**Abstract**

In this paper I attempt to show that mathematical economics is *unreasonably ineffective. Unreasonable,* because the mathematical assumptions are economically unwarranted; *ineffective* because the mathematical formalizations imply non-constructive and uncomputable structures. A reasonable and effective mathematization of economics entails *Diophantine formalisms.* These come with natural undecidabilities and uncomputabilites. In the face of this, an economics for the future will be freer to explore experimental methodologies.

Key Words: General Equilibrium Theory, Computable General Equilibrium, Constructive Mathematics, Computability, Diophantine Equations, Mathematical Economics

JEL Classification: **B41**,**C62**,**C68**,**D50**,**D58**

# The Unreasonable *In*effectiveness of Mathematics in Economics*

K. Vela Velupillai†
Department of Economics
National University of Ireland, Galway
Galway, Ireland
and
Department of Economics
University of Trento
Trento, Italy

August 14, 2003

## 1 Preamble

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

Eugene Wigner's *Richard Courant Lecture in the Mathematical Sciences*, delivered at New York University on May 11, 1959, was titled, picturesquely and, perhaps, with intentional impishness *The Unreasonable Effectiveness of Mathematics in the Natural Sciences,* [35]. Twenty years later, another distinguished scientist, Richard W. Hamming, gave an invited lecture to the Northern California Section of the *Mathematical Association of America* with the slightly truncated title *The Unreasonable Effectiveness of Mathematics, [10]*. A decade or so later Stefan Burr tried a different variant of Wigner's title by organising a short course on *The Unreasonable Effectiveness of Number Theory*, [4]. Another decade elapsed before Arthur Lesk, a distinguished molecular biologist at Cambridge, gave a lecture at the Isaac Newton Institute for Mathematical Sciences

*My title would appear to be a paraphrasing of the title of Eugene Wigner's celebrated *Richard Courant Lecture in Mathematical Sciences*, delivered at New York University on May 11, 1959: *The Unreasonable Effectiveness of Mathematics in the Natural Sciences* [35]. In fact, however, it is, in spirit and intent, a paraphrasing of the title Arthur Lesk wanted to give bur was, unfortunately, advised against giving, for his talk at the Isaac Newton Mathematical Centre in Cambridge: *The Unreasonable **In**effectiveness of Mathematics in Molecular Biology* (cf. [17], [18]).

†e-mail: vela.velupillai@nuigalway.ie or kumaraswamy.velupillai@economia.unitn.it

at Cambridge University where he invoked yet another variant of the Wigner theme: *The Unreasonable Effectiveness of Mathematics in Molecular Biology, [17].* First a physicist; then a computer scientist; then number theorists and, finally, also molecular biologists; so why not an economist, too? But note that my title is not about the *unreasonable effectiveness of mathematics in economics;* I am, instead, referring to the *ineffectiveness*. I was not a little influenced by the story behind Arthur Lesk's eventual choice of title (cf. [18]).

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

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

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

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

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

> '...biology lacks the magnificent compression of the physical sciences, where a small number of basic principles allow quantitative *prediction* of many observations to high precision. A biologist confronted with a large body of inexplicable observations does not have faith that discovering the correct mathematical structure will make sense of everything by exposing the hidden underlying regularities.

.... A famous physicist once dismissed my work, saying: "You're not doing science, you're just doing archaeology!" .... [I]t emphasizes a genuine and severe obstacle to applications of mathematics in biology.' (ibid).

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

> '[T]o dress scientific brilliancies and scientific absurdities alike in the impressive uniform of formulae and theorems. Unfortunately however, an absurdity in uniform is far more persuasive than an absurdity unclad.'
> ([28], p.22)

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

Wigner's essay was admirably concise (it was only 16 pages long) and dealt with a host of vast and deep issues within the confines of those brief number of pages. It was divided into five subsections, in addition to a brief introduction[1]. I shall, to some extent, mimic that structure. Hence, the next section in

[1] The five main subsections were titled:

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

this essay will try to summarise the salient points underlying alternative mathematical traditions. Wigner's brilliant lecture was delivered at a time when real analysis reigned supreme and formalism of one variety or another ruled, implicitly or explicitly[2]. There was, if not universal agreement, blissful ignorance or alternative traditions that may have provided different perspectives on physical theories, at least in the practice of the more formalized sciences. Hence, Wigner could happily confine his discussions on '*What is Mathematics?*'[3] to just a page and a half! Today such conciseness is almost impossible, even from the point of view of the knowledge of the mathematically minded economist. *Classical real analysis* is only one of at least four mathematical traditions within which economic questions can be formalized and discussed mathematically. *Nonstandard*, *constructive* and *computable* analyses have been playing their own roles in the formalization and mathematization of economic entities - but almost always within what I call the *closure* of neoclassical theory.

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

If I was to follow Lesk's strategy, then I have one of three possibilities. I can either work within the framework of *General Equilibrium Theory* (GET) as the core of neoclassical economics and choose its computational 'subset', i.e., *Computable General Equilibrium* theory (CGE) and discuss the unreasonable effectiveness, or not, of mathematics inside these, narrow but well-defined citadels of application of mathematics in economics. The second possibility is to choose the computable subset of either GET or some other part of economic theory, not necessarily neoclassical in spirit, and highlight the effectivity of mathematical theorising in these subsets. The third alternative is to confine my attention to that amorphous practice, increasingly called Computational Economics, and

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

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

discuss the effectivity of mathematical theorising in this field. I rule out the latter two alternatives in view of a lack of clearly defined disciplining criteria that would make it possible to provide a decent discussion within the confines of a single, page-constrained, essay. Therefore, I choose, in §3, to define the 'economic theory' to which mathematics has been applied *ineffectively*, and *unreasonably* so, as GET and confine myself to brief remarks on other, related areas of economics aspiring to the status of a mathematical discipline.

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

## 2 'For Poetry Makes Nothing Happen....'[4] - Mathematical Traditions.

> "Among the abstract arts music stands out by its precise and complex articulation, subject to a grammar of its own. In profundity and scope it may compare with pure mathematics. Moreover, both of these testify to the same paradox: namely that *man can hold important discourse about nothing.*"
>
> Michael Polanyi: **Personal Knowledge** ([23]), p.193; italics added.

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

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

[5] Dummett's question, and enlightened answer, is entirely consistent with Hardy's analogous question and equally felicitous answer - except that the latter aimed at characterizing the *mathematician*: '**A mathematician**, like a painter or a poet, **is a maker of patterns**. If his patterns are more permanent than theirs, it is because they are made with *ideas.* A painter makes patterns with shapes and colours, a poet with words..... .
The mathematician's patterns, like the painter's or the poet's, must be *beautiful*; the ideas, like the colours or the words, must fit together in a harmonious way. ...
It may be very hard to *define* mathematical beauty ... but that does not prevent us from recognising one when we [see] it. ([12], pp. 84-5; bold emphasis added).
Hardy's mathematician, who is a 'maker of beautiful patterns', is exactly Dummett's 'constructor of complex deductive arguments'. Just as Dummett's 'complex deductive arguments' are arrived at by 'routes far from immediately obvious', the 'beauty' of the patterns devised by Hardy's mathematicians are 'hard to define'.

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

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

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

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

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

[6] As David Ruells perceptively observed in his 'Gibbs Lecture':' We like to think of the discovery of mathematical structure as walking upon a path laid out be the Gods. But .... may be there is no path..' ([24], p.266).

the class of mathematicians; there are varieties of Platonists and, equally, varieties of Intuitionists, and others besides: *Hilbertian Formalists, Bournakists, Bishop-style Constructivists, Logicists,*and so on. A flavour of the main differences, based on the Dummett-Hardy characterisation of mathematics and the mathematician, can be discerned from the following artificial dialogue between a mythical Intuitionist (**I**) and an undifferentiated Formalist (**ME**)[7]:

**Example 1**

ME: I have just proved $\exists x A$.

I: Congratulations. *What* is it? *How* did you prove it?

ME: It is an economic equilibrium. I assumed $\forall x \neg A$ and derived a contradiction.

I: Oh! You mean you 'proved' $\neg\forall x \neg A$?

ME: That's what I thought I said.

I: I don't think so.

**Example 2**

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

I: Excellent. Which did you prove?

ME: What?

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

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

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

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

I: No - not at all.

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

The second example illustrates a widely used *non-constructive* principle, most conspicuously utilised in the 'proof' of the Bolzano-Weierstrass Theorem

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

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

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

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

[9] I add the name Dirichlet to the standard naming which justly credits Kuratowski with this definition, for historical reasons. However, it was Dirichlet who initiated this particular tradition, culminating in Kuratowski's 'function as a graph' definition. Dirichlet's definition, in terms of open, continuous, intervals, remains the touchstone, as one can see from the way the Bishop-style constructivists and the Brouwerian Intuitionists have, eventually, defined functions. (cf. for example, [15], p.274: 'It thus appears that an adequate definition of a

**Definition 3** *A function* $f : A \longrightarrow B$ *is any subset* $f \sqsubseteq (A \times B)$ *which satisfies:* $(\forall x \in A)\,(\exists y \in B)$ *s.t* $(x, y) \in f \,\&\, (x, y') \in f \Longrightarrow y = y'$.

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

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

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

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

On the other hand, almost all other traditions, as mentioned above, in their definitions of the notion of a function, retain fidelity with the ordinary meaning and mathematical tradition. Thus, in *Bishop-style constructive mathematics* the distinguishing starting point is that all existence proofs should be constructive in the precise sense that every proof can be implemented, in principle, as an algorithm in a computer[11] to demonstrate, by explicit construction, the object in question. This means, firstly, that the law of the excluded middle[12] is not invoked in infinitary cases; secondly, as a by-product os such a discipline on existence as construction, all functions are required to be uniformly continuous in

function for a continuous interval $(a, b)$ must take the form given to it by Dirichlet'. Hobson does not elaborate upon the meaning of 'adequate', but it certainly had nothing to do with 'performing a task'. ). Of course, the motivation and criteria in the latter two approaches were quite different from those of Dirichlet and Kuratowski.

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

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

[12] *Tertium non datur.*

each closed interval. In other words, if mathematics is about proving theorems, and if proofs are to be constructive - i.e., performable tasks, at least in principle, by a set of explicit instructions - then each function must be characterized in a certain precise way. In other words, Bishop-style constructive mathematics retains fidelity with the ordinary meaning of the concept of function by endowing it with certain *mathematical properties* - i.e., uniformly continuous in each closed interval - such that when they are used in the pattern formation activities of the mathematician they will facilitate the 'performance of tasks'.

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

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

It is, of course, possible to continue a finer classification of varieties of constructive mathematics and, also, varieties of Formalists, Platonists, and Logicists and so on[13]. However, this will achieve no particular purpose beyond that

[13] Although it may appear paradoxical, I am of the opinion that *non-standard analysis* should be placed squarely in the constructive tradition - at least from the point of view of practice. Ever since Leibniz chose a notation for the differential and integral calculus that was conducive to *computation*, a notation that has survived even in the quintessentially *non-computational* tradition of classical real analysis, the practice of non-standard analysis has remained firmly rooted in applicability from a computational point of view. Indeed, the first

which has been achieved with the above few considerations and characterisations for the following reasons. Given the Hardy-Dummett characterisation of mathematics and the activity of the mathematician in terms of 'the systematic construction of complex deductive arguments', it was inevitable that there would be some dissonance in the meaning and interpretation to be attached to 'construction' and the acceptability or not of valid deductive rules for the 'construction'. Depending on the kind of deductive rules and constructions accepted as valid, there are different ways to characterise mathematics and mathematicians. I have highlighted a few of the possible ways to do this - but many other ways could have been attempted with equal ease, which would have resulted in a many-splendoured world of possible mathematics and mathematicians. The main point to note is that it not a monolithic world, characterised by one concept of 'proof' and a single way of 'constructing patterns' from an inflexibly determined set of deductive rules.

# 3 '..A Glittering Deception ..'[14] - *Unreasonably Ineffective Mathematics* in Economics

> "And he wondered what the artist had intended to represent (*Watt knew nothing about painting*), ... a circle and a centre not its centre in search of a centre and its circle respectively, in boundless space, in endless time (*Watt knew nothing about physics*) ... ."
> Samuel Beckett: **Watt** ([2]), p.127; italics added.

In their seminal textbook on mathematical economics, Arrow and Hahn ([1]) state that their 'methods of proof are in a number of instances quite different' from those in Debreu's classic, codifying, text on the *Theory of Value ([7])*. Debreu, in turn, claimed that he was treating the theory of value, in his book, 'with the standards of rigor of the contemporary formalist school of mathematics' and that, this 'effort toward rigor substitutes correct reasonings and results

*modern* rejuvenation of the non-standard tradition in the late 50s and early 60s, at the hands of Schmieden and Laugwitz (cf. [26]), had constructive underpinnings. I add the caveat 'modern' because Veronese's sterling efforts (cf.[34]) at the turn of the 19th century did not succeed in revitalising the subject due to its unfair dismissal by Peano and Russell, from different points of view. The former dismissed it, explicitly, for lacking in 'rigour'; the latter, implicitly, by claiming that the triple problems of the *infinitesimal, infinity* and the *continuum* had been 'solved'.

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

for incorrect ones' (ibid, p.viii). But we are not told, by Arrow and Hahn or by Debreu, either what these 'different methods of proof' mean in the form of new insights into economics or what concept of 'rigor' underpins the substitution of 'correct reasonings and results for incorrect ones'.

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

As a sequel to the codification achieved by Debreu, Scarf began a sustained research program to 'constructivise' one aspect of the mathematics of general equilibrium theory: the problem of existence. Early on, he had realised that proving existence by non-constructive means was unsatisfactory from the point of view of economics as an applied subject, even apart from possible aesthetic motivations and intellectual challenges to constructivise a non-numerical concept. This is the research program under the rubric of *Computable General Equilibrium theory* (CGE), with far reaching policy implications. Surprisingly, no one has tried to constructivise or effectivise the formalizations of the two fundamental theorems of welfare economics.

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

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

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

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

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

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

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

> "We return to the subject of equilibrium theory. The existence theory of the static approach is deeply rooted to the use of the mathematics of fixed point theory. Thus one step in the liberation from the static point of view would be to **use a mathematics of a different kind**. Furthermore, proofs of fixed point theorems traditionally use difficult ideas of algebraic topology, and this has obscured the economic phenomena underlying the existence of equilibria. Also the economic equilibrium problem presents itself most directly and with the most tradition not as a fixed point problem, but as an *equation*, supply equals demand. **Mathematical economists have translated the problem of solving this equation into a fixed point problem**.
>
> I think it is fair to say that for the main existence problems in the theory of economic equilibrium, **one can now bypass the fixed point approach and attack the equations directly to give existence of solutions, with a simpler kind of mathematics** and even mathematics with dynamic and algorithmic overtones."
>
> [29], p.290; bold emphasis added.

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

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

> "They took up several wrong people, and they ran their heads very hard against wrong ideas and persisted in trying to fit the circumstances to the ideas, instead of trying to extract ideas from circumstances."
>
> Charles Dickens: **Great Expectations**, italics added.

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

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

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

1. The *axiom of completeness* ([7], §1.5.*d*, p.10)

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

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

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

**Theorem 4** *(Specker's Theorem in Computable Analysis)*
*A sequence exists with an upper bound but without a least upper bound.*

**Proposition 5** *The Heine-Borel Theorem (on Compactness) is invalid in Computable Analysis*

**Claim 6** *There are 'clear intuitive notions of continuity which cannot be topologically defined' ([**?**], p.73)*

**Proposition 7** *The Bolzano-Weierstrass Theorem is invalid in Constructive Mathematics*

**Claim 8** *The Hahn-Banach Theorem is invalid in Constructive and Computable analysis in its classical form*

**Claim 9** *The fixed point theorems in their classical versions are not valid in (Intuitionistically) Constructive Mathematics.*

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

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

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

**Claim 10** *The Uzawa Equivalence Theorem is neither constructively nor computably valid.*

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

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

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

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

".. [A]s economic analysts we are directed by, if not *prisoners of*, the mathematical tools that we possess."

[25], p.xix; italics added.

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

## 4 '..The Path We Will Never Walk Again'[17]

"Mathematics is not a finished object based on some axioms. It evolves genetically. This has not yet quite come to conscious realization. ...

[T]here might someday be entirely new points of view, even about sets or classes of sets. Sets may someday be considered as 'imaginary.' I think that will come to pass, though at present it is not admissible.

*Mathematics will change. Instead of precise theorems*, of which there are now millions, *we will have, fifty years from now, general theories and vague guidelines*, and the individual proofs will be worked out by graduate students or by computers.

*Mathematicians fool themselves when they think that the purpose of mathematics is to prove theorems, without regard to the broader impact of mathematical results.* Isn't it strange.

*In the next fifty years there will be, if not axioms, at least agreements among mathematicians about assumptions of new freedoms of constructions, of thoughts.* Given an undecidable proposition, there will be a preference as to whether one should assume it to be true or false. Iterated this becomes: some statements may be undecidably undecidable. This has great philosophical interest

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

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

"In all its main forms, modern economic dynamics is an accounting theory. It borrows its leading concepts from the work which had previously been done by accountants (with singularly little help from economists); and it in accordance with this that social accounting should be its main practical instrument of application."

[13]

[17] My translation of the last line from a stanza of one of Antonio Machado's great *Cantares: Caminante no hay camino, se hace camino al andar. Al andar se hace camino, y al volver la vista atras se ve la senda que nunca se ha de volver a pisar. ...'*

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

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

> "Thus without further examination it is not completely evident that the classical real number system will prove to be appropriate to the needs of probability. Perhaps the real number system is: (1) not rich enough - see non-standard analysis; (2) just what we want - see standard mathematics; or (3) more than is needed - see constructive mathematics, and computable numbers. ...
>
> What are all these uncountably many non-computable numbers that the conventional real number system includes?....
>
> The intuitionists, of whom you seldom hear about in the process of getting a classical mathematical education, have long been articulate about the troubles that arise in the standard mathematics ....
>
> What are we to think of this situation? What is the role in *probability theory* for these numbers which can never occur in practice?" [11]

Thus, the only kind of equations that can play any role in the analytical activities of the Political Arithmetician and the Accountant are *Diophantine equations*. How can the problem of solvability of such equations be studied and what methods are available to systematise and routinise their use? The paradoxical answer to both of these questions is that the problem of solvability is intractable and their systematic and routinised study is almost impossible.

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

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

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

They share, with that other 'Cinderella of pure mathematics', nonlinear differential and difference equations, a Linnean status, as poignantly and accurately described by George Temple[21]:

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

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

.

**Definition 11** *A relation of the form*

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

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

**Definition 12** *D in Definition 9 defines a set* $\mathcal{F}$ *of the parameters for which there are values of the unknowns such that:*

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

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

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

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

**Problem 13** *A set, say $\langle a_1, a_2, ....., a_n \rangle \in F$, is given. Determine if this set is Diophantine. If it is, find a Diophantine representation for it.*

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

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

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

**Proposition 14** *Given any parametric Diophantine equation, $D$, it is possible to construct a Turing Machine, $M$, such that $M$ will eventually* ***Halt****, beginning with a representation of the parametric n-tuple, $\langle a_1, a_2, ....., a_n \rangle$,* ***iff*** *$D$ in Definition 9 is solvable for the unknowns, $x_1, x_2, ..., x_m$.*

But, then, given the famous result on the *Unsolvability of the Halting problem for Turing Machines*, we are forced to come to terms with the unsolvability of Diophantine equations[23]. Hence, the best we can do, as Political Arithmeticians and Accountants, and even as behavioural agents, however rational, so long as the constraints are Diophantine, is to act according to the gentle and humble precepts enunciated by George Temple: 'collect specimens, to describe them with loving care, and to cultivate them for study under laboratory conditions'. Clearly, anyone familiar with the work of Charles Sanders Peirce will also realise that this kind of natural historic study fits comfortably with that great man's advocacy of the *retroduction*[24] in such disciplines. The tiresome dichotomy between induction and deduction, refreshingly banished by Peirce more than a century ago, may well get cremated in economics, once and forever, if we combine the methodology of the natural historian with the epistemology that is implied in retroduction.

The headlong rush with which economists have equipped themselves with half-baked knowledge of mathematical traditions has led to an un-natural mathematical economics and a non-numerical economic theory. Whether this trend

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

[24] Even knowledgeable scholars persist in referring to *retroduction* as *abduction*, in spite of Peirce explicitly stating: '....$\alpha\pi\alpha\gamma\omega\gamma\eta$ should be translated not by the word *abduction*, as the custom of the translators is, but rather by reduction or *retroduction*'. (peirce98, p.141; italics in the original)

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

# References


[1] Arrow, Kenneth, J & Frank H. Hahn (1971), **General Competitive Analysis**, Holden-Day, Inc., San Francisco.

[2] Beckett, Samuel (1963), **Watt**, John Calder (Publishers) Ltd., London.

[3] Brouwer, Luitzen Egbertus Jan (1952), *An Intuitionist Correction of the Fixed-Point Theorem on the Sphere*, **Proceedings of the Royal Society, London**, Vol. 213, Series A, June, pp. 1-2.

[4] Burr, Stefan A, Editor, (1992), **The Unreasonable Effectiveness of Number Theory**

[5] Cooper, Necia Grant, Editor (1989), **From Cardinals to Chaos - Reflections on the Life and Legacy of Stanislaw Ulam**, Cambridge University Press, Cambridge.

[6] Courant, Richard and Herbert Robbins (1958), **What is Mathematics?**, Oxford University Press, New York.

[7] Debreu, Gerard (1960), **Theory of Value - An Axiomatic Analysis of Economic Equilibrium**, John Wiley & Sons, Inc., London.

[8] Dummett, Michael, **Elements of Intuitionism**, Clarendon Press, Oxford.

[9] Dummett, Michael (1994), *What is Mathematics About?*, in: **Mathematics and Mind** edited by Alexander George, Chapter 1, pp. 11-26, Oxford University Press, Oxford.

[10] Hamming, Richard W (1980), *The Unreasonable Effectiveness of Mathematics*, **American Mathematical Monthly**, Vol. 87, Issue 2, February, pp. 81-90.

[11] Hamming, Richard W (1991), **The Art of Probability - For Scientists and Engineers**, Addison-Wesley Publishing Company, Inc. Redwood City, California.

[12] Hardy, G. H (1967), **A Mathematician's Apology**, Cambridge University Press, Cambridge.

[13] Hicks, John R (1956, [1982]), *Methods of Dynamic Analysis*, in: **Money, Interest & Wages - Collected Essays in Economic Theory, Volume II**, Chapter 18, pp. 217-35.

[14] Hicks, John R (1983), *A Discipline Not a Science,* in: **Classics and Moderns - Collected Essays on Economic Theory, Volume III**, Chapter 32, pp. 365-75, Basil Blackwell, Oxford.

[15] Hobson, Ernest William (1927), **The Theory of Functions of a Real Variable & the Theory of Fourier Series**, Vol. 1, Third Edition, Cambridge University Press, Cambridge.

[16] Keynes, John Maynard (1930, [1963]), *Economic Possibilities for Our Grandchildren,* in: **Essays in Persuasion** by John Maynard Keynes, Part V, Chapter 2, pp. 358-73, W.W. Norton & Company, Inc., New York.

[17] Lesk, Arthur (2000), *The Unreasonable Effectiveness of Mathematics in Molecular Biology*, **The Mathematical Intelligencer**, Vol. 22, No. 2, pp. 28-36.

[18] Lesk, Arthur (2001), *Compared to What?*, **The Mathematical Intelligencer**, Vol. 23, No. 1, p.4.

[19] Matiyasevich, Yuri V (1993), **Hilbert's Tenth Problem**, The MIT Press, Cambridge, Massachusetts.

[20] Moschovakis, Yiannis N (1994), **Notes on Set Theory**, Springer-Verlag, Berlin.

[21] Nelson, Edward (199?), *Understanding Intuitionism*, manuscript downloaded from: http://www.math.princeton.edu/~nelson/papers.html

[22] Peirce, Charles Sanders Peirce (1898, [1992]), **Reasoning and the Logic of Things - The Cambridge Conferences Lectures of 1898** edited by Kenneth Laine Ketner, with an Introduction by Kenneth Laine Ketner and Hilary Putnam, Harvard University Press, Cambridge, Massachusetts.

[23] Polanyi, Michael (1958), **Personal Knowledge: Towards a Post-Critical Philosophy**, The University of Chicago Press, Chicago.

[24] Ruelle, David (1988), *Is Our Mathematics Natural? The Case of Equilibrium Statistical Mechanics,* **Bulletin (New Series) of the American Mathematical Society**, Vol. 19, No. 1, July, pp. 259-68.

[25] Sargent, Thomas J (1987), **Macroeconomic Theory**, Second Edition, Academic Press, Inc., London.

[26] Schmieden, Curt & Detlef Laugwitz (1958), *Eine Erweiterung der Infinitesimalrechnung*, **Mathematisches Zeitschrift**, Vol. 69, pp. 1-39.

[27] Schwartz, Jacob. T (1961), **Lectures on the Mathematical Method in Analytical Economics**, Gordon and Breach, Science Publishers, Inc., New York.

[28] Schwartz, Jacob. T (1986), *The Pernicious Influence of Mathematics on Science*, in: **Discrete Thoughts - Essays on Mathematics, Science, and Philosophy** by Mark Kac, Gian-Carlo Rota and Jacob T. Schwartz, Birkhaüser, Boston.

[29] Smale, Steve (1976), *Dynamics in General Equilibrium Theory*, **American Economic Review**, Vol. 66, No.2, May, pp.288-94.

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

[31] Temple, George (1958), *Linearization and Delinearization*, **Proceedings of the International Congress of Mathematicians**, pp. 233-47, Cambridge University Press, Cambridge.

[32] Uzawa, Hirofumi (1962), *Walras' Existence Theorem and Brouwer's Fixed-Point Theorem*, **The Economic Studies Quarterly**, Vol. 8, No.1, pp.59-62.

[33] Velupillai, Kumaraswamy (2000), **Computable Economics**,Oxford University Press, Oxford.

[34] Veronese, Giuseppe (1891), **Fondamenti di geometria a più dimensioni e a più specie di unità rettilinee espositi in forma elementare**, Tipografia del Seminario, Padova.

[35] Wigner, Eugene (1960), The Unreasonable Effectiveness of Mathematics in the Natural Sciences, Communications in Pure and Applied Mathematics, Vol. 13, pp. 1-14.

[36] Wigner, Eugene (1964), *Events, Laws of Nature and Invariance Principles*, **Science**, Vol. 145, #3636, 4 September, pp. 995-9.