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Title	Varieties of Mathematics in Economics
Author(s)	Velupillai, K. Vela
Publication Date	2007
Publication Information	Velupillai, K. V., (2007) "Varieties of Mathematics in Economics" (Working Paper No. 0126) Department of Economics, National University of Ireland, Galway.
Publisher	National University of Ireland, Galway
Item record	http://hdl.handle.net/10379/328

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Varieties of Mathematics in Economics*

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Dedicated to Chico Doria and Newton Da Costa

in *homage* for their pioneering work on
Undecidability in Dynamical Systems.

November 21, 2007

*This is a revised and expanded text of the *Lectio Magistralis*, given in the Department of Economics at the University of Trento, on 2 October, 2006. My indebtedness to Enrico Zaninotto, friend, mentor and colleague, is immense - not only for inviting me to give these prestigious lectures. Over the past decade, he has supported my idiosyncratic research, as Dean, as Director of CIFREM, as a colleague and, above all, as a critical friend, on all intellectual matters. I am also grateful to my friends and colleagues, Elisabetta De Antoni, Giorgio Fodor, Roberto Tamborini and Stefano Zambelli, for being generous with their intellectual backing and personal friendships for the kind of *non-standard* (sic!) research programs I have been trying to foster via my work. Had any of the above been responsible for the contents of this 'lecture', it would have been much less prone to error and misconceptions. As it is, I am forced to bear all responsibilities for the inevitable infelicities that remain. Nicole Bertotti's logistical help and support was most important in making my preparations for the event smooth and enjoyable.

ABSTRACT

Real analysis, founded on the Zermelo-Fraenkel axioms, buttressed by the axiom of choice, is the dominant variety of mathematics utilized in the formalization of economic theory. The accident of history that led to this dominance is not inevitable, especially in an age when the digital computer seems to be ubiquitous in research, teaching and learning. At least three other varieties of mathematics have come to be used in the formalization of mathematics in more recent years. These other varieties, I claim, are more consistent with the intrinsic nature and ontology of economic concepts. In this paper I discuss aspects of the way real analysis dominates the mathematical formalization of economic theory and the prospects for overcoming this dominance.

1 Preamble

"Indeed virtually any 'interesting' question about dynamical systems is – in general – undecidable."

Ian Stewart, ([46], p.664)

Many years ago, my Cambridge mentor and maestro Richard Goodwin, pointed out that the dynamics of an economic system could be interpreted as the path traced by a computing device, ([19], p.1):

"[I]t is entirely permissible to regard the motion of an economy as a process of computing answers to the problems posed to it."

The idea he was trying to convey was that a path traced by a dynamical system, as we refer to them in the post-Smale era ([44]), could be viewed as being analogous¹ to the sequential outcome of a suitably programmed analogue or digital computing device. Forty years later, reporting the pioneering *undecidability results for dynamical systems* derived by Newton Da Costa and Francisco Doria, ([11], [12]), in *Nature*, Ian Stewart 'closed the circle', so to speak:

"It is clearly permissible to represent the behaviour of an electronic computer – or more simply a genuinely mechanical model of a Turing machine - as a dynamical system."

[46], p.664

A few years later one of the celebrated *decision problems* in dynamical systems theory - *Arnold's Hilbert Symposium Problem*² - was decisively and famously solved by Da Costa and Doria, [13].

¹Ulam, in his stimulating essay in honour of Marc Kac, extolled the virtues of invoking analogies, [47], p.35;

"Banach often remarked 'Good mathematicians see analogies between theorems and proofs; the very best see analogies between analogies.'..... Throughout the development of mathematics and with the growth of new concepts and more-complicated notions, a cohesive tendency and organic structure have been guided by a feeling of analogy between the old and new ideas."

²As reported in [13], (p.152, italics added), the problem posed in ([2]) was:

Is the stability problem for stationary points *algorithmically decidable*? The well-known Lyapunov theorem solves the problem in the absence of eigenvalues with zero real parts. In more complicated cases, where the stability depends on higher order terms in the Taylor series, there exist no algebraic criterion.

Let a vector field be given by polynomials of a fixed degree, with rational coefficients. *Does an algorithm exist*, allowing to decide, whether the stationary point is stable?

A similar problem: *Does there exist an algorithm to decide*, whether a plane polynomial vector field has a limit cycle?"

Of course, the discerning reader and a connoisseur of dynamical systems theory will recognise, immediately, the connection of this '*Arnold Problem*' with that most obdurate of *Hilbert's Problems*, part B of the 16th. But even the connoisseur may not be aware that a *purely economic hypothesis*, modelled as an innovative dynamical system, was the motivation for a partial solution to part B of *Hilbert's 16th Problem* ([15]).

My own intellectual trajectories, in their technical, mathematical incarnations, had been unambiguously schizophrenic till the time I came across, not purely fortuitously, these writings by Da Costa and Doria. After that the trajectory has been more coherent, albeit also unfathomably challenging - in the mathematical knowledge demanded and the economic underpinnings required. I had been dividing my life between the apparently unconnected fields of computable economics and endogenous economic dynamics, without ever seriously trying to forge a link or seek a coherency between them. However, by the late 80's, with a burgeoning economic theoretic literature, harnessing results and frameworks from the post-Smale literature on dynamical systems theory, dominating one strand of endogenous economic dynamics, my perplexities grew and the schizophrenia was beginning to become intolerable. The perplexity was, of course, the problem of *SDIC: sensitive dependence on initial conditions* of interesting nonlinear dynamical systems³: how much of the dynamics was due to the artifice of simulation and investigation using digital computing devices and how much due to the intrinsic nonlinearities of the dynamics. It is against this background, at a particular stage in my schizophrenic intellectual life, that I came across the above classics by Da Costa and Doria and, therefore, the mind was receptive to the challenges they broached and posed.

Soon after some reasonable acquaintance with the above papers by Da Costa and Doria I was able, for the first time, to interpret the textbook mapping of the price simplex into itself dynamically and use their insights to prove undecidabilities and uncomputabilities intrinsic to it. All that is meant by dynamic interpretation was to change the textbook notation (cf. [3], chapter 1, [45], chapter 1), to:

$$\mathbf{p}_{t+1} = \frac{\mathbf{p}_t + M(\mathbf{p}_t)}{(\mathbf{p}_t + M(\mathbf{p}_t)) \mathbf{e}} \quad (1)$$

Where:

$M(\mathbf{p}_t)$: a mapping from the standard price simplex into itself (depending on the usual excess demand vector, say $z(\mathbf{p})$);

\mathbf{p} : a vector of prices;

\mathbf{e} : an appropriately dimensioned *normalizing* column vector;

Interpreting, firstly, the mapping as a dynamical system and, following the hint in Goodwin's suggestion, the next step was to construct a Turing Machine, formally equivalent to it. Finally, assuming, for example that the price sequence was at least *recursively enumerable*, it was possible to use either *Rice's theorem* or the theorem of the *Unsolvability of the Halting Problem for Turing Machines*, to demonstrate various 'negative' - i.e., *undecidability* and *uncomputability* - results. All this entailed a series of mathematical results and a wholly different mathematical framework in which to encapsulate a standard problem. However, this alternative framework, that of classical recursion theory, was genuinely and

³Or, as Hirsch emphasised in his outstanding survey, ([24]. p.23, italics in the original):

"[O]nly *nonlinear* differential equations have interesting dynamics."

intrinsically computational. The domain over which prices and goods were defined was natural to the economics of the problem. Why, then, was the paraphernalia of standard real analysis so ubiquitous in mathematical economics?

In economic dynamics, stability and uniqueness of equilibrium solutions have become almost a *dogma*, ([40]). Economists have, with customary princely unconcern, ignored these fundamental algorithmic undecidabilities. That neither stability, nor uniqueness – and not even equilibria – can be decided algorithmically for ‘interesting’ dynamical systems may well be an implicit reason for economists to concentrate on decidable, uninteresting, predominantly linear systems, particularly in economic dynamics.

Or, perhaps, the reason is much simpler, and consistent with the main theme in this paper: pure ignorance of *the varieties of mathematics* that can be used to formalize and experimentally investigate economic problems. Algorithmic undecidability of properties of dynamical systems requires considerable mastery of recursion theory, mathematical logic, dynamical systems theory and even, in some cases, nonstandard analysis and nonstandard logic. Economists, festooned to a mathematical economics underpinned by real analysis, have been forced to assert controversial propositions regarding stability, uniqueness and equilibria entirely on the basis of an emaciated mathematical formalism.

The aim in this paper is to try to alert the interested and mathematically minded young economists to avail themselves of the opportunity to ask questions ‘unaskable’ in the orthodox framework of mathematical economics. The subject is vast; my attempt cannot be more than a sketch, given that it is based on a severely time-constrained lecture. If the sketch succeeds in whetting the appetite of a few, perhaps the author will attempt a more comprehensive essay at some future date; even better, someone else may feel inspired to carry a baton for a next lap.

In the formal introductory section, §2, I shall outline the aims and scope telegraphically. In §3, there will be some *examples* of the way alternative varieties of mathematics may enrich the possibilities of formalization to raise questions that cannot be posed in the framework of standard mathematical economics. The ultra-brief concluding section, §4, is perhaps to be read as an outline of a manifesto⁴ on *Re-Mathematizing Economic Theory*

2 Introduction

"The conventional approach [to mathematics] involves an idealization, because one cannot actually complete an infinite number of observations. The [nonstandard] approach also involves an idealization, because one cannot actually complete a nonstandard number of observations. In fact, it is in the nature of mathematics to deal with idealizations. The choice of formalism must be based on esthetic considerations, such as directness of expression, simplicity,

⁴A ‘manifesto’ in the sense in which the word is used in the influential work by Blum, et.al., [7].

and power. Actually, different formalisms in no way exclude each other, and it can be illuminating to look at familiar material from a fresh point of view."

Edward Nelson, [32], p.13.

The problem of alternative mathematical formalisms in economics enters at the ground floor, in an almost trivial way. Anyone who uses the real number continuum to model economic dynamics, particularly in policy contexts, faces the problem of 'causality' in the following precise sense. The real number continuum, when used with *standard* real analysis as its foundation, cannot furnish an *immediate predecessor* or an *immediate successor* to any given point in time⁵. However, the continuum of *nonstandard analysis*, encompassing the infinitesimal rigorously, can be viewed as a system composed of discernible infinitesimally discrete sequences. Naturally, the *constructing economist*⁶, basing economic theory on, say Bishop's version of *constructive analysis*, ([5]), almost naturally accommodates causality in a numerically meaningful and computationally significant sense. Economists, whether as general equilibrium theorists of a microeconomic or macroeconomic⁷ hue or as many other varieties of mathematical economists, are rather cavalier about invoking the real number continuum as their domain of definition and real analysis as the mathematics to encapsulate it.

At a much more basic level, the beginning graduate student of economics is introduced to real analysis, say via [33], and confronts, in the preliminary pages the following theorems, (ibid, p. 41 and p. 52):

Theorem 1 *Every nonempty subset S of \mathfrak{R} that is bounded from above has a supremum (a least upper bound).*

Theorem 2 *Every bounded real sequence has a convergent subsequence.*

The former is stated as 'The Completeness Axiom', and the latter as the 'Bolzano-Weierstrass Theorem', in [33]. The former is also stated as the *Least Upper Bound Property*. The point I wish to make is that neither of these theorems are numerically meaningful; nor are they computationally viable. Why, then, should economists be asked to build their formal mathematical foundations on these sands?

Or, take an example from *smooth infinitesimal analysis*[4], an imaginative eclectic synthesis of *nonstandard* and *constructive* analysis. Suppose we call two points a and b on \mathfrak{R} distinguishable or distinct when they are not identical, i.e.,

⁵The perceptive mathematical economist might say that things are no better with the domain of rational numbers. So much the worse for the rational numbers would be my response!

⁶See below, §4, where the difference between constructive and computable analysis is made in a way that gives content to this phrase.

⁷Roughly speaking, such theorists emphasize stability, uniqueness and equilibria; other mathematical economists are prepared to endow their mathematical formalisms with more tolerance for instability, non-uniqueness and disequilibria. There are, of course, notable exceptions on both sides

$\sim (a = b)$, written, as usual, as $a \neq b$; and, indistinguishable in the contrary case, i.e., *if* $\sim (a \neq b)$, then it does not imply $a \equiv b$. In other words, in the world of *smooth infinitesimal analysis*, it is not the case that all infinitesimals coincide with 0 and the *tertium non datur* does not necessarily hold. It shares the former property with nonstandard analysis; the latter with constructive analysis.

As another example of an interesting eclectic synthesis, entirely motivated by the need to encapsulate, ab initio, numerical meaning and computational content, is the so-called *Russian Constructivism*. [8]. Here the synthesis is between a *variant of constructive mathematics* and *recursion theory*. The logic that underpins Russian Constructivism is *intuitionistic logic*; but this school also accepts and works within a version of the *Church-Turing Thesis*.

In each of the last two examples, a philosophy of numerical meaning and computational content is a driving force for the development of the mathematical framework - the superstructure, so to speak. In the case of the first example, taken from a putative textbook on *Real Analysis with Economic Applications*, no such consideration ever arises in its copiously technical almost 800 pages.

It is this perplexity that I am trying to address and resolve, however partially, in this paper. What criteria are we to use or invoke for the kind of mathematics that economists should use?

Now, Nelson's entirely understandable view of the criterion for 'the choice of formalism' must deal with the problem of defining, in an acceptable and fairly uniform way, the notion of 'aesthetic considerations'. On the other hand, an outstanding pure mathematician like Hardy, [22] and an eminent and supremely successful mathematical physicist like Dirac, [16], would - indeed, have - both, from their own respective subject's point of view, endorsed Nelson's vision. Pragmatically, however, the Putnam-Quine *indispensability argument*, [37], [38], for choosing a formalism based on standard real analysis is, perhaps, the orthodox vision⁸. The 'indispensabilists' flounder on the deep ontological issues and doubts raised about their program by Feferman ([17], p.284)⁹:

⁸I dismiss ad hoc justifications given by unreflective economists - or would-be economists - as not worth serious consideration from the point of view of the foundations of mathematics or even from the vantage point of numerically meaningful and computationally significant mathematical formalisms. For example it is stated in ([31], p.1; italics added):

"Computing with *real numbers* is also relevant to applications in economic theory. Economic models typically use *real variables and functions of them*."

But it is not explained or even seriously discussed why 'economic models typically use real variable' The appeal to [6] is an attempt at *ex post* justification for a modelling philosophy that had nothing to do with 'scientific computation' at its inception. No one with the most basic knowledge of the exercise of mathematization of economic theory would claim anything other than pure accident, convenience and ignorance for the formalization of the subject in terms of standard real analysis.

⁹Feferman's thoughtful closing remark and query is also relevant in the context of the mathematical economists' penchant for modelling in terms of real numbers and standard real analysis, *ibid*, p. 298:

"[A]s long as science takes the real number system for granted, its philosophers must eventually engage the basic foundational question of modern mathematics: 'What are the real numbers, really?' "

"If one accepts the indispensability arguments, there still remain two critical questions:

Q1. Just which mathematical entities are indispensable to current scientific theories?

Q2. Just what principles concerning those entities are needed for the required mathematics?"

I believe these are the two crucial questions, even if not framed in the context of a critique of an 'indispensability argument', that a mathematical economist, who relies exclusively on any one type of mathematical formalism for economic modelling, should try to answer - or, at least, keep as disciplining background criteria. My vision in this paper is almost entirely disciplined by these two perceptive questions that Feferman raises against the 'indispensabilists'. In other words, I take it that the serious mathematical economist is at least a closet 'indispensabilist' and, therefore, the themes in this essay are grounded on: (a). casting doubt on the kind of 'mathematical entities that are considered indispensable' in orthodox mathematical economics; and, (b). questioning the kind of 'principles concerning these entities' that are claimed as 'necessary for the required mathematics'. My examples are, therefore, chosen to illustrate that the chosen 'mathematical entities' and the 'principles concerning these entities' are not appropriate, necessary, relevant or indispensable for mathematical economic modelling.

The infelicities and pitfalls of mathematical economics in the real analytic mode - or in terms of standard mathematics - are discussed more extensively in two companion pieces to this paper, [49], [50].

3 Examples

"The purpose was to present a number of examples in which a plausible expectation is not borne out by a more careful analysis. In some cases the outcome of a calculation is contrary to what our physical intuition appears to demand. In other cases an approximation which looks convincing turns out to be unjustified, or one that looks unreasonable turns out to be adequate.

....

The selection of examples is very subjective. Most concern surprises experienced by myself,, others I found fascinating when I heard of them."

Rudolf Peierls, [35], p.vii

I shall consider three examples in this section: the celebrated and fundamental *Peano existence theorem* for ordinary differential equations (*ODE*), the

The economic apologist's retort may well be that 'its philosophers' are irrelevant - or don't exist - for the mathematical modelling enterprise of the economic theorist. This instrumentalist position is, in fact, the dominant one in mathematical economics.

so-called initial value problem (*IVP*) for ODEs; the existence of *ducks*, discovered by a nonstandard analysis of the famous *van der Pol equation*; and the role of the *axiom of determinateness* in games and the possibility of dispensing with the *axiom of choice*. The first is part of the ‘folklore’ of mathematics education for graduate students of economics (cf. [33], 636); the second, i.e., the van der Pol equation, is – or, at least, has been – a staple for connoisseurs of macroeconomic endogenous business cycle theories. The third issue, the role of the axiom of choice in economic discourse, is a mysterious one. Mathematical economists seem not to be aware of, or not care if they are aware, of the possible implications of the use of this theorem in economic analysis. It played a significant role in Peano’s own choice of methods of proof for the theorem of existence for the IVP problem of ODEs.

3.1 The Peano Existence Theorem

"Our next application [of the Schauder fixed point theorem] concerns a famed existence theorem that was first proved in 1890 by Giuseppe Peano (by a different and *much clumsier technique*)."

[33], pp. 635-6; italics added.

The above quote is from the massive textbook on *Real Analysis with Economic Applications* that will, surely, become standard staple for graduate students of economics. We are not told by the author¹⁰ what it was that was ‘clumsier’ in Peano’s proof; nor are we told that *Peano went out of his way to avoid any appeal to the axiom of choice in his proof*. Furthermore, we are also deprived of the information that Peano dropped the assumption of the *Lipschitz condition* in the 1890 paper (thereby losing *uniqueness*, [26], p.66). Indeed, it was during the course of this proof that the ubiquity of this controversial axiom was first recognised and avoided - long before Zermelo christened it the axiom of choice, fourteen years later, in fact. It may be salutary to compare the above unscholarly dismissal of Peano’s proof with the studied and careful caveats given by Flett in his detailed study of the IVP problem for ODEs, ([18], p. 158; italics added):

"Peano’s second paper .. on the existence problem of continuous f deals with the general case of the vector equation $y' = f(t, y)$ Peano’s proof is both *long and arduous*, since what is essentially a proof of the Ascoli-Arzelà theorem is intricately embedded in it. His argument is also rendered *more arduous* than might be expected

¹⁰Incidentally, Peano’s 1890 paper was in French, published in a German Journal. His earlier paper, for the scalar case, was in Italian. There is a translation to English of the latter, but not of the former. Am I to believe that the author of [33] read this French version carefully to understand the nature of the ‘clumsiness’ in Peano’s proof? I gave it a try, in the original, and found it almost impenetrable, without considerable background *hilfenkonstruktion*. Of course, my difficulties are no criteria for the ability of others. But, in French ...! I also suggest that this author read both Rubel, [39], pp. 48-9, Problem 14 & Remark 14; and also [55] & [29], regarding Peano existence theorems.

by being couched in the symbolic language of the logical calculus, though he does include a six-page ‘résumé’ of the proof written in everyday mathematical language."

I ‘preface’ this section with the above observations just to make note of the fact that, once again, ignorance of alternative mathematical traditions and careless understanding of the underpinning axioms, lead to misleading assertions that are, then, propagated generation after generation.

Here is a ‘modern’ statement of the *Peano theorem*¹¹, ([27], pp.364-5):

Theorem 3 *Peano existence theorem:*

Let the function $\mathbf{f} : [t_0, t_0 + a] \times U \rightarrow \mathbb{R}^d$, where $U \subseteq \mathbb{R}^d$, be continuous in the cylinder:

$$S = \{(t, \mathbf{x})\} : t \in [t_0, t_0 + \alpha], \mathbf{x} \in \mathbb{R}^d, \|\mathbf{x} - \mathbf{y}_0\| \leq b\}$$

where $a, b > 0$ and the vector norm $\|\cdot\|$ is given. Then, the ODE:

$$\mathbf{y}' = \mathbf{f}(t, \mathbf{y}), \quad t \in [t_0, t_0 + \alpha], \quad \mathbf{y}(t_0) = \mathbf{y}_0 \in \mathbb{R}^d$$

where:

$$\alpha = \min \left\{ a, \frac{b}{\mu} \right\} \quad \text{and} \quad \mu = \sup_{(t, \mathbf{x}) \in S} \|\mathbf{f}(t, \mathbf{x})\|$$

possesses at least one solution

However, in the mathematics of *Russian Constructivism*, it can be shown that $\exists \mathbf{f}(t, \mathbf{y})$, satisfying the hypotheses of the *Peano existence theorem*, such that there is no solution to the IVP ([1], chapters 3 and 11). Why is this so? Essentially this is because the existence of a solution violates a cardinal theorem of computable calculus: the *Unsovability of the Halting Problem for Turing Machines*. More specifically, there are a series of *nonsolvable* problems by *finite means*, in the computable calculus of *Russian Constructivism*, some of which have to be made solvable – by *non-finite means* – for the *Peano existence theorem* to be satisfied. In the case of the *Peano existence theorem*, the relevant *non-solvable* problems are:

Proposition 4 *It is undecidable (by finite means) whether, $\forall a \in \mathbb{R}, a$ or $\sim a$ is rational.*

Proposition 5 *It is undecidable (by finite means) whether, $\forall a \in \mathbb{R}, a \geq 0$ or $a \leq 0$.*

Thus, implicit in any standard proof of the Peano existence theorem there are appeals to non-finite means to decide disjunctions. These are almost never made explicit, except by supremely careful mathematicians like Peano who go

¹¹I have chosen to state the ‘modern’ version from this admirable textbook by Iserless, ([27]), mainly because it is devoted to the ‘numerical analysis of differential equations’.

into great detail and pains to make sure that one is at least aware of the role of unacceptable non-finitary axioms, example, the axiom of choice. If the mathematics we invoke, in formalizing economics, appeals to non-finite means of verifying disjunctions, then it is useless for any kind of application, particularly in policy contexts.

This does not mean that theorems analogous to the Peano existence theorem cannot be devised in computable, constructive or nonstandard analysis, (cf., [10], [23], [55] and [39] and the references in the latter to Aberth and Pour-El & Richards, as well as [36]). It is just that one will have to be more selective in the hypotheses and less grandiose in the conclusions. Surely, these are virtues, to be made available to students at the earliest possible stage of their advanced education?

3.2 Non-standard *Ducks* in the van der Pol Equation

3.2.1 A Personal Prologue

"In the late 17th century, when Newton and Leibniz were first inventing calculus, part of their theory involved *infinitesimals* – i.e., numbers that are infinitely small but nonzero. Indeed, *the conventional real number system is Dedekind complete and therefore Archimedean, which essentially means that it lacks infinitesimals;*
..... .

The advantage of nonstandard mathematics is that its intuition is sometimes helpful; Leibniz and Newton had infinitesimals in mind when they invented the calculus; surely this is testimony to the usefulness of the intuition of nonstandard mathematics.

[41], pp.394-6; italics added.

Economists routinely reason in terms of infinitesimals, without, of course, realizing it¹². Every time mathematical economists cavalierly invokes ‘price taking’ behaviour due to the insignificance of individual agents in a perfectly competitive market, they are also invoking poor old Archimedes, too. My own realization of his immanent presence in the mathematics I was using came about entirely accidentally, but felicitously.

A completely accidental find, at a Cambridge antiquarian bookshop, of Max Newman’s copy of Hobson’s classic text on real analysis, [25], during what turned out, subsequently, to be a melancholy visit to that city in late 1977, was the beginning of my initiation into non-Archimedean mathematics. It so happened that I was spending that academic year as a Research Fellow at *C.O.R.E.*, in Louvain-La-Neuve and my neighbouring office was occupied by Bob Aumann. I found Hobson’s book eminently readable – all 770 pages of it, in that first edition format I was reading; it later expanded into double that size in later editions. However I was perplexed by the fact, clearly pointed out in the book, that

¹²The ubiquitous presence of Molière’s M.Jourdain, in different guises, among mathematical economists has to me mentioned!

Hobson referred to Veronese as the modern ‘resurrector’ of the older Leibniz(-Newton) notion of infinitesimals and his – Veronese’s – development of a calculus devoid of the Archimedean assumption, ([25], pp.54-6). The perplexity was, of course, that none of the historical allusions to the founding fathers of nonstandard analysis even remotely referred to Veronese as one of them. There were the great originators: Leibniz and Newton; then there was the great resurrection by Skolem; and, finally, the ‘quantum’ jump to Abraham Robinson. Neither Peirce nor Veronese, both of whom explicitly and cogently denied the Archimedean axiom in their development of analysis, were ever referred to, at least in the ‘standard’ texts on nonstandard analysis.

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

Why had Veronese’s modern classic, [54], ‘disappeared’ from orthodox histories of nonstandard analysis, at least at that time? Some rummaging through the historical status of Veronese’s work on non-Archimedean analysis, particularly in Italy, in preparing this lecture gave me a clue as to what had happened. It was Veronese’s misfortune to have published his work on nonstandard analysis just as his slightly younger great Italian mathematical contemporary, Giuseppe Peano, was beginning his successful crusade to consolidate the movement to make standard analysis rigorous. Veronese’s book was severely criticised¹³ for falling foul of the emerging orthodox standards of ‘rigour’ and fell off the backs of the official mathematical community like water off of a duck’s (sic!) back. If only they knew what nonstandard ducks would eventually be shown to be capable of, just in the study of the van der Pol equation alone!!

3.2.2 van der Pol Ducks

"In the course of making enquiries for the Colloquium on Dynamical Systems I found out that van der Pol was often more mathematical than physical in his approach to problems. He would construct a physical system to correspond to a mathematical equation, and pushed the study of the equation known by his name into regions

¹³See, in particular, Peano’s ‘open letter’ to Veronese, [34], in the very first volume of the Journal Peano founded in 1891, *Rivista di Matematica*. The hands of fate have a way of making confluences toll heavily in one direction than another! I may add that my interest, as a Trento economist, has a regional patriotic flavour in favour of Veronese. He was from Chioggia, ‘here’ in the Northeast of Italy; Peano was from, Spinetta, near Cuneo, at the other end of the horizontal divide of Italy, the Northwest!

where it ceased to correspond to the physical system as closely as some other mathematical formulation. In particular, in 1920, he chose a slightly unusual circuit in order to obtain his famous equation:

$$\ddot{x} + \epsilon(x^2 - 1)\dot{x} + x = 0$$

ϵ small, and he was the first (apart from Rayleigh) to obtain an equation representing a system with *a single strongly stable oscillation*."

Mary Cartwright, [9], p. 330.

The van der Pol equation, and its integrated form as the Rayleigh equation, played an important role in the nonlinear (endogenous) theory of the business cycle in the 'Golden Age' of Keynesian dominance¹⁴. Even now, the declining number of endogenous, disequilibrium, macrodynamists seek their foundations in the classic work of those pioneers who modelled the business cycle in terms of variations of the van der Pol equation. A full understanding of the equation remains elusive, particularly in its *forced form*, in which form it has played an important part in the development of dynamical systems theory. The full economic background to its use in business cycle theory, and the mathematical underpinnings, are extensively discussed in a series of three papers and I refer the interested reader to them for further information, ([51], [52] and [53]).

My interest here is to point out the way, using *nonstandard analysis*, unusual phase portraits were discovered for this fascinating equation¹⁵. Zvonkin and Shubin, in their detailed and rigorous analysis of the issue here, summarised admirably the nature of the discovery, [56], p.69, italics added:

"*Ducks* are certain singular solutions of equations with a small parameter, which are studied in the theory of relaxation oscillations. These solutions were *first found for the van der Pol equation*, and their form resembled that of a flying duck. *Duck theory* is, in the authors' opinion, *the most striking application of the techniques of non-standard analysis*.

.....

It was not by chance that ducks were discovered with the help of non-standard analysis and in connection with it. We think that the language of non-standard analysis will make it easy for a wide circle of mathematicians to become acquainted with the theory of ducks and the theory of relaxation oscillations in general."

¹⁴Its first appearances in the business cycle literature were in unfortunately neglected papers by Hamburger ([20], [21], as equation # 7, on p.5, in the former and in footnote 7, p.6 in the latter) in the formal form:

$$\frac{d^2y}{dt^2} - \alpha(1 - y^2)\frac{dy}{dt} + \omega^2y = 0$$

¹⁵I hasten to add that, *ex post*, standard analysis has been able to re-absorb the new discoveries into its fold. The point remains, however, that the original discovery came about by a fertile use of nonstandard analysis.

Relaxation oscillations encompass two-phase dynamics in the sense that there is an interaction between slow and fast variables in the system, rather like one set of markets (financial?) clearing ‘infinitely fast’, and another set (real?) clearing relatively slow. The problem, of course, is that ‘infinitely fast’ is a meaningless concept in standard analysis, but an eminently sensible notion in nonstandard analysis; analogously, the ‘infinitesimal’ is a fully viable concept in nonstandard analysis, but not so in standard analysis.

Consider, now, the following nonlinear equation:

$$\epsilon \frac{d^2x}{dt^2} (x + x^2) \frac{dx}{dt} + x + \alpha = 0 \quad (2)$$

This can be represented as:

$$\frac{dx}{dt} = \epsilon^{-1}(y - f(x)) \quad (3)$$

$$\frac{dy}{dt} = -(x + \alpha) \quad (4)$$

and the ‘characteristic’, $f(x)$, is given by:

$$f(x) = \frac{1}{3}x^3 + \frac{1}{2}x^2 \quad (5)$$

The phase-plane dynamics depicted in the diagrams below are for the following numerical values of α and ϵ (the red curves, in all cases, are the graphs of the ‘cubic characteristic’).

1. The values to get the first figure were so chosen that the phase-plane dynamics would resemble, as closely as possible, that given in the pioneering nonlinear trade cycle model of Goodwin; here, $\alpha = .45$; $\epsilon = 10$;
2. The second figure, almost similar in its geometry to the first one, has: $\alpha = .5$; $\epsilon = 1000$;
3. Figures 3 and 4 (the ‘Duck Headed vdP’ dynamics) are obtained for: $\alpha = .001012345$; $\epsilon = 1000$;
4. Finally, the two curves for the ‘Unheaded Duck vdP’s are obtained for: $\alpha = .00001025$; $\epsilon = 1000$;

For $\alpha = 0$ and if $\frac{1}{2}x^2$ is replaced by x , the system reduces to the van der Pol equation on the Liénard plane¹⁶.

It is clear that y is the ‘slow’ variable; i.e., it is finite for all finite points of the domain of the plane; x , then, is the ‘fast’ variable and takes infinitely large

¹⁶I chose the ‘characteristic’ with the $\frac{1}{2}x^2$ term, instead of the x term only because I had severe difficulties of precision to get the kind of phase-plane dynamics I could have got with a computer capable of more precise computations.

values for some finite values of its domain. If the trajectory of y is defined to be on that curve at which $\dot{x} = 0$, then its graph is given by $f(x)$.

As for the ‘Duck’ terminology, the idea should be obvious from a perusal of the diagrams (and a bit of imagination!).

The proof of existence of ‘Unheaded Ducks’, i.e., counter-intuitive cycles being attracted to unstable manifolds, for the van der Pol system is extremely simple - provided one learns a bit of nonstandard analysis - or, at least, non-standard terminology. Let me simply state it, in as heuristic and intuitive way as possible, to illustrate what I mean; the interested reader can get a clear idea from the exceptionally clear and detailed article by Zvonkin and Shubin. The only thing to keep in mind is that α is an infinitesimal in the sense of non-standard analysis. Then¹⁷ (referring to the last two phase-plane diagrams) and [56], §4.2:

Definition 6 *An admissible form for the characteristic, $f(x)$.*

$f(x)$ has an admissible form on a closed interval, say $[\beta_1, \beta_2]$, if:

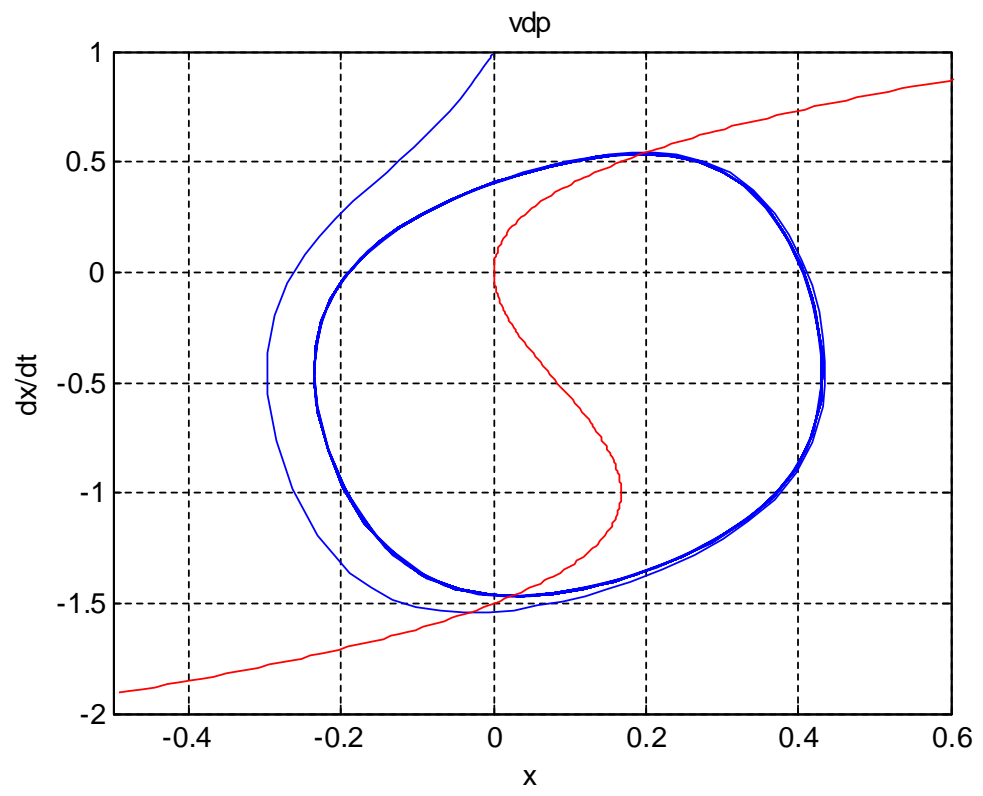
- (1). *$f(x) \in [\beta_1, \beta_2]$ is standard and \mathbb{C}^2 ;*
- (2). *$f(x) \in [\beta_1, \beta_2]$ has exactly two isolated extremum points, say a minimum at x_0 , and a maximum at x_1 , and $\beta_1 < x_1 < x_0 < \beta_2$, so that: $f'(x) > 0$ on $[\beta_1, x_1]$ and $(x_0, \beta_2]$ and $f'(x) < 0$ on (x_1, x_0) ;*
- (3). *$f(\beta_1) < f(x_0)$ and $f(\beta_2) > f(x_1)$;*

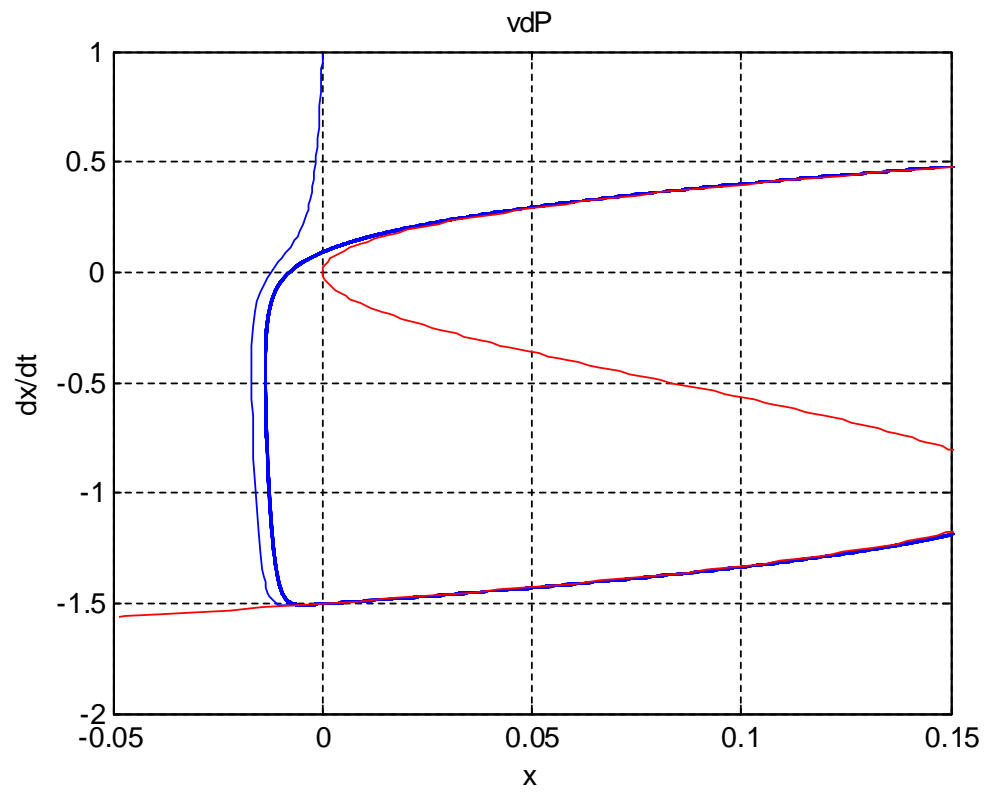
Theorem 7 *Existence of Duck Cycles in the van der Pol system [(2) or (3)–(5)].*

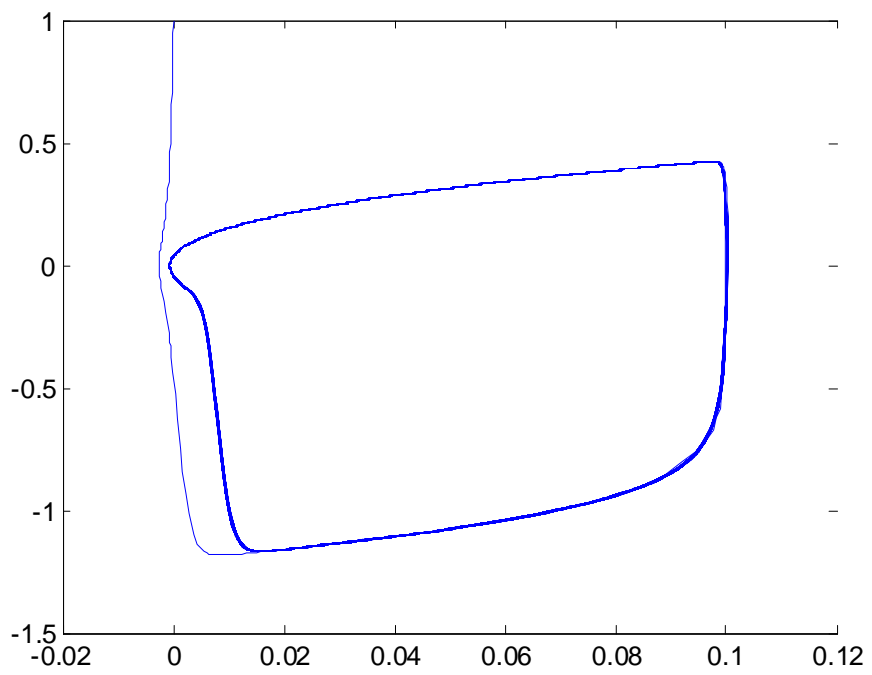
Suppose $f(x)$ has an admissible form on $[\beta_1, \beta_2]$; if $x_\beta \in \mathfrak{R}$ and $x_\beta \in (x_2, x_0)$, then \exists value of the infinitesimal α , for which the van der Pol system has a Duck-Cycle such that x_β is the value on the x -coordinate corresponding to the ‘beak’ of the ‘Duck’.

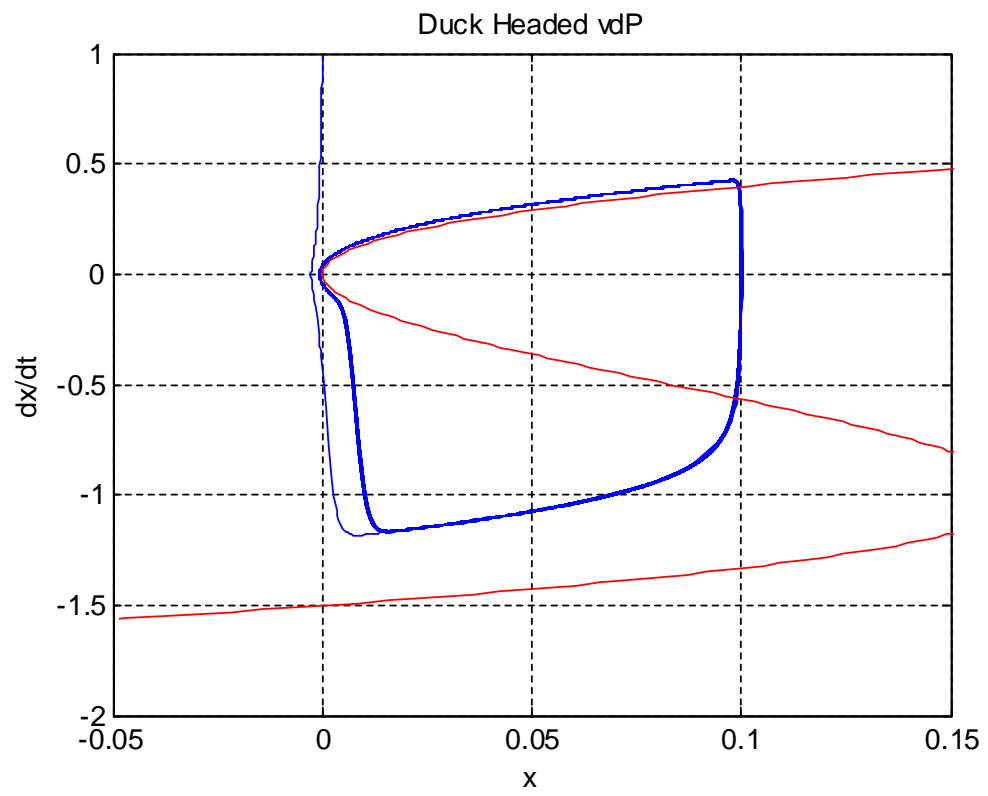
The point of the exercise is that a knowledge of the possibilities for exploring a dynamical system with parameters and variables taking infinitesimal and infinite values is indispensable - not just for reasons of pure mathematical aesthetics; but also for eminent economic reasons, where financial market variables move ‘infinitely’ fast, at least relative to ‘real’ variables; and reactions in market sentiments to ‘infinitesimal’ variations in parameters is a non-negligible factor in turbulent markets. An economist, narrowly trained in standard mathematics will always have to resort to ad hoceries to handle the infinitesimal and the infinity - for example, in models capable of relaxation oscillations. Quite apart from aesthetics and pragmatics, it is also the case that the mathematics of nonstandard analysis is intuitively natural and much simpler, without all the artificial paraphernalia of the ‘ $\epsilon - \delta$ ’calisthenics.

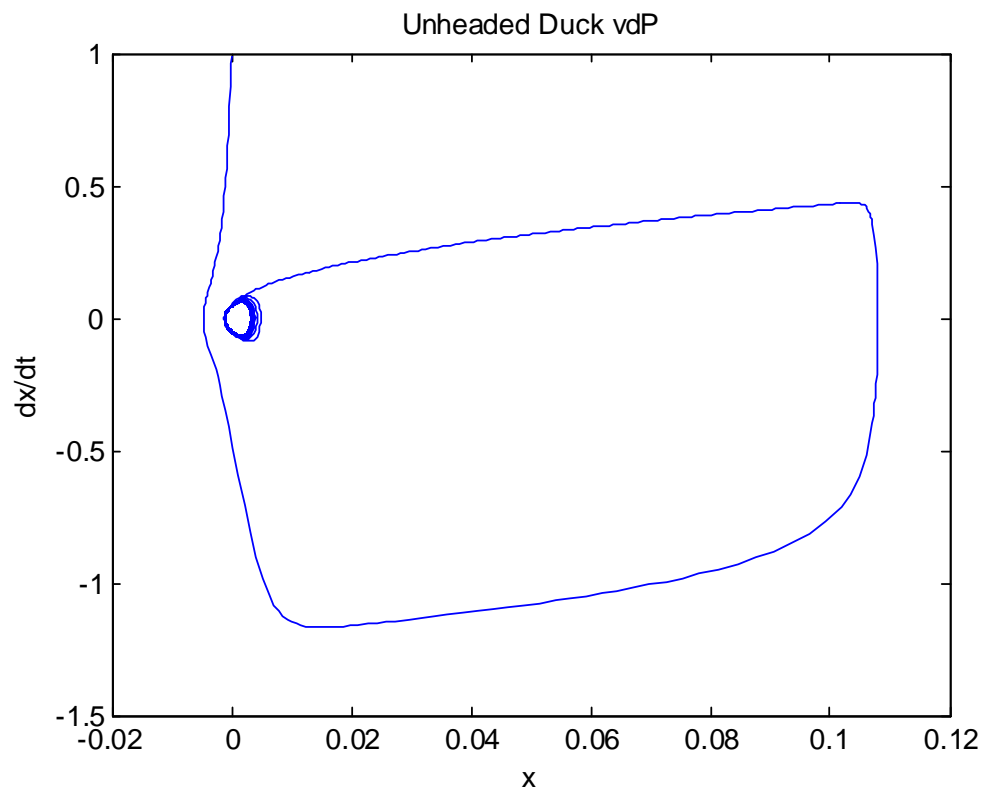
¹⁷This is only a sufficient condition and the ‘admissible curve’ is simply a formalization of the traditional ‘cubic characteristic’ for the van der Pol equation. I conjecture that ‘Duck Cycles’ can be shown to exist even without a ‘cubic characteristic’; say, for example, with a ‘characteristic’ of the form: $\tau(e^{\dot{x}} - 2)$. Such a form would have only one isolated maximum or minimum.

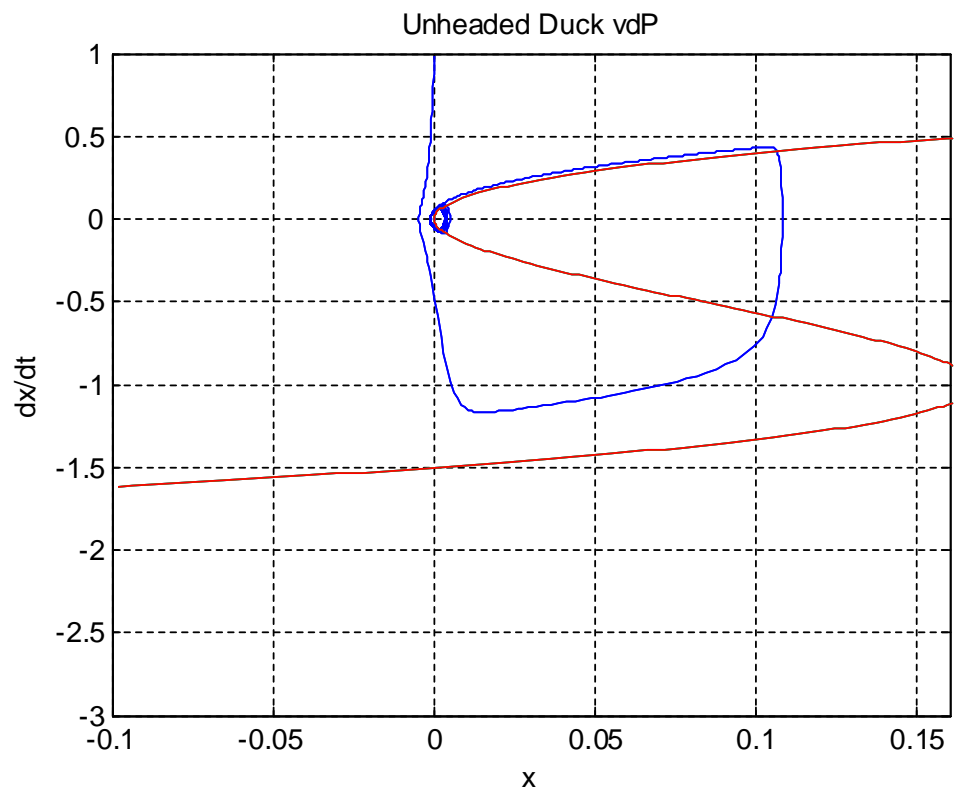












3.3 Axiom of Choice vs. Axiom of Determinacy

"[Steinhaus] was aware of the following theorem: Let G be a two-person game with perfect information, terminating in a finite number of moves in a win by one of the players. Then there must exist a winning strategy for either one or the other adversary.

....
 Steinhaus now proposed that this simple theorem be made into an axiom [the *Axiom of Determinacy*] by removing the restriction that n is finite, i.e., that the game *must terminate in a finite number of moves*.

It is here that one runs afoul of the axiom of choice."

Mark Kac, [28], p.577, italics added.

How does the *Axiom of Determinacy (AD)* ‘run afoul’ of the *Axiom of Choice (AC)*? Consider the following heuristic description of the Banach-Mazur game¹⁸.

Definition 8 Let $S \subset (0, 1)$; Players A and B choose, alternatively, binary digits $x_1^A, y_1^B, x_2^A, y_2^B, \dots$ which defines the real number, η :

$$\eta = \frac{x_1^A}{2} + \frac{y_1^B}{2^2} + \frac{x_2^A}{2^3} + \frac{y_2^B}{2^4} + \dots$$

Player A wins the game if $\eta \in S$;

Player B wins the game if $\eta \notin S$;

Claim 9 Mycielski showed the following: *AC implies that \exists a set $S \subset (0, 1)$ s.t neither A nor B had a winning strategy.*

Hence: $AD \rightarrow \sim AC$

My interest in the controversy over a choice between *AD* and *AC* is entirely motivated by my belief that the kind of games introduced by Banach, Mazur and Steinhaus, subsequently codified for economics by *Gale and Stewart*, are *alternative games* that are playable by *Turing Machines*. Hence, following the great pioneering work by Michael Rabin, it is possible to frame, entirely in Turing’s tradition, questions of ‘*playability*’. In other words, not only is it interesting to attempt to eschew any reliance on *AC* and its insidious consequences (for example, the *Banach-Tarski Paradox*); but also to supplement the determinateness of alternating games, guaranteed by working with *AD*, with questions of *effective playability*. Thus, even though the primary question, in the context of alternating games, is to guarantee determinateness, i.e., assurance that one or the other player has a winning strategy, the almost equally important next question should be: *given determinacy can the determined winner be provided with effective instructions to actually implement the winning strategy?* Using

¹⁸I have described and discussed this kind of game in some detail in [48], Chapter 7.

the methods devised in the *negative solution to Hilbert's 10th Problem*, it can be shown that, in general, that the answer is negative.

But there is a much simpler reason, as well. In orthodox mathematical economics the notions of equilibrium, stability and uniqueness are crucial. No respectable mathematical economist would dream of working within an axiom system that precludes equilibria, for example. That is the issue here, in the case of alternating games, a genre that, in its modern incarnation, descends from Zermelo - a supreme irony! After all, *AC* was a product of Zermelo's fertile imagination! A community that would consider it nothing short of a scandal if its mathematical framework precluded equilibria should be equally petrified by a reliance on an axiom - *AC* - that precludes determinacy in an interesting class of games.

But how can a program of education or research in, and for, the mathematically minded economist, be implemented without starting at the ground floor - i.e., graduate mathematical background?

About a quarter of a century ago, Harvey Friedman and those associated with him initiated the '*reverse mathematics*' program. Stephen Simpson's specific contribution on, '*Which Set Existence Axioms are Needed to Prove the Cauchy/Peano Theorem for Ordinary Differential Equations*', [43], was a significant part of that program. In the context of the topic discussed in the first subsection above and also in unearthing the implicit use of thoughtless axiom system to underpin the mathematics of economics, it may be useful to recall, against the backdrop of the issues raised in this essay, the opening lines of Simpson's thoughtful paper (*ibid*, p.783; italics in the original):

"This paper is part of a program whose ultimate goal is to answer the following Main Question: *what set existence axioms are needed to prove the theorems of ordinary mathematics?* We believe that such a program has important implications for the philosophy of mathematics, especially with respect to the foundations of mathematics and the existence of mathematical objects."

This is, of course, related to the doubts raised by Feferman to the Putnam-Quine thesis; it is, surely, worthy of some interest to the mathematical economist?

4 Re-Mathematizing Economic Theory

"[Nonstandard analysis] does not present us with a single number system which extends the real numbers, but with many related systems. Indeed, there seems to be no natural way to give preference to just one among them. This contrasts with the classical approach to the real numbers, which are supposed to constitute a unique or, more precisely, categorical totality. However,, I belong to those who consider that it is in the realm of possibility that at some stage

even the established number systems will, perhaps under the influence of developments in set theory, bifurcate so that, for example, future generations will be faced with several coequal systems of real numbers in place of just one."

Abraham Robinson, *Brouwer Memorial Lecture*, [14], p. 462.

Arend Heyting made the important distinction between ‘*theories of the constructible*’ and ‘*constructive theories*’. Recursion theorists, in general, and computable analysts, in particular, work in the domain of the constructible and theorise about them. Constructive mathematicians, both the intuitionist descendants of Brouwer and the modern followers of Bishop, develop constructive theories. The nonstandard analyst, too, is interested in construction. The classical mathematician, especially in the incarnation as a mathematical economist, blissfully unconcerned about such things. These traditions, philosophies and methodologies which characterise each of these varieties of mathematics leaves its imprint on scavenging subjects like economics, without the physicist’s tradition of developing an autonomous mathematical philosophy.

I am concerned, thus, with the numerical and computational emasculation of economics, at a most basic level; but I am also concerned with the philosophical, methodological and epistemological underpinnings of mathematical economics, the core determining force in the trajectory that is taken by economic theory - and, hence, a force beyond the confines of academic walls. For close to two centuries mathematical theorising in economics has proceeded fortuitously and haphazardly.

At the frontiers of research and policy, mathematical finance theory blindly formalizes its rules and laws in terms of, say, the *Ito Stochastic Calculus* - even while sterling attempts are made to do otherwise¹⁹, with a mathematics that is more consistent with the institutional basis of financial markets and respecting the discrete nature of high frequency data.

Mathematical economics is replete – indeed, inundated – with existence theorems and their nonconstructive proofs. It is rarely pointed out that, as a result of the methods devised to solve one of Hilbert’s famous ‘Mathematical Problems’, the 10th, it is equally easy to show that hardly any of these existence theorems have any hope of being algorithmised. Indeed, to take up the subject discussed above, the Peano Existence Theorem for the IVP of ODE’s, one of the frontier research results in applied recursion theory is the following, (see, [30], chapter 9) :

Theorem 10 *There is no effective method for determining, for an arbitrary system of differential equations of the form,*

$$\begin{aligned}
 P_1(x, \Xi_1(x), \dots, \Xi_k(x), \Xi'_1(x)) &= 0 \\
 &\dots \\
 &\dots \\
 P_k(x, \Xi_1(x), \dots, \Xi_k(x), \Xi'_k(x)) &= 0
 \end{aligned}
 \tag{6}$$

¹⁹I have in mind the innovative and highly stimulating book by Shafer and Vovk, [42].

where P_1, \dots, P_k are polynomials with integer coefficients, whether the system has a solution on the interval $[0, 1]$.

This is just one representative result, in an important applied domain, derived using a uniform method of proof. It is entirely analogous to the way the mathematical economist uses a few 'crown jewels' – one or another fix point theorem, the Hahn-Banach theorem, the Value Function in a dynamic programming framework, and so on – to prove, ad nauseum, equilibrium existence results and efficiency postulates. Neither an investigation of the economically meaningless axiomatic underpinnings of these theorems is ever undertaken; nor is the poor, hapless, graduate student ever warned of the paradoxical – even pernicious – implications of some of these axioms. Just as Uzawa's equivalence theorem, between the Brouwer fix point theorem and the Walrasian equilibrium existence theorem, is celebrated in economic theory and computable general equilibrium, the time will no doubt come when some enterprising graduate student will find a way to prove the equivalence between the Hahn-Banach Theorem and AC ²⁰

The pseudo-scientific status to which economists aspire, by cloaking imprecise concepts with ill-fitting mathematical clothes, has led to a neglect of the nobility of the Linnean tradition of research.

²⁰The one-way implication from AC to the Hahn-Banach theorem is already very well known. If the reverse implication is also shown to be valid, Friedman's infamous aphorism about free lunches will have to be abandoned - in view of the Banach-Tarski paradox. One apple will be more than sufficient to feed a whole population.

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