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Uncomputability and undecidability in economic theory ☆

K. Vela Velupillai

Department of Economics, University of Trento, Via Inama 5, 381 00 Trento, Italy

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ABSTRACT

Economic theory, game theory and mathematical statistics have all increasingly become *algorithmic sciences*. *Computable Economics*, *Algorithmic Game Theory* [Noam Nisan, Tim Roughgarden, Éva Tardos, Vijay V. Vazirani (Eds.), Algorithmic Game Theory, Cambridge University Press, Cambridge, 2007] and *Algorithmic Statistics* [Péter Gács, John T. Tromp, Paul M.B. Vitányi, Algorithmic statistics, IEEE Transactions on Information Theory 47 (6) (2001) 2443–2463] are frontier research subjects. All of them, each in its own way, are underpinned by (classical) recursion theory – and its applied branches, say computational complexity theory or algorithmic information theory – and, occasionally, proof theory. These research paradigms have posed new mathematical and metamathematical questions and, inadvertently, undermined the traditional mathematical foundations of economic theory. A concise, but partial, pathway into these new frontiers is the subject matter of this paper. Interpreting the core of *mathematical economic theory* to be defined by *General Equilibrium Theory* and *Game Theory*, a general – but concise – analysis of the *computable* and *decidable* content of the implications of these two areas are discussed. Issues at the frontiers of *macroeconomics*, now dominated by *Recursive Macroeconomic Theory* (The qualification 'recursive' here has nothing to do with 'recursion theory'. Instead, this is a reference to the mathematical formalizations of the rational economic agent's intertemporal optimization problems, in terms of Markov Decision Processes, (Kalman) Filtering and Dynamic Programming, where a kind of 'recursion' is invoked in the solution methods. The metaphor of the rational economic agent as a 'signal processor' underpins the recursive macroeconomic paradigm.), are also tackled, albeit ultra briefly. The point of view adopted is that of *classical recursion theory* and varieties of *constructive mathematics*.

1. A mathematical and metamathematical preamble[1]

Distinguished pure mathematicians, applied mathematicians, philosophers and physicists have, with the innocence of integrity and the objectivity of their respective disciplines, observing the mathematical practice and analytical assumptions of economists, have emulated the 'little child' in Hans Christian Andersen's evocative tale to exclaim similar obvious verities, from the point of view of *algorithmic mathematics*. I have in mind the 'innocent', but obviously potent, observations made by Rabin [35],

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E-mail address: kvelupillai@gmail.com

[1] I am deeply indebted to two anonymous referees for clarifying many obscure issues in an earlier version of this paper. They also contributed with deep and penetrating questions and suggestions that contributed considerably to improving the paper. Alas, they are not responsible for the remaining obscurities.

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Putnam [34], Osborne [29], Schwartz [41], Smale [42], Shafer and Vovk [39] and Ruelle [36],[2] each tackling an important core issue in mathematical economics and finding it less than adequate from a serious mathematical and computable point of view – in addition to being contrived, even from the point of view of common sense economics.[3] Decidability in games, uncomputability[4] in rational choice, inappropriateness of real analysis in the modelling of financial market dynamics, the gratuitous assumption of (topological) fix point formalizations in equilibrium economic theory, the question of the algorithmic solvability of supply-demand (diophantine) equation systems, finance theory without probability (but with an algorithmically underpinned theory of *martingales*), are some of the issues these 'innocent' pioneers raised, against the naked economic theoretic emperor (see the Prologue in Ref. [32]).

I hasten to add that there were pioneers even within the 'citadel' of economic theory. Their contributions have been discussed and documented in various of my writings over the past 20 years or so and, therefore, I shall not rehash that part of the story here.[5] Suffice it to mention just the more obvious pioneers who emerged from within the 'citadel': Peter Albin, Kenneth Arrow, Douglas Bridges,[6] Alain Lewis, Herbert Scarf and Herbert Simon. Albin, Arrow, Lewis, Scarf and Simon considered seriously, to a greater and lesser extent, the issue of modelling economic behaviour, both in the case of individually rational and in cases of strategically rational interactions, the place of formal computability considerations and their implications. Bridges and Scarf were early contributors to what may be called '*constructive economics*', complementing the '*computable economics*' of the former contributors. Scarf, of course, straddled both divides, without – surprisingly – providing a unifying underpinning in what I have come to call'*algorithmic economics*'.[7]

Economic theory, at every level and at almost all frontiers – be it microeconomics or macroeconomics, game theory or IO – is now almost irreversibly dominated by *computational*, *numerical*[8] and *experimental* considerations. Curiously, though, none of the frontier emphasis from any one of these three points of view – computational, numerical or experimental – is underpinned by the natural algorithmic mathematics of either computability theory or constructive analysis.[9] In particular, the much vaunted field of computable general equilibrium theory, with explicit claims that it is based on constructive and computable foundations is neither the one, nor the other.[10] Similarly, Newclassical Economics, the dominant strand in Macroeconomics, has as its formal core so-called *Recursive* Macroeconomic Theory. The dominance of computational and numerical analysis, powerfully underpinned by serious approximation theory, is totally devoid of computable or constructive foundations.

The reasons for this paradoxical lack of interest in computability or constructivity considerations, even while almost the whole of economic theory is almost completely dominated by numerical, computational and experimental considerations, are quite easy to discern: the reliance of every kind of mathematical economics on real analysis for formalization. I shall not go into too many details of this 'conjecture' in this paper, but once again the interested reader is referred to [51,53] for more comprehensive discussions and formal analysis (but see also Section 3, below).

Two distinguished pioneers of economic theory and, appropriately, national income accounting, Kenneth Arrow and Richard Stone (in collaboration with Alan Brown) – who also happened to be *Nobel Laureates* – almost delineated the subject matter of what I have come to call *Computable Economics*. The former conjectured, more than two decades ago, as a frontier research strategy for the mathematical economic theorist, that:

> "The next step in analysis, I would conjecture, is a more consistent assumption of computability in the formulation of economic hypothesis. This is likely to have its own difficulties because, of course, not everything is computable, and there will be in this sense an inherently unpredictable element in rational behavior." [1]

[2] In addition to the themes Ruelle broached in this '*Gibbs Lecture*', the first four, chapter 9 and the last four chapters of his elegant new book [37] are also relevant for the philosophical underpinnings of this paper. Although the two Ruelle references are not *directly* related to the subject matter of this paper, I include them because the mathematical themes of these works are deeply relevant to my approach here.

[3] Discerning scholars would notice that I have not included the absolutely pioneering work of Louis Bachelier in this list (cf. [9,12] for easily accessible English versions of Bachelier's Théorie de la Spéculation). This is only because he did not raise issues of computability, decidability and constructivity, that he could not possibly have done at the time he wrote, even though Hilbert's famous '*Paris Lecture*' was only five months away from when Bachelier defended his remarkable doctoral dissertation – also in Paris.

[4] By 'Uncomputability' I mean both that arising from (classical) recursion theoretic considerations, and from those due to formal non-constructivities (in any sense of constructive mathematics).

[5] The absence of any detailed discussion of honest priorities from within the 'citadel' in this paper is also for reasons of space constraints.

[6] Douglas Bridges is, of course, a distinguished mathematician who has made fundamental contributions – both at the research frontiers and at the level of cultured pedagogy – to constructive analysis, computability theory and their interdependence, too. However, I consider his contributions to 'constructive economics' to be at least as pioneering as Alain Lewis's to 'computable economics'. Alas, neither the one nor the other seems to have made the slightest difference to the orthodox, routine, practice of the mathematical economist.

[7] In this paper I shall not discuss the place of computational complexity theory in economics, which has an almost equally distinguished ancestry. I provide a fairly full discussion of the role of computational complexity theory, from the point of view of algorithmic economics in [54].

[8] By this I aim to refer to *classical numerical analysis*, which has only in recent years shown tendencies of merging with computability theory – for example through the work of Steve Smale and his many collaborators (cf. for example [2]). To the best of my knowledge the foundational work in computable analysis and constructive analysis was never properly integrated with classical numerical analysis.

[9] With the notable exception of the writings of the above mentioned pioneers, none of whom work – or worked – in any of these three areas, as conceived and understood these days. For excellent expositions of numerical and computational methods in economics, particularly macroeconomics, see [4,18,23].

[10] A complete and detailed analysis of the false claims – from the point of view of computability and constructivity – of the proponents and practitioners of CGE modelling is given in my recent paper devoted explicitly to the topic (cf. [52]).

Richard Stone (together with Alan Brown), speaking as an applied economist, grappling with the conundrums of adapting an economic theory formulated in terms of a mathematics alien to the digital computer and to the nature of the data,[11] confessed his own *credo* in characteristically perceptive terms[12]:

> "Our approach is quantitative because economic life is largely concerned with quantities. We use [digital] computers because they are the best means that exist for answering the questions we ask. It is our responsibility to formulate the questions and get together the data which the computer needs to answer them." [3, p. viii]

Economic analysis, as practised by the mathematical economist – whether as a microeconomist or a macroeconomist, or even as a game theorist or an IO theorist – continues, with princely unconcern for these conjectures and conundrums, to be mired in, and underpinned by, conventional real analysis (see also Ref. [6]). Therefore, it is a 'cheap' exercise to extract, discover and display varieties of uncomputabilities, undecidabilities and non-constructivities in the citadel of economic theory. Anyone with a modicum of expertise in recursion theory, constructive analysis or even nonstandard analysis in its constructive modes, would find, in any reading from these more algorithmically oriented perspectives, the citadel of economic theory, game theory and IO replete with uncomputabilities, undecidabilities and non-constructivities – even elements of incompleteness.

Against this 'potted' background of pioneering innocence and core issues, the rest of this paper is structured as follows. Some of the key results on uncomputability and undecidability, mostly derived by this author, are summarized in a fairly merciless telegraphic form (with adequate and detailed references to sources) in the next section. In Section 3 some remarks on the mathematical underpinnings of these 'negative' results are discussed and, again, stated in the usual telegraphic form. The concluding section suggests a framework for invoking my 'version' of unconventional computation models for mathematical models of the economy.[13]

2. Uncomputability and undecidability in economic theory

Although many of the results described in this section may appear to have been obtained'cheaply' – in the sense mentioned above – my own reasons for having worked with the aim of locating uncomputabilities, non-constructivities and undecidabilities in core areas of economic theory have always been a combination of intellectual curiosity – along the lines conjectured by Arrow, above –and the desire to make the subject meaningfully quantitative – in the sense suggested by Brown and Stone [3]. In the process an explicit research strategy has also emerged, on the strategy of making economic theory consistently algorithmic. The most convincing and admirably transparent example of this research strategy is the one adopted by Michael Rabin to transform the celebrated Gale-Stewart Game to an Algorithmic Game and, then, to characterise its effective content. A full discussion of this particular episode in the development of Computable Economics is given in [48] and [50, chapter 7]. However, the various subsections below simply report some of the results I have obtained, on uncomputability, non-constructivity and undecidability in economic theory, without, in each case, describing the background motivation, the precise research and proof strategy that was developed to obtain the result and the full extent of the implications for Computable Economics.

[11] *Maury Osborne, with the clarity that can only come from a rank outsider to the internal paradoxes of the dissonance between economic theory and applied economics, noted pungently:* "There are numerous other paradoxical beliefs of this society [of economists], consequent to the difference between discrete numbers... in which data is recorded, whereas the theoreticians of this society tend to think in terms of real numbers. ...No matter how hard I looked, I never could see any actual real [economic] data that showed that [these solid, smooth, lines of economic theory] ... actually could be observed in nature. ... At this point a beady eyed Chicken Little might ... say, 'Look here, you can't have solid lines on that picture because there is always a smallest unit of money ... and in addition there is always a unit of something that you buy...[I]n any event we should have just whole numbers of some sort on [the supply-demand] diagram on both axes. The lines should be dotted. ... Then our mathematician Zero will have an objection on the grounds that if we are going to have dotted lines instead of solid lines on the curve then there does not exist any such thing as a slope, or a derivative, or a logarithmic derivative either. .... If you think in terms of solid lines while the practice is in terms of dots and little steps up and down, this misbelief on your part is worth, I would say conservatively, to the governors of the exchange, at least eighty million dollars per year. [29, pp. 16–34].

[12] *Prefaced, elegantly and appositely, with a typically telling observation by Samuel Johnson*: "Nothing amuses more harmlessly than computation, and nothing is oftener applicable to real business or speculative enquiries. A thousand stories which the ignorant tell, and believe, die away at once when the computist takes them in his grip" [3, p. vii] *Surely, this is simply a more literary expression of that famous credo of Leibniz*: "...[W]hen a controversy arises, disputation will no more be needed between two philosophers than between two computers. It will suffice that, pen in hand, they sit down ... and say to each other: Let us calculate." [19].

[13] An acute observation by one of the referees requires at least a nodding mention. The referee wondered why the paper did not consider the famous 'Socialist Calculation Debate', emerging, initially, from careless remarks by Pareto about the computing capabilities of a decentralised market. This issue later – in the 1920s and 1930s, revisited by one of the protagonists as late as 1967 – became a full-blooded debated about the feasibility of a decentralised planning system, an oxymoron if ever there was one. However, the reason I am not considering the debate in this paper is twin-pronged: firstly, it was, essentially, about analog computing (although Oskar Lange muddied the issue in his revisit to the problem in 1967 in the *Dobb Festschrift*); secondly, it is less about computability than computational complexity. For reasons of space, I have had to refrain from any serious consideration of any kind of complexity issue – whether of the computational or algorithmic variety.

2.1. Undecidability (and Uncomputability) of maximizing choice

All of mathematical economics and every kind of orthodox game theory rely on some form of formalized notion of individually 'rational behaviour' Two key results that I derived more than two decades ago, are the following, stated as theorems[14]:

Theorem 1. *Rational economic agents in the sense of economic theory are equivalent to suitably indexed Turing Machines; i.e, decision processes implemented by rational economic agents – viz., choice behaviour – is equivalent to the computing behaviour of a suitably indexed Turing Machine.*

Put another way, this theorem states that the process of rational choice by an economic agent is equivalent to the computing activity of a suitably programmed Turing Machine.

Proof. Essentially by construction from first principles (no non-constructive assumptions are invoked). See [49]. □

An essential, but mathematically trivial, implication of this Theorem is the following result.

Theorem 2. *Rational choice, understood as maximizing choice, is undecidable.*

Proof. The procedure is to show, again by construction, that preference ordering is effectively undecidable. See [50, Section 3.3] for the details. □

Remark 3. These kinds of results are the reasons for the introduction of formalized concepts of bounded rationality and satisficing by Herbert Simon. Current practice, particularly in varieties of experimental game theory, to identify boundedly rational choice with the computing activities of a Finite Automaton are completely contrary to the theoretical constructs and cognitive underpinnings of Herbert Simon's framework. The key mistake in current practice is to divorce the definition of bounded rationality from that of satisficing. Simon's framework does not refer to the orthodox maximizing paradigm; it refers to the recursion theorist's and the combinatorial optimizer's framework of decision procedures.

2.2. Computable and decidable paradoxes of excess demand function

2.2.1. Algorithmic undecidability of a computable general equilibrium

The excess demand function plays a crucial role in all aspects of computable general equilibrium theory and, indeed, in the foundation of microeconomics. Its significance in computable general equilibrium theory is due to the crucial role it plays in what has come to be called *Uzawa's Equivalence Theorem* (cf. [44], §11.4) – the equivalence between a Walrasian Equilibrium Existence Theorem (**WEET**) and the Brouwer Fixed Point Theorem. The finesse in one half of the equivalence theorem, i.e., that **WEET** implies the Brouwer fix point theorem, is to show the feasibility of devising a continuous excess demand function, $X(p)$, satisfying *Walras' Law*[15] (and homogeneity), from an arbitrary continuous function, say $f(\cdot): S \to S$, where S is the unit simplex in $\mathbb{R}^N$, such that the equilibrium price vector, p^*, implied by $X(p)$ is also the fix point for $f(\cdot)$, from which it is 'constructed'.

I am concerned, firstly, with the recursion theoretic status of $X(p)$. Is this function computable for arbitrary $p \in S$? Obviously, if it is, then there is no need to use the alleged constructive procedure to determine the Brouwer fix point (or any of the other usual topological fix points that are invoked in general equilibrium theory and CGE Modelling) to locate the economic equilibrium implied by **WEET**.

The key step in proceeding from a given, arbitrary, $f(\cdot): S \to S$ to an excess demand function $X(p)$ is the definition of an appropriate scalar:

$$\mu(p) = \frac{\sum_{i=1}^{n} p_i f_i\left[\frac{p}{\lambda(p)}\right]}{\sum_{i=1}^{n} p_i^2} = \frac{p \cdot f(p)}{|p|^2}, \tag{1}$$

where:

$$\lambda(p) = \sum_{i=1}^{n} p_i. \tag{2}$$

[14] A perceptive referee wondered '*why rational choice can be interpreted as an equivalent of the whole class of Turing machines, maybe we should consider in this context only some subclass of Turing machines (e.g. polynomial-time Turing machines)*' . This is an obviously important point, which I have addressed in other writings, in the spirit of Herbert Simon's research program on *boundedly rational choice* and *satisficing decision problems* by economic agents. However, the question of restricting the class of Turing machines to a 'relevant' subclass becomes pertinent when one begins to focus attention on the 'complexity of choice', implemented in empirically relevant contexts. Unfortunately, to go into details of this aspect will require me to expand this paper beyond the allocated constraints and its limited scope.

[15] For the benefit of those whose memories may well require some rejuvenations, a simple, succinct, version of *Walras' Law* can be stated as follows:

$$\forall p, \quad p \cdot X(p) = \sum_{i=1}^{N} p_i \cdot X_i(p) = 0.$$

From (1) and (2), the following excess demand function, $X(p)$, is defined:

$$x_i(p) = f_i\left(\frac{p}{\lambda(p)}\right) - p_i\mu(p), \tag{3}$$

i.e.,

$$X(p) = f(p) - \mu(p)p. \tag{4}$$

I claim that the procedure that leads to the definition of (3) [or, equivalently, (4)] to determine p^* is provably *undecidable*. In other words, the crucial scalar in (1) cannot be defined recursion theoretically (and, *a fortiori*, constructively) to effectivize a sequence of projections that would ensure convergence to the equilibrium price vector.

Clearly, given any $p \in S$, all the elements on the r.h.s of (1) and (2) *seem* to be well defined. However, $f(p)$ is not necessarily computable (nor meaningfully constructive) for arbitrary $p \in S$. Restricting the choice of $f(\cdot)$ to the partial recursive functions may most obviously violate the assumption of *Walras' Law*. Therefore, I shall show that it is impossible to devise an algorithm to define (3) [or (4)] for an arbitrary $f(p)$, such that the equilibrium p^* for the defined excess demand function is also the fix point of $f(\cdot)$. If it were possible, then the famous *Halting Problem for Turing Machines* can be solved, which is an impossibility.

Theorem 4. *$X(p^*)$, as defined in (3) [or (4)] above is undecidable; i.e., cannot be determined algorithmically.*

Proof. Suppose, contrariwise, there is an algorithm which, given an arbitrary $f(\cdot) : S \to S$, determines $X(p^*)$. This means, therefore, that the given algorithm determines the equilibrium p^* implied by **WEET**. In other words, given the arbitrary initial conditions $p \in S$ and $f(\cdot) : S \to S$, the assumption of the existence of an algorithm to determine $X(p^*)$ implies that its *halting* configurations are decidable. But this violates the *undecidability of the Halting Problem for Turing Machines*. Hence, the assumption that there exists an algorithm to determine – i.e., to construct – $X(p^*)$ is untenable. □

Remark 5. The algorithmically important content of the proof is the following. Starting with an arbitrary continuous function mapping the simplex into itself and an arbitrary price vector, the existence of an algorithm to determine $X(p^*)$ entails the feasibility of a procedure to choose price sequences in some determined way to check for p^* and to *halt* when such a price vector is found. Now, the two scalars, μ and λ are determined once $f(\cdot)$ and p are given. But an arbitrary initial price vector p, except for flukes, will not be the equilibrium price vector p^*. Therefore the existence of an algorithm would imply that there is a systematic procedure to choose price vectors, determine the values of $f(\cdot)$, μ and λ and the associated excess demand vector $X(p; \mu, \lambda)$. At each determination of such an excess demand vector, a projection of the given, arbitrary, $f(p)$, on the current $X(p)$, for the current p, will have to be tried. This procedure must continue till the projection for a price vector results in excess demands that vanish for some price. Unless severe recursive constraints are imposed on price sequences – constraints that will make very little economic sense – such a test is algorithmically infeasible. In other words, given an arbitrary, continuous, $f(\cdot)$, there is no procedure – algorithm (constructive or recursion theoretic) – by which a sequence of price vectors, $p \in S$, can be *systematically* tested to find p^*.

Corollary 6. *The Recursive Competitive Equilibrium (***RCE***)of New Classical Macroeconomics – Recursive Macroeconomic Theory – is uncomputable.*

Remark 7. See [55] (Definition 2, p. 16) for a detailed definition of **RCE** and hints on proving this Corollary.

Remark 8. The proof procedure is almost exactly analogous to the one used above to show the *recursive undecidability* of a computable general equilibrium – with one significant difference. Instead of using the *unsolvability of the Halting problem for Turing Machines* to derive the contradiction, I use a version of *Rice's Theorem*.

Remark 9. The more empirically relevant question would be to consider the question of the feasibility of approximating $X(p^*)$. This, like the issue of considering a subclass of Turing machines to formalize empirically relevant rational choice procedures, falls under a burgeoning research program on the complexity of computing varieties of economic and game theoretic equilibria. Since I have had to limit the scope of my considerations in this paper to questions of computability and decidability in principle, I must – albeit reluctantly – refrain from going further into these issues. An excellent reference on the problem, via a discussion of the complexity of computing Nash equilibria, can be found in [30].

2.2.2. Recursive undecidability of the excess demand function

The nature of economic data and the parameters underpinning the mechanisms generating the data – as noted by Stone and Osborne, if any substantiation of the obvious must be invoked via the wisdom of eminence – should imply that the excess demand function is a Diophantine relation. Suppose we take economic reality, Stone, Osborne and Smale seriously assume that all variables and parameters defining the excess demand functions are, in fact, integer or rational valued (with the former, in addition, being non-negative, as well).

Indeed, Smale has brilliantly articulated the perplexity of the Arrow–Debreu 'subversion' of the classic problem of supply-demand equilibrium as a system of equations to be solved for non-negative valued, rational-number variables, into a system of inequalities whose consistency is proved by blind appeals to non-constructive fix point theorems and, thereby, an existence of a set of equilibrium prices is asserted:

> "We return to the subject of equilibrium theory. The existence theory of the static approach is deeply rooted to the use of the mathematics of fixed point theory. Thus one step in the liberation from the static point of view would be to **use a mathematics of a different kind**. Furthermore, proofs of fixed point theorems traditionally use difficult ideas of algebraic topology, and this has obscured the economic phenomena underlying the existence of equilibria. Also the economic equilibrium problem presents itself most directly and with the most tradition not as a fixed point problem, but as an *equation*, supply equals demand. **Mathematical economists have translated the problem of solving this equation into a fixed point problem**.
> I think it is fair to say that for the main existence problems in the theory of economic equilibrium, **one can now bypass the fixed point approach and attack the equations directly to give existence of solutions, with a simpler kind of mathematics** and even mathematics with dynamic and **algorithmic** overtones."
> [42, p. 290]; bold emphasis added.

To 'attack the equations directly,' taking into account the obvious constraints on variables and parameters in economics – i.e., that the variables have to be non-negative, rational numbers and the parameters at least the latter (and if they are not the former, then there are feasible transformations to make them so, cf. [24, chapter 1]) – is actually a very simple matter. I shall only indicate the skeleton of such an approach here. Full details are available in the author's other writings.

Now, dividing the vector of parameters and variables characterizing the excess demand function X into two parts, a vector a of parameters and the vector of prices, x, we can write a relation of the form (in supply-demand **equilibrium**)

$$X(a_1, a_2, \dots, a_n, x_1, x_2, \dots, x_m) = 0,$$

where:

Definition 10. X is a polynomial[16] with integer (or rational number) coefficients with respect to the **parameters** $a_1, a_2, \dots, a_n$ and **variables** $x_1, x_2, \dots, x_m$ (which are also non-negative) and is called a **parametric Diophantine equation**.

Definition 11. X in Definition 10 defines a set F of the parameters for which there are values of the unknowns such that:

$$\langle a_1, a_2, \dots, a_n \rangle \in \mathsf{F} \Longleftrightarrow \exists x_1, x_2, \dots, x_m [X(a_1, a_2, \dots, a_n, x_1, x_2, \dots, x_m) = 0].$$

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

Problem 12. A set, say $\langle a_1, a_2, \dots, a_n \rangle \in \mathsf{F}$, is given. Determine if this set is Diophantine. If it is, find a Diophantine representation for it.

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

$$R(a_1, a_2, \dots, a_n) \Longleftrightarrow \exists x_1, x_2, \dots, x_m [X(a_1, a_2, \dots, a_n, x_1, x_2, \dots, x_m) = 0].$$

Next, how can we talk about the *solvability* of a Diophantine representation of the excess demand relation? This is where undecidability (and uncomputability) will enter – through a remarkable connection with recursion theory, summarized in the next Proposition:

Proposition 13. *Given any parametric Diophantine equation, X, it is possible to construct a Turing Machine, M, such that M will eventually* **Halt***, beginning with a representation of the parametric n-tuple,* $\langle a_1, a_2, \dots, a_n \rangle$, **iff** *X in Definition 10 is solvable for the unknowns,* $x_1, x_2, \dots, x_m$.

But, then, given the famous result on the *Unsolvability of the Halting problem for Turing Machines*, we are forced to come to terms with the algorithmic unsolvability of the excess demand function as a Diophantine equations.

2.3. Non-constructivity of welfare theorems

Let me conclude this section by showing, in a very general way, the role played by the *Hahn–Banach Theorem* in proving the crucial '*Second Welfare Theorem*' in economics. I shall refer to the way it is presented, proved and discussed in [22] (although I could equally well have chosen the slightly simpler and clearer exposition in [44]). The *Second Welfare Theorem*

[16] I am restricting the excess demand functions to be polynomials simply to be consistent with the traditional definition. The more mathematically satisfying approach may have been to consider, in the general case, arbitrary functions from $\mathbb{N}$ to $\mathbb{N}$. I am indebted to a referee's observation regarding the need to clarify this point.

establishes the proposition that *any Pareto optimum can, for suitably chosen prices, be supported as a competitive equilibrium*. The role of the Hahn–Banach theorem in this proposition is in establishing *the suitable price system*.

Lucas and Stokey state 'their' version of the Hahn–Banach Theorem in the following way[17]:

Theorem 14. *Geometric form of the Hahn–Banch Theorem. Let S be a normed vector space; let $A, B \subset S$ be convex sets. Assume:*

(a) Either B has an interior point and $A \cap \mathring{B} = \emptyset$, ($\mathring{B}$: closure of B);
(b) Or, S is finite dimensional and $A \cap B = \emptyset$;

Then: $\exists$ *a continuous linear functional* ϕ, *not identically zero on* S, *and a constantc s.t:*$\phi(y) \leqslant c \leqslant \phi(x)$, $\forall x \in A$ *and* $\forall y \in B$.

Next, I state the economic part of the problem in merciless telegraphic form as follows.

There are I consumers, indexed $i = 1, \ldots, I$.

S is a vector space with the usual norm.

Consumer i chooses from commodity set $X_i \subseteq S$, evaluated according to the utility function $u_i : X_i \to \Re$.

There are J firms, indexed $j = 1, \ldots, J$.

Choice by firm j is from the technology possibility set, $Y_j \subseteq S$; (evaluated along profit maximizing lines).

The mathematical structure is represented by the following absolutely standard assumptions:

1. $\forall i,\ X_i$ *is convex*;
2. $\forall i$, *if* $x, x' \in C_i, u_i(x) > u_i(x')$, *and if* $\theta \in (0,1)$, *then* $u_i[\theta x + (1-\theta)x'] > u_i(x')$;
3. $\forall i, u_i : X_i \to \Re$ *is continuous*;
4. *The set* $Y = \sum_j Y_j$ *is convex*;
5. *Either the set* $Y = \sum_j Y_j$ *has an interior point*, or *S is finite dimensional*;

Then, the *Second Fundamental Theorem of Welfare Economics* is:

Theorem 15. *Let assumptions 1–5 be satisfied; let* $[(x_i^0), (y_j^0)]$ *be a Pareto Optimal allocation; assume, for some* $h \in \{\bar{1}, \ldots, \bar{I}\}$, $\exists \hat{x}_h \in X_h$ *with* $u_h(\hat{x}_h) > u_h(x_h^0)$. *Then* $\exists$ *a continuous linear functional* $\phi : S \to \Re$, *not identically zero on S, s.t:*

(a) $\forall i, x \in X_i$ *and* $u_i(x) \geqslant u_i(x^0) \Rightarrow \phi(x) \geqslant \phi(x_i^0)$.
(b) $\forall j, y \in Y_j \Rightarrow \phi(j) \leqslant \phi(y_i^0)$.

Anyone can see, as anyone would have seen and has seen for the last 70 years, that an economic problem has been 'mangled' into a mathematical form to conform to the structure and form of a mathematical theorem. This was the case with the way Nash formulated his problems; the way the Arrow–Debreu formulation of the general equilibrium problem was made famous; and legions of others.

It is a pure mechanical procedure to verify that the assumptions of the economic problem satisfy the conditions of the Hahn–Banach Theorem and, therefore, the powerful *Second Fundamental Theorem of Welfare Economics* is 'proved'.[18]

The Hahn–Banach theorem does have a constructive version, but only on subspaces of *separable* normed spaces. The standard, 'classical' version, valid on nonseparable normed spaces depends on *Zorn's Lemma* which is, of course, equivalent to the axiom of choice, and is therefore, non-constructive.[19]

Schechter's perceptive comment on the constructive Hahn–Banach theorem is the precept I wish economists with a numerical, computational or experimental bent should keep in mind ([38, p. 135]; italics in original; bold emphasis added):

> "[O]ne of the fundamental theorems of classical functional analysis is the *Hahn–Banach Theorem;* ... some versions assert the existence of a certain type of linear functional on a normed space X. The theorem is inherently nonconstructive, but a constructive proof can be given for a variant involving normed spaces X that are ***separable***– i.e., normed spaces that have a countable dense subset. ***Little is lost in restricting one's attention to separable spaces,***[20] ***for in applied math most or all normed spaces of interest are*** separable. ***The constructive version of the Hahn–Banach Theorem is more complicated, but it has the advantage that it actually*** *finds* ***the linear functional in question***."

[17] Essentially, the 'classical' mathematician's Hahn–Banach theorem guarantees the extension of a bounded linear functional, say ρ, from a linear subset Y of a separable normed linear space, X, to a functional, η, on the whole space X, with exact preservation of norm; i.e., $|\rho| = |\eta|$. The constructive Hahn–Banach theorem, on the other hand, cannot deliver this pseudo-exactness and preserves the extension as: $|\rho| \leqslant |\eta| + \varepsilon$, $\forall \varepsilon > 0$. The role of the positive ε in the constructive version of the Hahn–Banach theorem is elegantly discussed by Nerode, Metakides and Constable in their beautiful piece in the Bishop Memorial Volume [27, pp. 85–91]. Again, compare the difference between the 'classical' IVT and the constructive IVT to get a feel for the role of ε (see Ref. [53]).

[18] To the best of my knowledge an equivalence between the two, analogous to that between the Brouwer fix point theorem and the Walrasian equilibrium existence theorem, proved by Uzawa [47], has not been shown.

[19] This is not a strictly accurate statement, although this is the way many advanced books on functional analysis tend to present the Hahn–Banach theorem. For a reasonably accessible discussion of the precise dependency of the Hahn–Banach theorem on the kind of axiom of choice (i.e., whether countable axiom of choice or the axiom of dependent choice), see [26]. For an even better and fuller discussion of the Hahn–Banach theorem, both from 'classical' and constructive points of view, Schechter's encyclopedic treatise is unbeatable [38].

[20] *However, it must be remembered that Ishihara [17] has shown the constructive validity of the Hahn–Banach theorem also for uniformly convex spaces.*

So, one may be excused for wondering, why economists rely on the 'classical' versions of these theorems? They are devoid of numerical meaning and computational content. Why go through the rigmarole of first formalizing in terms of numerically meaningless and computationally invalid concepts to then seek impossible and intractable approximations to determine uncomputable equilibria, undecidably efficient allocations, and so on?

Thus my question is: why should an economist *force* the economic domain to be a normed vector space? Why not a *separable normed vector space*? Isn't this because of pure ignorance of constructive mathematics and a carelessness about the nature and scope of fundamental economic entities and the domain over which they should be defined?

2.4. Noneffectivity of games

The most celebrated exercise in Computable Economics or what has recently come to be called Algorithmic Game Theory is Michael Rabin's famous result.

Theorem 16 [35]. *There are games in which the player who* **in theory** *can always win cannot do so* **in practice** *because it is impossible to supply him with* **effective instructions** *regarding how he should play in order to win.*

Rabin's strategy to obtain this result is the paradigmatic example of what I conceive to be the typical research program of a Computable Economist. Essentially, the idea is to consider any formal, orthodox, game theoretic example and strip it away of all non-effective considerations and, then, ask whether the remaining scaffolding is capable of being algorithmically decidable in an empirically meaningful sense. A complete description and explanation of Rabin's strategy is fully discussed in [48].

But at the time I first studied Rabin's example – about twenty years ago – and extracted his implicit research strategy as a paradigmatic example for the work of a Computable Economist, I missed an important aspect: its place in a particular tradition of game theory. It was only in very recent times that I have been able to place it in the original tradition of game theory – the tradition that began with Zermelo, before it was 'subverted' by the von Neumann–Nash subjective approach which dominates all current frontiers of research in game theory, at least in the citadel of economic theory (including its computational and experimental branches). My starting point for the tradition that came to a transitory completion, therefore, would be Zermelo's celebrated lecture of 1912 [58] and his pioneering formulation of an adversarial situation into an *alternating game* and its subsequent formulation and solution as a mini–max problem by Jan Mycielski in terms of *alternating the existential and universal quantifiers*.

The Zermelo game has no subjective component of any sort. It is an entirely objective game of perfect information, although it is often considered part of the orthodox game theoretic tradition. Let me describe the gist of the kind of game considered by Zermelo, first. In a 2-player game of perfect information, alternative moves are made by the two players, say A and B. The game, say as in Chess, is played by each of the players 'moving' one of a finite number of counters available to him or her, according to specified rules, along a 'tree' - in the case of Chess, of course, on a board of fixed dimension, etc. Player A, say, makes the first move (perhaps determined by a chance mechanism) and places one of the counters, say $a_0 \in A_0$, on the designated 'tree' at some allowable position (again, for evocative purposes, say as in Chess or any other similar board game); player B, then, observes the move made by A – i.e., observes, with perfect recall, the placement of the counter a_1 – and makes the second move by placing, say $b_1 \in B_1$, on an allowable position on the 'board'; and so on. Let us suppose these alternating choices terminate after Player B's nth move; i.e., when $b_n \in B_n$ has been placed in an appropriate place on the 'board'.

Definition 17. A **play** of such a game consists of a sequence of such alternative moves by the two players

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

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

Suppose we define the set α as the set of plays which are wins for player A; and, similarly, the set β as the set of plays which are wins for player B.

Definition 18. A strategy is a function from any (finite) string of natural numbers as input generates a single natural number, say σ, as an output.

Definition 19. A game is **determined** if one of the players has a winning strategy; i.e., if either $\sigma \in \alpha$ or $\sigma \in \beta$.

Theorem 20. *Zermelo's Theorem: $\exists$ a winning strategy for player A, whatever is the play chosen by B; and vice versa for B.*[21]

Remark 21. This is Zermelo's version of a minimax theorem in a perfect recall, perfect information, game.

[21] *One referee found this way of stating the celebrated 'Zermelo Theorem' somewhat 'unclear'. The best I can do, to respond to the referee's gentle – albeit indirect – admonition to state it more intuitively is to refer to the excellent pedagogical discussion, and a particularly lucid version, of the Zermelo Theorem in [45].*

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

> "[My] inability [to prove the minimax theorem] was a consequence of the ignorance of Zermelo's paper [58] in spite of its having been published in 1913. .... J von Neumann was aware of the importance of the minimax principle (cf. [57]); it is, however, *difficult to understand the absence of a quotation of Zermelo's lecture in his publications*."
> Steinhaus ([45, p. 460]; italics added)

Why didn't von Neumann refer, in 1928, to the Zermelo-tradition of alternating games? The tentative answer to such a question is a whole research program in itself and I will simply have to place it on an agenda and pass on. I have no doubts whatsoever that any serious study to answer this almost rhetorical question will reap a rich harvest of further cons perpetrated by the mathematical economists, perhaps inadvertently. The point I wish to make is something else and has to do with the axiom of choice and its place in *economic conning*. So, let me return to this theme.

Mycielski (cf., [45, pp. 460–461]) formulated the Zermelo minimax theorem in terms of alternating logical quantifiers as follows[22]:

$$\begin{aligned}&\sim \{\cup_{a_0\in A_0}\cap_{b_1\in B_1}\ldots\cup_{a_n\in A_{n-1}}\quad \cap_{b_n\in B_n}(a_0b_1a_2b_3\ldots a_{n-1}b_n)\}\in\alpha\\ &\quad\Rightarrow \{\cap_{a_0\in A_0}\cup_{b_1\in B_1}\ldots\cap_{a_n\in A_{n-1}}\quad \cup_{b_n\in B_n}(a_0b_1a_2b_3\ldots a_{n-1}b_n)\}\notin\beta.\end{aligned}$$

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

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

The above definitions, descriptions and structures define, therefore, an *Arithmetical Game* of length *n* (cf. [50, pp. 125–126] for a formal definition). Stating the Zermelo theorem in a more formal and general form, we have:

Theorem 22. *Arithmetical Games of finite length are determined.*

Remark 23. However, the qualifications required by Harrop's Theorem (see below) have to be added as 'constructive' caveats to this result.[23]

The more general theorem, for games of arbitrary (non-finite) length, can be proved by standard diagonalization arguments and is[24]:

Theorem 24. *Arithmetical Games on any countable set or on any set which has a countable complement is determined.*

Now, enter the axiom of choice! Suppose we allow any unrestricted sets α and β. Then, for example if they are *imperfect sets* [25], the game is not determined. If we work within ZFC, then such sets are routinely acceptable and lead to games that cannot be determined – even if we assume perfect information and perfect recall. Surely, this is counter-intuitive? For this reason, this tradition in game theory chose to renounce the axiom of choice and work with an alternative axiom that restricts the class of sets within which arithmetical games are played. The alternative axiom is the *axiom of determinacy*, introduced by Steinhaus:

Axiom 25. The **Axiom of Determinacy**: Arithmetical Games on every subset of the Baire line[26] is determined.

The motivation given by Steinhaus [45, pp. 464–465] is a salutary lesson for mathematically minded economists or economists who choose to accept the axiom of choice on 'democratic' principles or economists who are too lazy to study carefully the economic meaning of accepting a mathematical axiom:

> "It is known that [the Axiom of Choice] produces such consequences as the decomposition of a ball into five parts which can be put together to build up a new ball of twice the volume of the old one [the Banach–Tarski paradox], a result considered as paradoxical by many scientists. There is another objection: how are we to speak of perfect information for [players] A and B if it is impossible to verify whether both of them think of the same set when they speak of ["α"]? *This*

22 Discerning and knowledgeable readers will recognize, in this formulation, the way Gödel derived undecidable sentences.
23 I am indebted to a referee for making me think about this important point.
24 The real time paradox of implementing an infinite play is easily resolved (cf., [45, p. 465]; [50, chapter 7]).
25 A set F is a *perfect set* if it is a *closed set in which every point is a limit point*.
26 A Baire line is an irrational line which, in turn, is a line obtainable from a continuum by removing a countable dense subset.

impossibility is inherent in every set having only [the Axiom of Choice] as its certificate of birth. In such circumstances it is doubtful whether human beings will ever play really [an infinite game].
All these considerations impelled me to place the blame on the Axiom of Choice. Sixty years of the theory of sets have elapsed since this Axiom was proclaimed, and some ideas have ... convinced me that a purely negative attitude against [the Axiom of Choice] would be dangerous to propose. Thus I have chosen the idea of replacing [the Axiom of Choice] by the [above Axiom of Determinacy].
italics added.

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

3. *Towards* unconventional computational models in economic theory

My personal view – indeed, *vision* – has been evolving, very gradually, towards a mathematical economics that is formalized exclusively in terms of strict Brouwerian Constructive Mathematics. To make this case, from methodological and epistemological points of view would require more space than I have at my disposal and, moreover, would necessitate a widening of the scope of the paper. Therefore, I shall only make a few salient observations that may indicate the reasons for this *vision*.

Even more than a justification from an algorithmic point of view, for which it is possible to make almost equally strong cases for either a classical computability or Bishop-style constructivist approaches towards a consistent quantitative formalization of economic theory, there is the epistemological question of the meaning of proving economic propositions using classical, non-constructive, logic. To the best of my knowledge, there is only one serious, fundamental, work in economic theory that is consistently constructive in the way the economic propositions in it are proved: Piero Sraffa's *magnum opus* [43]. I have dissected the methods of proof in this elegant, terse, text and made my case for a constructive epistemology in the formalization of economic theory (see [56]). However, from a strictly methodological point of view, I remain indifferent between formalizing economic entities and processes recursion theoretically or constructively, either of them in any of the many variants in which they are being developed these days.

Whether methodologically or epistemologically, a formalization of economic theory via classical recursion theory or any variant of constructive mathematics, will have to lead to fairly drastic rethinking of fundamental issues in economics from policy and empirical points of view. This is primarily because the welfare theorems and computable properties of equilibria lose their quantitative underpinnings and become almost mystically sustained. The issues I have chosen to dissect in this paper suggest this implication, sometimes explicitly, but more than often only implicitly. The following remarks are somewhat limited further elaborations of these issues, but primarily from a methodological standpoint.[27]

I am not in any way competent in any form of *unconventional models of computation*. The remarks below should, therefore, be taken as reflections of a Computable Economist who is deeply committed to making economics algorithmically meaningful – from a methodological point of view – so that computation and experimentation can be seriously and rigorously underpinned in the honest mathematics of the computer, whether digital or analog.

My original motivation for coining the term *Computable Economics*, to encapsulate the kind of issues raised by Arrow and Stone, mentioned in the opening section, was quite a different kind of perplexity. It was a perplexity grounded in *proof theory* and *model theory*.[28] There is no better way to summarize these originating concerns, from the perspective of a *Computable Economist*, than to recall two deep and subtle caveats added by two of the pioneers of computability theory, Alonzo Church and Emil Post, in their pathbreaking contributions to the origins of classical and higher recursion theory. Their insights suggested a deeper interplay between computability and constructivity than is normally understood or acknowledged by any of the social scientists now deeply immersed in developing the frontiers of computable economics, algorithmic game theory and algorithmic statistics.

To place these insights in the context of the present paper, let me state a conjecture in the form of a theorem[29]:

Theorem 26. *Nash equilibria of finite games are constructively indeterminate.*[30]

[27] The brief few opening paragraphs of this concluding section were added in response to observations made by two very helpful referees.
[28] To a large extent in their incarnation as *constructive* and *non-standard* analysis.
[29] These thoughts were inspired entirely by a reading of a fundamental series of results by Francisco Doria and his collaborators (cf. [10,11]), which introduced me to Harrop's important work [15]. These papers put in proper perspective my initial proof- and model-theoretic perplexities when faced with assumptions and proofs in mathematical economics.
[30] *The proof of the existence of Nash equilibria, given in standard textbooks, rely on one or another of the nonconstructive fix point theorems (Brouwer's, Kakutani's, etc.). Since these, in turn, are proved invoking the Bolzano–Weierstrass theorem, which is intrinsically non-constructive, due to essentially undecidable disjunctions, the proof of the non-constructivity of Nash equilibria for general infinite games is 'easy' – and 'cheap' – at least from one point of view.*

Proof. Apply Harrop's Theorem [15, p. 136]. □

To make sense of this 'theorem', and its proof using 'Harrop's Theorem', it is necessary to understand the subtle differences between computability as understood in (classical) recursion theory, accepting Church's Thesis[31] and computations by algorithmic mathematics as specified in varieties of constructive mathematics, particularly intuitionistic constructive mathematics. I shall not enter into the deep domain of the foundations of mathematics and its thorny controversies here – although I, too, have my view and take my 'sides' and find myself, as always, in the minority! The subtle issues that have to be clarified, to make sense of the above almost counter-intuitive 'theorem', were made clear in Charles Parson's 'review', [31] of Harrop's result.[32] The issues are the bearing of Harrop's Theorem on, whether:

1. Every finite set is recursive.
2. Every recursive set is effectively decidable.
3. Every finite set is effectively decidable.

This neat three-point characterization of Harrop's theorem, by Parsons, is a summary of the following explanation of the implications of his Theorem by Harrop himself:

> "Although it is correct classically to state that the values of a partial function computed by a machine of arbitrary Gödel number form a finite set or an infinite set, this statement should *not* be used together with the statements that *every finite set is recursive* and that *every recursive set has an intuitively effective test for membership* (*converse of Church's thesis*) to conclude that if we know classically that a certain integer is the Gödel number of a machine which computes a function with a finite range then automatically there is an intuitively effective test for membership of that range. Our theorem shows that as far as the general case is concerned there is no recursive method for obtaining machines which will compute the characteristic functions which would all individually be obtainable if there were such intuitively effective tests. There may in any particular case be an intuitively effective test."
> [31, p. 139]; italics added.

Now how this links up with the early contributions by Church and Post to the defining frameworks for classical and higher recursion theory, can easily be gauged by two caveats they made in two of their classic writings ([5,33], respectively). Church observed [5, p. 351]:

> "It is clear that for any recursive function of positive integers there exists an algorithm using which any required particular value of the function can be effectively calculated."

To this almost innocuous observation – 'innocuous' at least to the modern 'classical' recursion theorist – Church added the subtle (I almost wrote 'slightly devious' – but no one can possibly accuse Alonzo Church of being 'devious'!!) caveat:

> "The reader may object that this algorithm cannot be held to provide an effective calculation of the required particular values of F^i *unless the proof is constructive* that the required equation $f^i_{n_i}(k^i_1, ,k^i_2, \dots, k^i_n) = k^i$ will ultimately be found. But if so this merely means that he should take the existential quantifier which appears in our definition of a set of recursion equations *in a constructive sense*. *What the criterion of constructiveness shall be is left to the reader*."
> [5, p. 351], footnote 10; italics added.

Post, analogously, first states what may seem obvious to a modern 'classical' recursion theorist *([*33, p. 469*]*; italics in the original):

> "Clearly, *any finite set of positive integers is recursive*. For if $n_1, n_2, \dots, n_\nu$ are the integers in question, we can test n being, not being, in the set by directly comparing it with $n_1, n_2, \dots, n_\nu$."

But, then, goes on with *his* caveat:

> "*The mere*[33] *existence of a general recursive function defining the finite set is in question*. Whether, given some definition of the set, we can *actually discover* what the members thereof are, is *a question for a theory of proof* rather than for the present theory of finite processes. For sets of finite sets the situation is otherwise, ..."
> [33, p. 469], footnote 10; italics added.

Harrop's theorem's clarifies these caveats and drives the wedge between computabilty, classically understood, and algorithms as understood by (at least) some constructivists. I believe this 'wedge' allows the Computable Economist to seek 'unconventional models of computation' – i.e., going beyond or, at least, sideways from, the phenomenological limits imposed by the Church–Turing Thesis.

31 Or the Church–Turing Thesis.
32 And the subsequent development and simplification of Harrop's result and proof by Jiří Hořješ [16].
33 *I suspect a reading of this sentence by replacing 'mere' with 'very' will make the sense more accurate!*

Whether the institutions and mechanisms of a market economy make feasible such 'unconventional models of computation', depending on the 'wedge' between the Church–Turing Thesis and its 'converse', is not something that is formally decidable – let alone algorithmically decidable. But that does not mean the market mechanism is not actually involved in 'unconventional computation'. However, to make sense of this question it will be necessary to algorithmise orthodox economic theory – or, even better, develop an algorithmic economics, *ab initio*. To this extent I find it sobering to contemplate on an analogy between the lessons to be learned from Abel's impossibilities – and Gödel's:

> "Why was it that, in His infinite wisdom, God should have created algebraic solutions for general equations of the first four degrees, but not for the equation $ax^5 + bx^4 + cx^3 + dx^2 + ex + f = 0$? Is it the case that human powers are too limited to understand such a transcendent matter? Or have we simply not yet ascended to the 'meta-mathematical' level in which comprehension will be forthcoming? If Abel's proof was spared such conundrums Gödel's theorem unfortunately was not;.... For while Gödel's theorem looks like – and was initially intended to be seen as – a *closure*, it has been widely interpreted as a *transitional* impossibility proof."
> [40, p. 167]; italics in original

The 'fallacy of composition' that drives a felicitous wedge between micro and macro, between the individual and the aggregate, and gives rise to *emergent* phenomena in economics, in non-algorithmic ways – as conjectured, originally more than a century and a half ago – by Mill [25] and Lewes [20], and codified by Lloyd Morgan in his *Gifford Lectures* [21] – may yet be tamed by *unconventional models of computation*.

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Quite serendipitously, I am in the happy position of being able to pay long overdue acknowledgements to four of my "fellow-invitees" at this meeting: Ann Condon, Barry Cooper, Chico Doria and Karl Svozil – although they are, almost certainly, unaware of the kind of ways in which I have benefitted from their wisdom and scholarship, over the years (cf. in particular, [7,8,46], respectively). Indeed, in the case of Barry Cooper, I am also deeply indebted to his own distinguished teacher, R.L. Goodstein, whose works have had a lasting influence in the way I think about the kind of mathematics that is suitable for mathematizing economics. In particular, it was from his outstanding calculus text [14] that I learned the felicitous phrase 'undecidable disjunction', which was instrumental in my understanding of the pernicious influence of the Bolzano-Weierstrass theorem in algorithmic mathematics and, *a fortiori*, in algorithmic economics. *But, as always these days, it is to Chico Doria and Stefano Zambelli that I owe most – without the slightest implications for the remaining infelicities in the paper.*

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