour*. The former will be my 'model' for pedagogical success; the latter for the paradigm shift, to be utterly banal about the choice of words, in theories, in the sense in which Keynes meant it, in the opening quote of this section. The non-numerical content and the pervasive use of the *tertium non datur* in the *Theory of Games and Economic Behaviour*, all the way from its rationality postulates to the massively complex many-agent, multilayered, institutional context, can only be made clear when an alternative mathematics is shown to be possible for problems of the same sort. This means a reformulation of mathematical economics in terms of Diophantine decision theory as the starting point and it is, ultimately, the equivalent of the revolution in vision wrought by von Neumann and Morgenstern, for the generations that came before us.

They had a slightly easier task, in a peculiarly subversive sense: they were confronted, largely, with an economics community that had not, as yet, been permanently 'contaminated' by an orthodox mathematics. Those of us, following Simon and others who believe in the algorithmic revolution in economics, face a community that is almost over-trained and overwhelmed by the techniques of classical mathematics and non-intuitionistic logic and, therefore, let preposterous assertions, claims and 'accusations', like those by Efe Ok, to students who are never made aware of alternative possibilities of formalization respecting the computational and numerical prerogatives of economics.

Two simple examples will suffice to show the enormous task facing the textbook writer of algorithmic economics. The notion of function, in addition to that of sets, is all pervasive in the mathematical economics tradition that comes down from the revolutions that were set in their paces by the Foundations of Economic Analysis and The Theory of Games and Economic Behaviour. To disabuse a young, mathematically trained graduate student in economics, of a reliance on these seemingly all-pervasive notions – long before even beginning to frame economic problems as Diophantine decision problems – cannot be easy, but it is possible. This new generation is computer-literate in a way that was unimaginable at the time Samuelson and von Neumann–Morgenstern began their journeys on a new path. It should be easy to introduce the *λ-calculus* as the natural function concept for computation and *categories* (on which basis *toposes* can be introduced) as the basic mathematical entity, replacing sets. From these basic conceptual innovations it is easy to make clear, at a very elementary pedagogical level, why the *teritum non datur* is both unnecessary and pernicious for a subject that is intrinsically computational, numerical – and phenomenological.

The strategy would be the Wittgensteinian one of letting those mesmerized by Hilbert's invitation to stay in Cantor's paradise leave it of their own accord:

> Hilbert (1925 [1926], p. 191): 'No one shall drive us out of the paradise which Cantor has created for us'.
>
> Wittgenstein, (1939, p. 103): I would say, 'I wouldn't dream of trying to drive anyone out of this paradise'. I would try to do something quite different: I would try to show you that it is not a paradise – so that you'll leave of your own accord. I would say, 'You're welcome to this; just look about you'.

I do not envisage the slightest difficulty in gently replacing the traditional *function* concept and the abandonment of the reliance on the notion of *set* by, respectively the λ-*notation* and the λ-*calculus*, on the one hand, and *categories*, on the other. But disabusing the pervasive influence of reliance on the *tertium non datur* is quite another matter – replacing *Hilbert's Dogma* with (*constructive*) *proof-as-algorithm* vision as the natural reasoning basis. This is where one can only hope, by sustained pedagogy, to persuade students to 'leave' *Hilbert's paradise* 'of their own accord.

I fear that just two generations of text book writing will not suffice for this.

The part that will require more gentle persuasion, in its implementation as well as in its dissemination pedagogically, will be the philosophical part, the part to be underpinned by something like Husserlian phenomenology, extolling the virtues of indeterminacies and unknowability. This part is crucial in returning economics to its humanistic origins, away from its increasingly vacuous tendencies towards becoming simply a branch of applied mathematics.

The ghost, if not the spirit, of Frege looms large in the epistemology that permeates the themes in this paper. I can think of no better way to conclude this paean to an algorithmic revolution in economics than remembering Frege's typically perspicacious reflections on *Sources of Knowledge of Mathematics and the Mathematical Natural Sciences*[37] (Frege, 1924/1925, p. 267):

> When someone comes to know something it is by his recognizing a thought to be true. For that he has first to grasp the thought. Yet I do not count the grasping of the thought as knowledge, but only the recognition of its truth, the judgement proper. What I regard as a source of knowledge is what justifies the recognition of truth, the judgement.

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Notes

1. I suspect Cohen means 'solve mathematical *problems*', because 'solving mathematical *theorems*' seems a meaningless phrase.
2. Whether this was, in fact, 'Hilbert's project' and 'vision' I am not sure and am not prepared to endorse that it was so. My own take on this is partially summarized in Section 3 below and Velupillai (2010a).
3. However, as I shall try to show in Section 4, below, the status of algorithmic game theory is more in line with computable general equilibrium theory than with the other fields mentioned above, which are solidly grounded in some form of computability theory.
4. See Bell (1998, especially chapter 8) for a lucid, yet rigorous, substantiation of this claim, although presented in the context of *Smooth Infinitesimal Analysis*, which is itself of relevance to the mathematical economist over-enamoured by orthodox analysis and official non-standard analysis. As in constructive analysis, in smooth infinitesimal analysis, all functions in use are continuous. A similar – though not exactly equivalent – case occurs also in computable analysis. Incidentally, I am not quite sure whether the plural of a *topos* is *topoi* or *toposes*!
5. See Section 3 for further discussion of this point.
6. Some, but not all, of them are referred to as theorems; none of the 'propositions' in Sraffa (1960) are referred to as theorems, lemmas, or given any other formal, mathematical, label.
7. I hope in saying this I am reflecting the general opinion of the mathematical economics community.
8. Debreu refers to this as a 'deeper theorem', without suggesting in what sense it is 'deep'. Personally, I consider it a trivial – even an 'apologetic' – theorem, and I am quite prepared to suggest in what sense I mean 'trivial'.
9. For many years I referred to Sraffa's proofs as being constructive in the strict mathematical sense. I now think it is more useful to refer to them as algorithmic proofs.
10. Herbert Simon, together with Newell and Shaw (1957), in their work leading up to the monumental work on *Human Problem Solving* (Newell and Simon, 1972), and Hao Wang (1960), in particular, automated most of the theorems in first 10 chapters of *Principia Mathematica* (Whitehead and Russell, 1927). Surely, it is time one did the same with von Neumann-Morgenstern (1947)? I am confident that none of the theorems of this classic are proved constructively, in spite of occasional claims to

the contrary. If I was younger – but, then, much younger – I would attempt this task myself!

11. In the same important collection of essays that includes the previously cited papers by Cohen and Hyland.
12. Alan Turing arrived at a similar definition prior to von Neumann.
13. Practically all my research and teaching activities for the past decade has tried to make this point, from every possible economic point of view. One representative reference, choosing a mid-point in the decade that has passed, is Velupillai (2005), where the way to formalize even elementary supply–demand systems as Diophantine decision problems is outlined. The point here, apart from remaining faithful to the natural data types and problem focus – solvability of Diophantine equations – is to emphasize the roles of ambiguity, unsolvability, undecidability and uncomputability in economics and de-throne the arrogance of mathematical determinism of orthodox economic theory. Economists have lost the art of solving equations at the altar of Hilbert's Dogma, i.e. proof-as-consistency = existence, the topic of the next section.
14. It would be useful to recall Robin Gandy's somewhat 'tongue-in-cheek' attempt at a 'precise' characterization of this term (Gandy, 1980, p. 125; italics in the original):

    > 'For vividness I have so far used the fairly nebulous term "machine." Before going into details I must be *rather* more precise. Roughly speaking I am using the term with its 19th century meaning; the reader may like to imagine some glorious contraption of gleaming brass and polished mahogany, or he may choose to inspect the parts of Babbage's "Analytical Engine" which are preserved in the Science Museum at South Kensington'.

    It is refreshing to read, in the writing of a logician of the highest calibre, someone being '*rather* more precise' doing so in 'roughly speaking' mode!
15. Sometimes this approach is referred to as the 'proofs-as-program paradigm' (cf. Maietti and Sambin, 2005, chapter 6, especially pp. 93–95).
16. The trouble is that almost no one, outside the somewhat small circle of the constructive mathematical community, makes much of an effort to read or 'look between the lines'.
17. However, the unwary reader should be made aware that the appeal to the *tertium non datur* by the recursion theorist is usually for the purpose of deriving *negative universal assertions*; *positive existential assertions* are naturally constructive, even within recursion theory.
18. In posing, and trying to answer, this question, I am not addressing myself to flippant assertions in popularized nonsense – as distinct from pretentious nonsense, an example of which is discussed in the next section – such as Beinhocker (2006), for example, p12: 'Evolution is an *algorithm*'.
19. Aberth's important work was instrumental in showing the importance of integrating Ramon Moore's pioneering work in *Interval Analysis* and *Interval Arithmetic* (Moore, 1966) in algorithmic implementations (cf. Hayes, 2003).
20. For an exceptionally lucid demonstration and discussion of these issues, see Shankar (1994).
21. In other words, the rich literature on the formal characterization of a *mechanism* and its *limitations*, have played no part in economic theory or mathematical economics (cf. Gödel, 1951; Kreisel, 1974; Gandy, 1980, 1982; Shapiro, 1998). This is quite similar to the way the notion of information has been – and is being – used

in economic theory, in both micro and macro, in game theory and industrial organization (IO). None of the massive advances in, for example, *algorithmic information theory*, unifying Claude Shannon's pioneering work with those of Kolomogorov and Chaitin, have had the slightest impact in formal economic theorizing, except within the framework of *Computable Economics* (Velupillai, 2000), a phrase I coined more than 20 years ago, to give content to the idea of an economic theory underpinned by recursion theory (and constructive mathematics).

22. In van Dalen's measured, studied, scholarly, opinion, (van Dalen, 2005, pp. 576–577; italics added): 'Because Hilbert's yardstick was calibrated by the continuum hypothesis, Hilbert's dogma, "consistency ⇔ existence," and the like, he was by definition right. But *if one is willing to allow other yardsticks*, no less significant, but based on alternative principles, then Brouwer's work could not be written off as obsolete 19th century stuff'.
23. At the end of his paper Euwe reports that von Neumann brought to his attention the works by Zermelo and Konig, after he had completed his own work (Euwe, 1929, p. 641). Euwe then goes on (italics added):

    '*Der gegebene Beweis is aber nicht konstruktive*, d.h. es wird keine Methode angezeigt, mit Hilfe deren der gewinnweg, wenn überhaupt möglich, in *endlicher Zeit konstruiert werden kann*'.

24. Gaisi Takeuti's important observation is obviously relevant here (Takeuti, 2003, pp. 73–74; italics added):

    'There has been an idea, which was originally claimed by Gödel and others, that, if one added an axiom which is a strengthened version of the existence of a measurable cardinal to existing axiomatic set theory, then various mathematical problems might all be resolved. Theoretically, nobody would oppose such an idea, but, in reality*, most set theorists felt it was a fairy tale and it would never really happen*. *But it has been realized by virtue of the axiom of determinateness, which showed Gödel's idea valid*'.

25. Brouwer could not have been clearer on this point, when he wrote, in his 1907 thesis (Brouwer, 1907, p. 45; quotes added):

    '*[T]he continuum as a whole* was given to us by 'intuition'; *a construction* for it, an action which would create 'from the mathematical intuition' "all" its points as individuals, is inconceivable and impossible. *The* 'mathematical intuition' is unable to create other than denumerable sets of individuals'.

26. Even as early as in 1908, we find Brouwer dealing with this issue with exceptional clarity (cf. Brouwer 1908b, pp. 109–110; quotes added):

    'Now consider the principium *tertii exclusi*: It claims that every supposition is either true or false; . . . Insofar as only 'finite discrete systems' are introduced, the investigation whether an imbedding is possible or not, can always be carried out and admits a definite result, so in this case the principium tertii exclusi is reliable as a principle of reasoning. [I]n infinite systems the principium tertii exclusi is as yet not reliable'.

27. See, also, Bishop and Bridges (1985, p. 13, 'Notes').
28. Bishop (1967, p. 9), refers to a version of the law of the excluded middle as the *principle of omniscience*.

29. A timely conversation with Brian Hayes, who happened to be in Trento while this paper was being finalized, on the λ-calculus, and an even more serendipitous event in the form of a seminar on, *How Shall We Educate the Computational Scientists of the Future* by Rosalind Reid, on the same day, helped me structure this concluding section with pedagogy in mind.
30. Which I have, in recent writings, referred to also as 'classical behavioural economics' and outlined its algorithmic basis in Velupillai (2010b).
31. Most elegantly, pedagogically and rigorously summarized in Shafer and Vovk (2001), although I trace the origins of research in algorithmic finance theory in the extraordinarily perceptive work by Osborne (1977).
32. In Velupillai (2000), chapters 5 and 6, I discussed both algorithmic probability theory and algorithmic learning theory as *The Modern Theory of Induction* and *Learning in a Computable Setting*, respectively.
33. In my '*Tampere Lecture'* (Velupillai, 2009b), I tried to outline the development of algorithmic statistics.
34. Again, in Velupillai (2009b) and Velupillai (1997) I referred to algorithmic game theory as arithmetic game theory and discussed its origins and mathematical framework is some detail.
35. However, Gandy adds the important explicit caveat that he (Gandy, 1982, p. 125; italics in the original): '[E]xcludes from consideration devices which are *essentially* analogue machines'. The use of isolated probabilistic elements in the implementation of an algorithm does not make it – the algorithm – a random mechanism; and, even if they did, there is an adequate way of dealing with them within the framework of both recursion and constructive mathematical theories of algorithms.
36. It was in a footnote in chapter 17 of the *General Theory* (Keynes, 1936) stressed the importance of *transition regimes* and made the reference to *Hume as the progenitor of the equilibrium concept in economics* (p. 343, footnote 3; italics added):

    '[H]ume began the practice amongst economists of stressing the importance of *the equilibrium position* as compared with the ever-shifting transition towards it, though he was still enough of a mercantilist not to overlook the fact *that it is in the transition that we actually have our being*: . . .'.

37. Naturally, I believe he would have added 'mathematical social sciences' had he been writing these thoughts today.

References

Aberth, Oliver (1980) *Computable Analysis*. New York: McGraw-Hill Book Company.

Aberth, Oliver (2001) *Computable Calculus*. San Diego, CA: Academic Press.

Beinhocker, Eric. D. (2006) *The Origin of Wealth: The radical Remaking of Economics and What It Means for Business and Society*. Boston, MA: Harvard Business School Press.

Bell, John L. (1998) *A Primer of Infinitesimal Analysis*. Cambridge, UK: Cambridge University Press.

Bishop, Errett. A. (1967) *Foundations of Constructive Analysis*. New York: McGraw-Hill Book Company.

Bishop, Errett and Douglas Bridges (1985) *Constructive Analysis*. Heidelberg, Germany: Springer-Verlag.

Blum, Lenore, Felipe Cucker, Michael Shub and Steve Smale (1998) *Complexity and Real Computation*. New York: Springer Verlag.

Bridges, Douglas and Fred, Richman (1987) *Varieties of Constructive Mathematics*. Cambridge, UK: Cambridge University Press..

Brouwer, Lutizen E.J. (1907/1975) Over de grondslagen der wiskunde [On the foundations of mathematics], Academic Thesis. In Arend Heyting (ed.), *L.E.J. Brouwer Collected Works: Vol. 1 – Philosophy and Foundations of Mathematics* (pp. 11–104). Amsterdam, Netherlands: North-Holland; New York: American Elsevier.

Brouwer, Lutizen E.J. (1908a/1975) Over de grondslagen der wiskunde [On the foundations of mathematics], Academic Thesis. In Arend Heyting (ed.), *L.E.J. Brouwer Collected Works: Vol. 1 – Philosophy and Foundations of Mathematics* (pp. 105–106). Amsterdam, Netherlands: North-Holland; New York: American Elsevier.

Brouwer, Lutizen E.J. (1908b/1975) De onbetrouwbaarheid der logische principes [The unreliability of the logical principles]. In Arend Heyting (ed.), *L.E.J. Brouwer Collected Works: Vol. 1 – Philosophy and Foundations of Mathematics* (pp. 197–111). Amsterdam, Netherlands: North-Holland; New York: American Elsevier.

Brouwer, Luitzen E.J. (1952) An intuitionist correction of the fixed-point theorem on the sphere. *Proceedings of the Royal Society*, London, UK, Vol. 213, 1–2, 5 June 1952.

Clower, Robert. W. and Peter W. Howitt (1978) The transaction theory of the demand for money: a reconsideration. *Journal of Political Economy* 86(3): 449–466.

Cohen, Daniel I. A. (1991) The superfluous paradigm. In J.H. Johnson and M.J. Loomes (eds), *The Mathematical Revolution Inspired by Computing* (pp. 323–329). Oxford: Oxford University Press.

Davis, Martin (1978) What is a computation? In Lynn Arthur Steen (ed.), *Mathematics Today – Twelve Informal Essays* (pp. 242–267). New York: Springer-Verlag.

Debreu, Gerard (1959) *Theory of Value: An Axiomatic Analysis of Economic Equilibrium*. London, UK: John Wiley & Sons, Inc.

Dummett, Michael (1977) *Elements of Intuitionism*. Oxford: Clarendon Press.

Euwe, Max (1929) *Mengentheoretische Betrachtungen über das Schachspiel*, Communicated by Prof. R. Weizenböck (May, 25, 1929), *Proc. Koninklijke Nederlandse Akademie Van Wetenschappen*, Amsterdam, 32(5): 633–642.

Feferman, Solomon (2009) Gödel, Nagel, minds, and machines. *The Journal of Philosophy* 106(4): 201–219.

Frege, Gottlob (1924/1925/1970) Sources of knowledge of mathematics and the mathematical natural sciences. In Hans Hermes, Friedrich Kambartel and Friedrich Kaulbach (eds), with the assistance of Gottfried Gabriel and Walburgs Rödding, *Posthumous Writings* (pp. 267–277). Oxford: Basil Blackwell.

Gács, Péter, John T. Tromp and Paul Vitányi (2001) Algorithmic statistics. *IEEE Transactions on Information Theory* 47(6): 2443–2463.

Gandy, Robin. O. (1980) Church's thesis and principles for mechanisms. In J. Barwise, H.J. Keisler and K. Kunen (eds), *The Kleene Symposium* (pp. 123–148). Amsterdam, Netherlands: North-Holland.

Gandy, Robin. O. (1982) Limitations to mathematical knowledge. In D. van Dalen, D. Lascar and J. Smiley (eds), *Logic Colloquium '80* (pp. 129–146). Amsterdam, Netherlands: North-Holland.

Gödel, Kurt (1951/1995) Some basic theorems on the foundations of mathematics and their implications. In Solomon Feferman, John W. Dawson, Jr., Warren Goldfarb, Charles Parsons and Robert N. Solovay (eds), *Kurt Gödel – Collected Works, Volume III, Unpublished Essays and Lectures* (pp. 304–323). Oxford: Oxford University Press.

Goodstein, Reuben. L. (1944) On the restricted ordinal theorem. *Journal of Symbolic Logic* 9(2): 33–41.

Harrop, Ronald (1961) On the recursivity of finite sets, Zeitschrift für Mathematische Logik und Grundlagen der Mathematik, Bd, 7, pp. 136–140.

Hayes, Brian (2003) A lucid interval. *American Scientist* 91(6): 484–488.

Hilbert, David (1925 [1926]), On the infinite. In Paul Benacerraf and Hilary Putnam (eds) *Philosophy of Mathematics – Selected Readings*, 2nd edn (pp. 183–201), 1983. Cambridge, UK: Cambridge University Press.

Hyland, J.M.E. (1991) Computing and foundations. In J.H. Johnson and M.J. Loomes (eds), *The Mathematical Revolution Inspired by Computing* (pp. 269–284). Oxford: Oxford University Press.

Keynes, John Maynard (1936) *The General Theory of Employment, Interest and Money*. London, UK: Macmillan and Co., Limited.

Kirby, Laurie and Jeff Paris (1982) Accessible independence results for Peano arithmetic. *Bulletin of the London Mathematical Society* 14: 285–293.

Knuth Donald. E. (1973) *The Art of Computer Programming: Volume 1/Fundamental Algorithms*, 2nd edn. Reading, MA: Addison-Wesley Publishing Company.

Knuth, Donald E. (1981) Algorithms in modern mathematics and computer science. In A.P. Ershov and Donald E Knuth (eds), *Algorithms in Modern Mathematics and Computer Science* (pp. 82–99). Berlin, Germany: Springer-Verlag.

Kreisel, Georg (1974) A notion of mechanistic theory. *Synthese* 29: 11–26.

Maietti, Maria Emilia and Giovanni Sambin (2005) Toward a minimalist foundation for constructive mathematics. In L. Crosilla and P. Schuster (eds), *From Sets and Types to Topology and Analysis: Towards Practicable Foundations for Constructive Mathematics* (Chapter 6, pp. 91–114). Oxford: Clarendon Press.

Markov, A.A. (1954/1961) Theory of Algorithms, Academy of Sciences of the USSR, Moscow and Leningrad, translated by Jacques J. Schorr-Kon and PST Staff and published for The National Science Foundation, Washington, D.C., and The Department of Commerce, USA, by The Israel Program for Scientific Translations, Jerusalem.

Matiyasevich, Yuri M. (1993) *Hilbert's Tenth Problem*. Cambridge, MA: The MIT Press.

McCulloch, Warren. S. (1961/1965) *What is a Number, that a Man May Know It, and a Man, that He May Know a Number*, The Ninth Alfred Korzybski Memorial Lecture, reprinted in: *Embodiments of Mind* by Warren S. McCulloch Chapter 1, pp. 1–18. Cambridge, MA: The M.I.T. Press.

Moore, Ramon. E. (1966) *Interval Analysis*. Englewood Cliffs, NJ: Prentice-Hall.

Moschovakis, Yiannis. N. (1998) On founding the theory of algorithms. In H.G. Dales and G. Oliveri (eds), *Truth in Mathematics* (Chapter 4, pp. 71–104). Oxford: Clarendon Press.

Moschovakis, Yiannis. N. (2001) What is an algorithm? In B. Engquist and W. Schmid (eds), *Mathematics Unlimited – 2001 and Beyond* (pp. 919–936). Berlin, Germany: Springer-Verlag.

Moschoavakis, Yiannis. N. and Vasilis Paschalis (2008) Elementary algorithms and their implementations. In S. Barry Cooper, Benedikt Löwe and Andrea Sorbi (eds), *New Computational Paradigms: Changing Conceptions of What is Computable* (pp. 87–118). New York: Springer Science and Business Media LLC.

Newell, Allen and Herbert A. Simon (1972) *Human Problem Solving*. Englewood Cliffs, NJ: Prentice-Hall, Inc.

Newell, Allen, J.C. Shaw and Herbert A. Simon (1957) Empirical explorations of the logic theory machine: a case study in heuristics. *Proceeding of the Western Joint Computer Conference* 11: 218–239.

Nisan, Noam (2007) Introduction to mechanism design (for computer scientists). In Noam Nisan, Tim Roughgarden, Éva Tardos and Vijay V. Vazirani (eds), *Algorithmic Game Theory* (chapter 9, pp. 209–241). New York: Cambridge University Press.

Ok, Efe. A. (2007) *Real Analysis with Economic Applications*. Princeton, NJ: Princeton University Press.

Osborne, Maury (1977) *The Stock Market and Finance from a Physicist's Viewpoint*. Minneapolis, MN: Crossgar Press.

Papadimiriou, Christos H. (2007) Forward. In Noam Nisan, Tim Roughgarden, Éva Tardos and Vijay V. Vazirani (eds), *Algorithmic Game Theory* (pp. 29–51). New York: Cambridge University Press.

Pareto, Vilfredo (1927/1971) *Manual of Political Economy, translated from the French Edition of 1927 by Ann S. Schwier*, Ann S. Schwier and Alfred N. Page (eds). London: The Macmillan Press Ltd.

Paris, Jeff and Reza Tavakol (1993) Goodstein algorithm as a super-transient dynamical system. *Physics Letters A* 180(1–2): 83–86.

Posy, Carl. J. (1998) Brouwer versus Hilbert: 1907–1928. *Science in Context* 11(2): 291–325.

Rabin, Michael. O. (1957) Effective computability of winning strategies. In M. Dresher, A.W. Tucker and P. Wolfe (eds), *Annals of Mathematics Studies, No. 39: Contributions to the Theory of Games, Vol. III* (pp. 147–157). Princeton, New Jersey: Princeton University Press.

Shafer, Glenn and Vladimir Vovk (2001) *Probability and Finance: It's Only a Game*. New York: John Wiley & Sons, Inc.

Shankar, N. (1994) *Metamathematics, Machines, and Gödel's Proof*. Cambridge, UK: Cambridge University Press.

Shapiro, Stewart (1998) Incompleteness, mechanism, and optimism. *The Bulletin of Symbolic Logic* 4(3): 273–302.

Smale, Steve (1976) Dynamics in general equilibrium theory. *American Economic Review* 66(2): 288–294.

Smale, Steve (1981) Global analysis and economics. In Kenneth J. Arrow and Michael D. Intrilligator (eds), *Handbook of Mathematical Economics* (Vol. I, Chapter 8, pp. 331–370). Amsterdam, Netherlands: North-Holland Publishing Company.

Sraffa, Piero (1960) *Production of Commodities by Means of Commodities: Prelude to a Critique of Economic Theory*. Cambridge, UK: Cambridge University Press.

Steinhaus, Hugo (1965) Games, an informal talk. *The American Mathematical Monthly* 72(5): 457–468.

Timpson, Christopher. G. (2004) Quantum computers: the Church-Turing hypothesis versus the turing principle. In Christof Teuscher (ed.), *Alan Turing – Life and Legacy of a Great Thinker* (pp. 213–240). Berlin, Germany: Springer-Verlag.

van Dalen, Dirk (1999) Mystic, geometer and intuitionist: the life of L.E.J. Brouwer – Volume 2: *Hope and Disillusion*. Oxford: Clarendon Press.

van Lambalgen, Michiel (1987) Random sequences, Doctoral Dissertation, University of Amsterdam, 16 September, 1987.

Velupillai, K. Vela (1997) Expository notes on computability and complexity in (arithmetical) games. *Journal of Economic Dynamics and Control* 21(6): 955–979.

Velupillai, K. Vela (2000) *Computable Economics*. Oxford: Oxford University Press.

Velupillai, K. Vela (2005) The unreasonable ineffectiveness of mathematics in economics. *Cambridge Journal of Economics* 29(6): 849–872.

Velupillai, K. Vela (2006) The algorithmic foundations of computable general equilibrium theory. *Applied Mathematics and Computation* 179(1): 360–369.

Velupillai, K. Vela (2009) Uncomputability and undecidability in economic theory. *Applied Mathematics and Computation* 215(4): 1404–1416.

Velupillai, K. Vela (2009a) A computable economist's perspective on computational complexity, J. Barkley Rosser, Jr. (ed.), *The Handbook of Complexity Research* (Chapter 4, pp. 36–83). Cheltenham, Gloucestershire, UK: Edward Elgar Publishing Ltd.

Velupillai, K. Vela (2009b) *The Algorithmic Revolution in the Social Sciences: Mathematical Economics, Game Theory and Statistics*. Invited Lecture, presented at the Workshop on Information Theoretic Methods in Science and Engineering, Tampere, Finland, August, 17/19, 2009. Published in the Proceedings of WITMSE 2009.

Velupillai, K. Vela (2010a) In praise of anarchy in research and teaching. *Economic and Political Weekly* XLV(14): 51–55.

Velupillai, K. Vela (2010b) Foundations of boundedly rational choice and satisfying decisions. *Advances in Decision Sciences*, April, 2010.

Velupillai, K. Vela (2010c) Reflections on mathematical economics in the algorithmic mode. *New Mathematics and Natural Computation* Forthcoming in, Vol. 6.

Velupillai, K. Vela and Stefano Zambelli (2010) Computation in economics. Forthcoming In John Davis and Wade Hands (eds), *The Elgar Companion to Recent Economic Methodology*, Cheltenham: Edward Elgar, 2011.

von Neumann, John (1928) Zur theorie der gesellsschaftsspiele. *Mathematische Annalen* 100: 295–320.

von Neumann, John (1938/1945–6) A model of general economic equilibrium. *Review of Economic Studies* XIII(1): 1–9.

Wang, Hao (1960/1970) Toward mechanical mathematics, In *Logic, Computers and Sets* (Chapter IX, pp. 224–268). New York, NY: Chelsea Publishing Company.

Whitehead, Alfred North and Bertrand Russell (1927) *Principia Mathematica* (Vol. I–III). Cambridge, UK: Cambridge University Press.

Wittgenstein, Ludwig (1939) Wittgenstein's Lectures on the Foundations of Mathematics – Cambridge, 1939. In Cora Diamond (ed.). *From the Notes of R.G. Bosanquet, Norman Malcolm, Rush Rhees, and Yorick Smuthies*. Chicago, IL: The University of Chicago Press.

Zermelo, Ernst (1913) *Üb*er ein anwendung der mengenlehre auf die theorie des schachspiels. In E.W. Hobson and A.E.H. Love (eds). *Proceedings of the Fifth International Congress of Mathematicians* (11–28 August, 1912, Vol. 2, pp. 501–504). Cambridge, UK: Cambridge University Press.