Algorithmic Social Sciences Research Unit

TOWARDS AN *INTUITIONISTIC CONSTRUCTIVE MATHEMATICAL ECONOMICS*♣

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"Freedom in thinking is important;

But correct thinking is more important."♠

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§1. An Intuitionistic Prologue

> "I am alluding to Brouwer's theory of *the relation between mathematics on the one hand and language and logic on the other*. ….
>
> Brouwer solved the problem by making a sharp distinction between *mathematics as such and its linguistic expression and communication*. Mathematics itself he saw as an extra-linguistic activity, essentially an activity of mental construction on the basis of our pure intuition of time. By way of this construction we create in our intuition, in our mind, the objects of mathematics which **afterwards** – after their creation – we can try to describe, and to convey to others. Thus **the linguistic description**, **and the** discursive argument with its **logic**, **come after** the essentially mathematical activity: they always come after an object of mathematics – such as **a proof** – has been **constructed**."
> Popper (1968; 1972), p. 132; bold emphasis added.

It is in this precisely delineated way, brilliantly clarified by Popper (but also by legions of intuitionistic constructivists[1]) that mathematics was languageless.

Yet, economic theorists, with allegiance to the mathematization[2] of the subject, have accepted Paul Samuelson's invocation of an *alleged* statement by Willard Gibbs[3] as the epitaph for his *Foundations* (Samuelson, 1947):

> Mathematics *is* a language.

But, alas, Samuelson – even the gods, sometimes, nod – went further, five years later (Samuelson, 1952, p. 36; bold italics added):

> "I have only one objection to [the above statement]. I wish [Gibbs had made it 25 per cent shorter – so as to read as follows: 'Mathematics *is* language.' Now I mean this entirely literally. In principle, mathematics cannot be worse than prose in economic theory; in principle, it certainly cannot be better than prose. For in deepest logic – and leaving out all tactical and pedagogical questions – *the two media are strictly identical*."

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[1] I have been decisively influenced in my convictions in favour of intuitionistic constructive mathematics as the basis for economic theorizing – not formalization or axiomatisation, neither of which were advocated by Brouwer – by the works of the Post-Bouwer/Heyting Dutch quartet of Anne Troelstra, Dirk van Dalen, Mark van Atten and W.P. van Stigt; and Michael Dummett, Carl Posy, Richard Tieszen and Charles McCarty, more-or-less in that chronological order.

[22] It is very important to distinguish between *mathematization*, *formalization* and *axiomatization* – at least when doing *intuitionistically constructive mathematics*.

[3] I say 'alleged' because my admittedly scratchy archival research can only substantiate Gibbs as the source of this aphorism through a poem by Muriel Rukeyser (*Gibbs*, in the collection of poems titled, **A Turning Wind** (1939)), later codified in her brief biography of **Willard Gibbs** (Rukeyser, 1942), but only as '*a story is told of him* [Gibbs]', ibid, p. 280. To the credit of Paul Samuelson's characteristic candidness, he did refer to it as a quote from a '*supposed*' speech by Willard Gibbs. Such subtleties are lost on non-scholars like Lucas (see, below).

It is precisely this – the 'identity of the two media'[4] – that is questioned, indeed challenged by Brouwerian Intuitionistic Constructive mathematics. In simple terms, for Brouwer, and his Intuitionistic followers, there is no question of such an 'identity'; in fact, mathematics is prior to, and independent of, language and logic. As enunciated with the utmost clarity by (the mature) Brouwer (1952b, pp. 139-140; italics in the original), in the same year as Samuelson, in another continent, to a different audience, stated his conviction[5];

> "To begin with, the
> ### FIRST ACT OF INTUITIONISM
> *Completely separates mathematics from mathematical language, in particular from the phenomena of language which are described by theoretical logic, and recognizes that intuitionist mathematics is an essentially languageless activity of the mind .... .*
> …
> In [intuitionistic] mathematical thought .. , language plays no other part than that of an efficient, but never infallible or exact, technique for memorizing mathematical constructions, and for suggesting them to others; so that mathematical language by itself can never create new mathematical systems."

This is especially to be remembered in any context involving intuitionism, particularly in its Brouwerian variants, since he - more than anyone else, with the possible exception of Wittgenstein - insisted on *the independence of mathematics from logic*. In Brouwer's enunciation of the famous *first act of intuitionism* (Brouwer, 1981), there is the uncompromising requirement for (his version of) constructive mathematics to be independent of `theoretical logic' *and* to be '*languageless*'.

Imagine, therefore, the incredulity with which I read a leading advanced textbook on **Real Analysis with Economic Applications** *asserting* (Ok, 2007, p. 279; italics added:

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[4] Really, three – mathematics, language and logic.

[5] A half a century later, 'Mathematics *is* language' was transmogrified into 'Economic Theory *is* mathematical analysis', in the view of Lucas (1952, p. 9; italics in the original), whose knowledge of the philosophy, techniques and foundations of the possible existence (sic!) of a variety of types of mathematical analysis – constructive, computable, classical, non-standard is, to the best of my knowledge, fairly modest, if not highly jaundiced, to put it mildly. The full statement, and the Samuelsonian – *Foundations* - context in which the above Lucasian straitjacket is enunciated is (*ibid*):
> "I loved the *Foundations*. Like so many others in my cohort, I internalized its view that if I couldn't formulate a problem in economic theory mathematically, I didn't know what I was doing. I came to the position that mathematical analysis is not one of many ways of doing economic theory: It is the only way. Economic theory *is* mathematical analysis. Everything else is just pictures and talk.'
Suppose I, 'like many others in my cohort', decided to 'formulate a problem in economic theory' in a mathematics that was different from that which Lucas and his cohorts used; suppose also that I – and my cohorts – derived results diametrically opposed to those derived by 'Lucas and his cohorts', then who are we to believe? Does it boil down to which mathematical analysis is appropriate for 'formulating economic theory'? What criteria is one supposed to apply for choice of the 'appropriate' mathematical analysis for formulating economic theory?

"It is worth noting that in later stages of his career, he became the most forceful proponent of the so-called intuitionist philosophy of mathematics, which not only forbids the use of the Axiom of Choice but also rejects *the axiom* that a proposition is either true or false (thereby disallowing the method of proof by contradiction). The consequences of taking this position are dire. For instance, an intuitionist would *not accept the existence of an irrational number*! In fact, *in his later years, Brouwer did not view the Brouwer Fixed Point Theorem as a theorem*. (He had proved this result in *1912,* when he was functioning as a 'standard' mathematician).
If you want to learn about intuitionism in mathematics, I suggest reading - *in your spare time, please* - the four articles[6] by Heyting and Brouwer in Benacerraf and Putnam (1983)."

I don't suppose I should be surprised at this kind of preposterously ignorant and false assertions, observations and claims. These are made in a new advanced text book on mathematics for graduate (economic) students, published under the imprint of an outstanding publishing house, and peddled as a text treating the material it does contain `rigorously' (although the student is not warned that there are many yardsticks of 'rigour', and that which is asserted to be 'rigorous' in one kind of mathematics could be considered 'flippant and slippery' in another kind).

Yet, every one of the assertions in the above quote is false, and also severely misleading. Brouwer did not 'become the most forceful proponent of the so-called intuitionist philosophy of mathematics in *later stages* of his career; he was an intuitionist long before he formulated and proved what came, later, to be called the *Brouwer Fix-Point theorem* (cf. Brouwer, 1907, 1908A & 1908B).

Just for the record, even the fixed-point theorem came earlier than 1912. It is nonsensical to claim that Brouwer did not consider the 'Fixed Point Theorem as a theorem'; he did not consider it *a valid theorem in intuitionistic constructive mathematics*, and he had a very cogent reason for it, which was stated with admirable and crystal clarity when he finally formulated and proved it, forty years later, within intuitionistic constructive mathematics (Brouwer, 1952).

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[6]Given the shoddy scholarship displayed in this – and many other – assertions by Ok, it is not surprising he forgets (or, more likely, does not understand) to add Michael Dummett's important contribution to the mathematical philosophy of intuitionism as the fifth article for 'spare time' reading from the fine collection in Benacerraf & Putnam.

It is worse than nonsense - if such a thing is conceivable - to state that 'an intuitionist would not accept the existence of an irrational number'. Moreover, the law of the excluded middle is *not a mathematical* axiom; it is a logical law, which when *added* as an axiom to Heyting's system of first-order intuitionistic *logic* 'reduces' it to first-order *classical* logic[7]. In any case, the law of the excluded middle is accepted even by the intuitionists so long as meaningless - *precisely* defined - infinities are not being considered as alternatives from which to 'choose'[8].

As for the unfinessed remark about the axiom of choice being forbidden, the author should have been much more careful. Had this author done his elementary mathematical homework properly, Bishop's deep and thoughtful clarifications of the role of a choice axiom in varieties of mathematics may have prevented the appearance of such nonsense (Bishop1967, p.9):

> "When a classical mathematician claims he is a constructivist, he probably means he avoids the axiom of choice. This axiom is unique in its ability to trouble the conscience of the classical mathematician, but in fact it is not a real source of the unconstructivities of classical mathematics. A choice function exists in constructive mathematics, because a choice is *implied by the very meaning of existence*[9]. Applications of the axiom of choice in classical mathematics either are irrelevant or are combined with a sweeping appeal to the principle of omniscience[10]. The axiom of choice is used to extract elements from equivalence classes where they should never have been put in the first place."

Unfortunately, core areas of mathematical economics and game theory, with impeccable orthodox sanction, are replete with even worse false claims and assertions about constructivity, intuitionism and computability. I chose the phrase `even worse' most deliberately. The above inanities by Ok, admittedly in a textbook that may 'corrupt' the mind of fresh and innocent graduate students in economics, are just that: marginal textbook assertions that may pass - with luck - by the average reader without inflicting too much damage.

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[7] See also below, Appendix.

[8] Even as early as in 1908, we find Brouwer dealing with this issue with exceptional clarity (cf., Brouwer, 1908B, pp. 109-110; bold emphasis, added):

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

[9] See, also, Bishop & Bridges, 1985, p. 13, `Notes'.

[10] Bishop (op.cit, p. 9), refers to a version of the law of the excluded middle as *the principle of omniscience*.

However, I chose the phrase 'even worse' also to highlight the fact that the above attitudes and lack of understanding of the basics of Brouwerian Intuitionistic Mathematics must mean economics cannot be studied as an activity in 'problem solving' and the economist viewed as a 'problem solver', in the precise Turing-Simon sense. Yet lip service is paid to 'problem solving' and the 'problem solver' in standard economics, as if they can be underpinned by a 'real analysis' based on set theory plus the axiom of choice.

How, and why, does the claim that intuitionistic *constructive* mathematics is *logicless* manifest itself? Consider the following pictures and the series of numbers below them (cf. **Mathematics Magazine**, Vol. 79, Feb., 2006, p.44)[11]:

$$0, 1, 3, 6, 10, 15, 21, 28, 36, 45, 55, 66, 78, 91, 105, 120 \, .$$
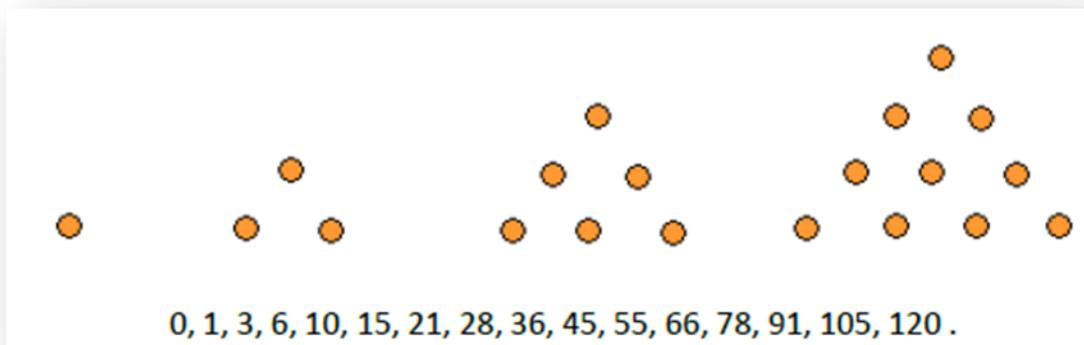
Now, consider the following formulas:

$$t_k = 1 + 2 + \cdots + k \Rightarrow 2^4 = 15 + 1 = t_5 + t_1,$$
$$3^4 = 66 + 15 = t_{11} + t_5,$$
$$4^4 = 190 + 66 = t_{19} + t_{11},$$
$$\vdots$$
$$n^4 = t_{n^2+n-1} + t_{n^2-n-1}.$$

Suppose one had some 'lego' pieces and 'experimented' as follows:

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[11] Just as I was trying to use these 'pictures', formulas and *constructions* to illustrate, in as *intuitively* elementary way as possible, at the first 'outing' of this paper, in the form of a contribution at a conference, earlier this year, I was interrupted by an apparent *saccente*'s remark that he could interpret the formulas and results *logically*! I did not bother to go on, simply gave up, because it would have taken the time required for a whole course on intuitionistic logic to elucidate the kind of non-intuitionistic principles his kind of logic entailed.
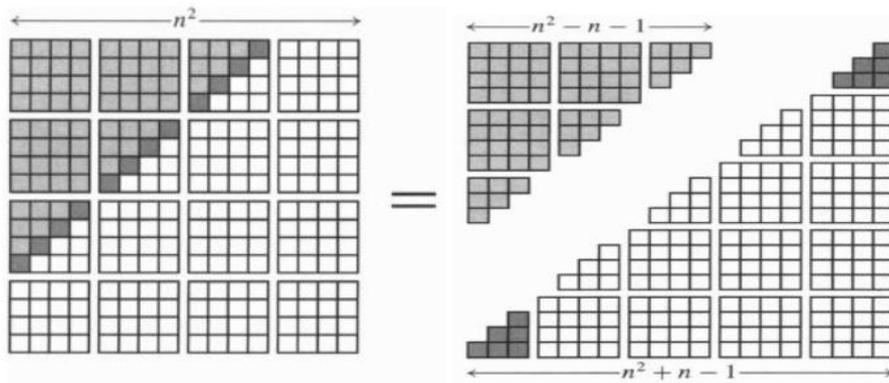
Is there any role for *language* or *logic* in these exercises? If *logic*, what kind of logic – and, then, what underlying principles would have to be invoked to make that *logic* operate on these formulas? Is some kind of *transfinite induction* invoked, if *classical logic*[12] is used to derive the results suggested by the 'pictures', series of numbers and formulas.

In Brouwerian Intuitionistic Constructivism, the first picture would be considered constructive sets of sequences, generated in a precisely defined, and common, way. These, he called, *spreads*. Given the intuitive construction of the natural number sequence, the *spread* in the picture is generated in a precisely defined, common, way: and one more 'ball' to the first, second, third, …, base. This is a *rule*, which, for admissible *finite* sequences (of natural numbers), generates (at least) one admissible successor. As we can intuit from the picture, an infinite sequence of natural numbers belong to this spread, whenever a finite (sub)sequence does. Since this particular *spread* admits only finitely many successors, to each admissible finite 'picture', the triangularly arranged set of 'balls', it is called a *fan*[13].

These intuitive definitions allow the derived result, from the formulas, pictures and series of numbers, to be justified by the intuitionistic construction – without any appeal to any kind of logic (Heyting-type intuitionistic logic or not).

This is the way one should read *PCC*, in terms of *spreads* and *fans* – then, the 'imaginary experiment' Sraffa suggests (*ibid*, p. 26), for *constructing* the 'Standard system', from the given 'actual economic system', would be perfectly intelligible intuitive constructions, themselves the 'proofs' of the result. Ask yourself, as an innocent economist, whether, for example, the *standard commodity* and the *standard system* in *PCC* could not be *constructed*

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[12] As, obviously, intended by the above *saccente*.
[13] See, however, Brouwer (1952b, p. 512) for a more detailed definition of a *fan*.

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from a sufficiently large number of 'lego' pieces – or 'produced' by a Turing Machine, itself constructed from 'lego' pieces?[14]

Indeed, on 3 September John Hicks wrote a warm, appreciative, letter[15], on after having read *PCC*, in which he wondered (italics added):

> "You tell us that your work on the subject goes back a long way – you mention Frank Ramsey; is it possible that it was somehow through you and your mathematical friends that von Neumann got onto what is in so many ways a similar *construction* …? I have never been able to understand how he should have hit on it out of the blue. Formally, I believe your standard system is identical with the von Neumann equilibrium, though it arises in response to a different question. But the model, even to the treatment of fixed capital, is exactly the same."

Sraffa replied on 8 September, 1960, on the issues raised in the above part of Hicks' letter, as follows (italics added):

> "The reason for the analogy between the several *constructions* seems to me to lie in their having a common source, although by devious ways, in the old classical economists (…..): that is undoubtedly the case for the treatment of fixed capital as a joint product. There are however important differences with the von Neumann *construction*, and the saddle point and the 'free goods' are peculiar to it: …."

Note the use of the word '*construction*' by both Hicks and Sraffa – and not words and phrases like *formalization*, *formal model*, *axiomatic method*, etc[16].

And so on!

More than a century ago, as Ehrlich (2006, pp. 1-2; italics added) pointed out, Russell was explicit in his dismissal of any philosophical or foundational enrichment – rigorous or not – of the nature of the triptych of the *infinitesimal*, the *infinite* and the *continuum*:

> "In his paper Recent Work On The Principles of Mathematics, which appeared in 1901, Bertrand Russell reported that the three central problems of traditional mathematical philosophy - the nature of the infinite, the nature of the infinitesimal, and the nature of the continuum - had all been 'completely solved' . ... Indeed, as Russell went on to add: 'The solutions, for those acquainted with mathematics, are so clear as to leave no longer the slightest doubt or difficulty' ... . According to Russell,

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[14] See: http://www.legoturingmachine.org/

[15] A copy of the letter, together with Sraffa's reply was made available to those of us who participated in the *Hicks Centennial* conference at the University of Bologna, October, 2004.

[16] I have developed one strand of this discussion and stance, in some detail, in Velupillai (2011, particularly on pp. 2, ff.).

the structure of *the infinite* and *the continuum* were completely revealed by Cantor and Dedekind, and the concept of *an infinitesimal* had been found to incoherent and was `banish[ed] from mathematics' through the work of Weierstrass and others."

Thus arose the dominance, in the 20th century, of the mathematics, the mathematical philosophy and the foundations of mathematics, in terms of what may now be called the research program of Frege and Cantor, Dedekind and Weierstrass, Peano and Zermelo, codified in *Hilbert's Program*.

Brouwer, standing on the shoulders of Kronecker and Poincaré, Borel and Lebesque, underpinned by a (restricted) Kantian philosophy of *intuition*, single-handedly challenged the Hilbert Program and provided a coherent, deep and, ultimately, *languageless*, *logicless* (perhaps, even *negationless*) philosophical foundation for an intuitionistic constructive mathematics. I shall call this codification *Brouwer's Program*.

This paper is a companion piece to the 'manifesto' enunciated in Velupillai (1996), where it was argued that there was a *Computable Alternative in the Formalization of Economics*. Now, a decade and a half later, I am still in favour of the *computable* alternative, but less enthusiastic about its *formalization* – hence, the reorientation towards an intuitionistic constructive underpinning of economic theory. As Heyting (1971, p. 106; italics added), cogently argued, from a Brouwerian standpoint:

> "It must be remembered that *no formal system* can be *proved* to represent *adequately* an intuitionistic theory. There always remains *a residue of ambiguity* in the interpretation of signs, and it can *never be proved with mathematical rigour that the system of axioms really embraces every valid method of proof*."

If *PCC* is interpreted as an intuitionistic theory, as I now do, then 'no formal system can be proved to represent it adequately'. All the matrix theoretic calisthenics in the formalization of *PCC* are empty exercises with no necessary valid Sraffian economic interpretations to them – especially none whatsoever on returns to scale. Conversely, the axiomatic treatment of *The Theory of Value* (Debreu, 1959)[17] cannot be 'proved to represent adequately an intuitionistic theory.'

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[17] I choose this reference for simplicity; I could equally well have chosen von Neumann-Morgenstern (1947), as an earlier example – or any number of later contributions to axiomatic choice, production and game theories – to make the same claim.

In the next section I discuss and present two concrete examples of the applications of intuitionistic constructive mathematics in economics and finance, respectively. The third section is devoted to a general discussion of intuitionistic logic as problem solving. Sections four and five are, respectively, on Hilbert's and Brouwer's programs for the foundations of mathematics. The concluding section is a brief programmatic hope for the future of an intuitionistically constructive mathematical economics.

§2. *Fleeing, Principle of Continuity, Free Choice Sequences & Intuitionistic Logic[18] : Implications in Economics*

In this section I want to consider four examples of intuitionistic constructive mathematical and intuitionistic logical issues in elementary economics, finance, game theory and behavioural sciences:

- Bolzano-Weierstrass and the *Fleeing Property* in Computable General Equilibrium;
- Brouwer's *Principle of Continuity* in elementary finance and accounting theory;
- Constructive Proof of the Existence of a Winning Strategy in Chess
- Intuitionistic Logic as *Problem Solving*

§2.1 The Unconstructifiability of the Classical Bolzano-Weierstrass Theorem

In Brouwer (1952), the reason why his original theorem was unacceptable in intuitionistic constructive was first stated in general terms:

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

Note how Brouwer refers to a 'classical ... form of the fixed-point theorem'. The *invalidity* of the Bolzano-Weierstrass theorem[19] in *any* form of constructivism is due to its reliance on the *law of the excluded middle in an infinitary context of choices* (cf. also Dummett, 1977, pp.

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[18] I will have to assume some familiarity with the terminology and definitions *of mathematical intuitionism*, as in, for example, Heyting (1971). I expect the interested reader, that ever elusive character, who takes the arguments in this paper seriously, will equip him/herself with the necessary background knowledge, as in the case of computability in the case of Velupillai (1996). There, too, the necessary 'tutorial' in computability theory came later (in Velupillai, 2000), as it will, in this case for mathematical intuitionism.

[19] For the 'benefit' of the absolute novice in these things, a simple version of this celebrated theorem is:
*Every bounded sequence **has** a convergent subsequence*.
The word '*has*' is an euphemism for the many sins of undecidable disjunctions in the eyes of the constructive mathematician.

10-12). The part that invokes the Bolzano-Weierstrass theorem entails *undecidable disjunctions* and as long as any proof invokes this property, it will remain unconstructifiable.

More concretely, the notion of *set* in intuitionistic constructive mathematics is equivalent to *spreads* (as defined intuitively, above) or *species* (Heyting, *ibid*, chapter 3). The latter is defined in terms of a common intuitionistic property of a collection of elements[20]. Then, consider the following special case of the Bolzano-Weierstrass theorem (*loc.cit.*, §3.4.6):

> <u>Theorem</u>
> To every bounded infinite *species* of real numbers a point of accumulation *can be found*.
> <u>Remark 1</u>
> Note that this is an intuitionistic version of a special case of the Bolzano-Weierstrass theorem, in view, for example, of the use of *species* (and not sets, as in classical mathematics) and the phrase '*can be found*' (rather than 'there exists').
> <u>Proof</u>
> Define a sequence, $\{a_n\}$ as follows.
> - $a_n = 2^{-n}$, whenever no sequence 0123456789 occurs in the decimal expansion of π.
> - $a_n = 1 - 2^{-n}$, whenever such a sequence does occur in the decimal expansion of π.
>
> The species of real numbers $\{a_n\}$ is bounded and infinite; but it is intuitionistically undecidable whether the point of accumulation is *0* or *1* (or even whether such a thing 'exists').
> <u>Remark 2</u>
> Note, however, that this does not imply that *at some future* date someone would not be able to decide, intuitionistically, that there exists a point of accumulation and it is either *0* or *1*. To go beyond this possibility and to prove intuitionistically that there is no hope at all of demonstrating the validity of the full Bolzano-Weierstrass theorem, Brouwer used the *fleeing property* (cf. Brouwer, 1952b, p. 510).

Now, suppose you know the meaning of Brouwer's conceptual – i.e., mentally imagined – device of the '*fleeing property*' and the way it is used to demonstrate, in intuitionistically constructive mode, that there is, inherently, an *undecidable disjunction* in the classical mathematician's version of the *Bolzano-Weierstrass theorem*. Would you attempt to construct algorithms invoking such a theorem to show the 'existence' of a computable Walrasian or Nash equilibrium? Yet, this is precisely being done at almost every level of economic theory and applied economics.

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[20] Note: this has nothing to do with the usual 'predicative' definition in classical set theory.

The more pernicious indulgence in mathematical economics with computational claims is the lack of understanding that the same intuitionistic framework applies to the demonstration that *Sperner's Lemma is invalid constructively*.

§2.2 Brouwer's *Principle of Continuity* in Elementary Finance Theory

"As for the question of *replacing* rows of closely spaced *dots by solid lines*, you can do that too if you want to, and the governors of the exchange and the community of brokers and dealers who make markets will bless you. *If you think in terms of solid lines while the practice is in terms of dots and little steps up and down, this misbelief on your part is worth, I would say conservatively, to the governors of the exchange, at least eighty million dollars per year.*"
Osborne (1977), p. 34; italics added.

Maury Osborne's warning to traders in the stock market, not to approximate by the continuous that which is intrinsically discrete, made over a quarter of a century ago applies with even more force today, when everything seems to have been 'financialized'! Today, I would put the conservative estimate at more than several multiples of that figure of 27 years ago, even adjusting for inflation. The reason, once again, a reliance on an illegitimate domain of analysis, unrealistic assumptions and the wrong mathematics for analyzing digital data, by digital agents in a digital medium using a digital machine for computing discrete numbers.

This mistake is not made by Stecher and Van Atten (2013) who, using a version of (intuitionistic) *choice sequences* and Brouwer's *Principle of Continuity*, show how to resolve an elementary conundrum in deciding between two ways of valuing equities, in the domain of natural numbers (or integers). I include this as an important example of applications of notions of intuitionistic constructive mathematics, and thereby getting familiar with a few more notions – in addition to spreads, species and the fleeing property of the previous subsection – that are basic to intuitionistic thinking.

Consider the following two formulas for 'estimating the intrinsic worth of a share of stock' (i.e., equities):

$$P_t = \sum_{\tau}^{\infty} \frac{D_{t+\tau}}{(1+r)^{\tau}} \quad ....(1)$$

$$P_t = S_t + \sum_{\tau=1}^{\infty} \frac{R_{t+\tau}}{(1+r)^{\tau}} \quad\text{........(2)}$$

Where:

(1) states that the share price, P, reckoned at its 'intrinsic worth', is the discounted value of all future dividends, D;
(2) states that the 'value of a share of stock is the accounting book value of shareholders' equity, S, plus the discounted value of future abnormal accounting income, R.

The orthodox economist would balk at the assumptions and (economic) arguments that enabled Stecher and Van Atten to derive these two simple formulas, but they are not being frivolous in any way. In any case, they begin at the beginning, i.e., with Irving Fisher's classic *Rate of Interest* of 1907 (which I think is the serious, academic, version of his later, more popular, *Theory of Interest*). More importantly, they assume the numbers that make their way into the above formulas, to calculate them, are integers and they do so on the eminently practical ground that this is 'realistic'. The orthodox economist or finance theorist is indiscriminate in the choice of the domain of relevant numbers, opting for the standard reals for theoretical reasons, yet ignoring that no real world stream of values to calculate the various inputs to the above two formulas cannot be arbitrary real numbers. Finally, somehow, Stecher and Van Atten are able to be impressively agnostic, perhaps due to the induced bliss of innocent ignorance, about the whole paraphernalia of rational choice under risk!

I shall let all this pass, because I am convinced their more important argument is adaptable to the orthodox theorist's framework with very minor effort.

They then point out, invoking impeccably sound orthodox literature in accounting and finance theory, that the two formulas should lead to identical estimated earnings, whereas empirical results seem to show the 'accounting income approach generally outperforming the dividends approach.' (*ibid*, p.1). The theoretical resolution of this discrepancy is sought by them by interpreting the valuing in terms of intuitionistic *choice sequences* and *Brouwer's continuity principle* (cf. van Atten & van Dalen, 2002) which enables them to derive the following result (*loc.cit.*, p.6):

<u>Proposition</u>

If there is no reliable way to forecast a company's accounting earnings or dividend payouts *beyond a finite horizon*, then formulas (1) and (2) do not lead to identical results.

I believe there is a great deal of pedagogical value in using this paper to introduce an elementary way of considering intuitionistic mathematics and thereby helping the resolution of the more famous puzzles that confront the orthodox economic and finance theorist. I have in mind, in particular, the celebrated equity premium puzzle – but also those that have been derived by indiscriminate anomaly mongering by orthodox behavioural economic and finance theorists.

§2.3 Max Euwe's Intuitionistically Constructive Analysis of Chess

"We now prove that every finite extensive game with perfect information has a subgame perfect equilibrium. *Our proof is constructive … .*"
Osborne & Rubinstein, 1994, p. 99; italics added.

This is one of many such incorrect claims of *constructive proofs*, particularly in game theoretic contexts[21], but also, equally pervasive, in the general area of mathematical economics, especially in its general equilibrium versions (see, above, section §2.1).

In his masterly, sympathetic and comprehensive biography of Brouwer, van Dalen (2005, p. 636; italics added) mentions, almost as an aside:

"In 1929 there was another publication in the intuitionistic tradition: an intuitionistic analysis of the game of chess by Max Euwe[22]. It was a paper in which the game was

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[21] For example Giocoli (2003), is based on an incorrect understanding of constructive proofs – especially in game theory of the von Neumann-Morgenstern variety. There is, naturally, not an iota of any aspect of intuitionistic constructivism touched upon in the somewhat confusing definition(s) of a constructive proof. Moreover, the remark on 'constructive existence proofs of general economic equilibrium' (*loc.cit*, p. 35) is further evidence that the author has not quite mastered the mathematical meaning of 'constructive', especially in the context of a (mathematical) proof. But I suspect this deficiency is due to his reliance on imperfect sources than any incompetency on his part.

[22] Max Euwe was, later, the world chess champion, defeating Alekhine in the championship of 1935. He characterized Brouwer as 'The best mathematician Holland has ever had, and also one of the world's greatest specialists' (Münninghoff, 2001, p. 17). A close reading of Münninghoff (ibid) convinces me that his masterly intuitionistic theoretical analysis of Chess (Euwe, 1929) was not a purely theoretical exercise. Indeed, during an interview in 1978 Euwe stated:

"If he could not get the initiative or some advantage in the opening, he was willing to enter complications to try to muddy the water. So I went to Vienna for a few months to study Becker's files on the openings, which were the most complete and up to date at that time."

viewed as a *spread … .* Euwe carried out *precise constructive estimates* of various classes of [Chess] games, and considered the influence of the rules for draws. When he wrote his paper he was not aware of the earlier literature of Zermelo and Dénès König. Von Neumann called his attention to these papers, and in a letter to Brouwer Von Neumann sletched a classical approach to the mathematics of chess, pointing out that *it could easily be constructivized.*"

To this day, to the best of my knowledge, 'it' is still waiting to be 'easily constructivized'; von Neumann, himself, never contributed to the constructivization of any aspect of orthodox game theory[23].

Max Euwe (1929) is the *only* contribution to any variant of game theory – in this case, for the specialized case of Chess – from a comprehensive Brouwerian intuitionistic constructive mathematical point of view. The very title[24] of his pioneering contribution *can* be misleading, if the reader does not pay adequate attention to the immediately succeeding heading of §1 of the paper: '*Unseren betrachtungen liegt die BROUWERsche Mengendefinition zu Grunde*'[25].

Apart from the important point highlighted by van Dalen – that Euwe 'carried out *precise constructive estimates* of various classes of [Chess] games' – there was his absolutely crucial consideration of the implications of the *reduction* of the general *solvability* ('*lösung*'). *to that of a finite problem* ('*die Reduktion zu einem endlichen Problem*'). It is at this point that the notion of 'playability' in Arithmetical Games, within the framework of computability theory, touches on the more general – mathematically – notion of 'reducibility' of a potentially infinite play to a finite problem.

I assume this is a reference to Oskar Becker who, in his *habilitationsschrift*, had dealt with Browuer's choice sequences in detail (van Atten, 2005). It was during a correspondence with Becker that Heyting noted that he had developed his original attempt at formulating an intuitionistic logic by going through the scheme in **Principia Mathematica**, deleting the intuitionistically unacceptable 'laws of logic' (see below).

[23] Despite the claim in his joint paper with George Brown (1950) that 'the proof is constructive in a sense that lends itself to utilization when actually computing the solutions of specific games.' It is not often realized, even by the gods, that algorithms underpinned by computability theory are not necessarily constructive in the sense of Brouwer's intuitionistic mathematics. This is, of course, well known to those who work in Arithmetical Games (cf., Velupillai, 2000).

[24] '*Mengentheoretische Betrachtungen über das Schachspiel*' – i.e., *Set theoretic considerations on Chess* (my translation). The unwary reader, remembering the title of Zermelo's classic, *Über eine Anwendung der **Mengenlehre** auf die Theorie des Schachspiels*, may be misled into thinking that Euwe was invoking the same kind of set theory as was used in the former's contribution – unless the same reader also takes the next line in Euwe's paper seriously and begins to wonder *what was so different about Brouwer's definition of sets*!

[25] '*Our considerations are based on Brouwer's definition of sets*' (my translation).

It is also the felicitous way Brouwer's intuitionistically constructive definition of *sets* – in terms of *spreads* and *species* – interacts with his fertile notion of (*lawless* – a qualifying word introduced by Kreisel – and *lawlike*) **choice sequences**. These two intuitionistically constructive notions are the key components in Euwe's analysis of the theoretical possibilities (based on Browuer, 1927) of deriving precise numerical estimates of winning plays in Chess and, in special cases, of draws, and of 'reducibility to finite' considerations.

These are the notions, in addition to the *fleeing property*, that have fertilized the use of intuitionistically constructive mathematical analysis of economics, finance and game theories. Using just the three key contributions I have invoked in sections 2.1 -2.3 (Brouwer, 1952, Stecher & van Atten, 2013 & Euwe, 1929), it is possible to introduce the methods of Brouwerian intuitionistically constructive mathematics to serious graduate students in economics – not frivolously, as suggested by Ok (op.cit), only 'in one's spare time'.

§2.4. Intuitionistic Logic as Problem Solving

> "*The calculus of problems is formally identical with the Brouwerian intuitionistic logic, which has recently been formalized by Mr. Heyting.*"
> Kolmogorov, 1932 (1998), p. 328; italics in the original.

My interest in computable behavioural economics – or, as I have been referring to it as *Classical Behavioural Economics*, in recent years - has one of its anchorings in the formalization of *Problem Solving* by Turing (1954) and Simon (Newell & Simon, 1972), both of whom underpin the *problem solver* in terms of a Turing Machine[26].

Hilbert had begun the search for a rigorous definition of *problem*, *problem solving* and, eventually also, the *problem solver*, by explicitly stating in his influential address to the Paris International Congress of Mathematicians in August, 1900, titled famously and simply: *Mathematical Problems* (Hilbert, 1900, p.444, last italics in the original):

> "[T]he conviction (which every mathematician shares, but which no one has as yet supported by a proof) that every definite mathematical problem must necessarily be susceptible of an exact settlement, either in the form of an actual answer to the question asked, or by the proof of the impossibility of its solution and therewith the necessity failure of all attempts. .....

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[26] Simon refers to the problem solver as an *Information Processing System* (**IPS**), who is, essentially, a *Turing Machine*.

Is this axiom of the solvability of every problem a peculiarity characteristic of mathematical thought alone, or is it possibly a general law inherent in the nature of the mind, that all questions which it asks must be answerable? For in other sciences also one meets old problems which have been settled in a manner most satisfactory and most useful to science by the proof of their impossibility. ....

This conviction of the *solvability of every mathematical problem* is a powerful incentive to the worker. We hear within us the perpetual call: There is *the problem*. Seek its solution. You can find it by pure reason, for in mathematics there is no *ignoramibus*."

In that same famous lecture, Hilbert had also stated[27], clearly and unambiguously, the acceptable criteria for the '*solution of a mathematical problem*':

"[I]t shall be possible to establish the correctness of the solution by means of a *finite number* of steps based upon a *finite number* of hypotheses which are implied in the statement of the *problem* and which must always be exactly formulated. This requirement of logical deduction by means of a *finite number* of processes is simply the requirement of *rigour in reasoning*."
ibid, p. 409; italics added.

When I was developing the field of computable economics (from about 1979), it was against the backdrop of viewing *economics as problem solving* in the above Turing-Simon sense, with foundations in computability theory, which was an outcome of Hilbert's inadvertent 'challenge'.

But it was only about a decade and a half later that I understood that problem solving had a natural 'habitat' in *intuitionistic logic*. That intuitionistic logic provided the natural foundations for problem solving was persuasively and rigorously put forth by Kolmogorov (1932; 1998; see opening quote of this section) more than eighty years ago[28].

In the earlier classic Kolmogorov also showed, without a shadow of a doubt, why Brouwer – in particular – was adamant on his refusal to accept the *tertium non datur* (*tnd*) and the *law of double negation* in any 'reasoning' which *goes beyond the finite*[29]:

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[27] Hilbert's vision of the *solvability of mathematical problems*, and *criteria for solvability*, were interpreted by Brouwer, correctly in my opinion, as a way of unconditionally accepting the untrammeled validity of the *tertium non datur*. Hence, of course, violating a basic tenet of intuitionistic logic.

[28] A sequel to the classic that eventually led to the *BHK* – **Brouwer-Heyting-Kolmogorov** – system (Kolmogorov, 1925). BHK also refers to *Brouwer-Heyting-Kreisel* (cf. Ruitenburg, 1991, p. 156)!

[29] Hence the importance of Euwe's reduction to a *finite problem* in his intuitionistic constructive approach to Chess, thereby eschewing any appeal to the *tnd*..

> "[W]ithout the help of the principle of excluded middle it is impossible to prove any proposition whose proof usually comes down to an application of the principle of *transfinite induction*. For example a proposition of that kind is: every closed set is the sum of a perfect set and a denumerable set.
> The proof of such propositions is often carried out without the help of the principle of *transfinite induction*. But all these proofs rest upon *the principle of excluded middle, applied to infinite collections*, or upon *the principle of double negation*."
> Kolmogorov, 1925 (1967), p. 436; italics added.

It was to Brouwer's credit that he was early able to intuit (sic!) the *transfinite* implications of the *unrestricted* use of the *tertium non datur* (and the law of double negation). In a problem solving context, then, it was clear – even to me – that it was *impossible to rely on classical logic* for studying *solvability* in a mathematical framework.

On 8 September 1930 Hilbert gave the opening address to the German Society of Scientists and Physicians, in Königsberg[30], titled: *Naturkennen und Logik*. This lecture ended famously echoing those feelings and beliefs he had expressed in Paris, thirty years earlier, (Dawson, 1997, p. 71; italics added):

> "For the mathematician there is no Ignoramibus and, in my opinion, not at all for natural science either. … The true reason why [no one] has succeeded in finding an *unsolvable problem* is, in my opinion, there is *no unsolvable problem*. In contrast to the foolish Ignoramibus, our credo avers:
> We must know,
> We shall know."[31]

A day before that, on Sunday, 7th September, 1930, at the Roundtable Discussion on the final day of the Conference on Epistemology of the Exact Sciences, organised by the *Gesellschaft für Empirische Philosophie*, a Berlin Society allied to the *Wiener Kreis*, the young Kurt Gödel had presented what came to be called his *First Incompleteness Theorem*. In fact, in one fell swoop, Gödel had shown that it was *recursively demonstrable* that in the formal system of classical mathematics, assuming it was *consistent*, there were true but unprovable statements - i.e., *incompleteness* and, almost as a corollary to this famous result, also that

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[30] Where he was also honoured, in those enlightened pre-Nazi days, by being presented, by the Königsberg Town Council, with an 'honorary citizenship'. Incidentally, it is now Kaliningrad – and no longer part of Germany!

[31] The marker that was placed over Hilbert's grave in Göttingen had etched on it the German original of these last two lines:
"*Wir müssen wissen.*
Wir werden wissen."

mathematics was *inconsistent*[32]. Two of the pillars on which Hilbert was hoping to justify formalism had been shattered[33].

There remained the third: *Decidability*. The problem of resolving this question depended on finding an acceptable - to the mathematician, metamathematician and the mathematical philosopher - definition of *definite finitary method*. It is on this 'altar' that standard mathematical economics falters – and it is this that underpins problem solving and the problem solver in the Turing-Simon sense; it is this that underpins, in yet another – the fourth – way, the case for an *Intuitionistic approach to mathematical economics*.

§2.4 a. A Brief Appendix on Heyting's Formalisation of Intuitionistic Logic

> "Wie sind sie überhaupt auf Ihre Axiome gekommen?"
> Becker to Heyting, 19 March, 1933 (Troelstra, 1988)

To this interesting query from Becker, the candid response from Heyting, on 23 September, 1933, was as follows, in Troelstra, *ibid*, p. 8, underlining in the original):

> "Sie fragen, wie ich zu meinen Axiomen der Logik gekommen bin. Ich habe die Axiome und Sätze der Principia Mathematica gesichtet und aus den zulässig befundenen ein System von unabhängigen Axiomen gemacht."

For convenient reference for a background to this section, a concise statement of Heyting's formal system of first-order intuitionistic logic, is as follows[34]:

Axioms

$$p \rightarrow (q \rightarrow p)$$

$$[p \rightarrow (q \rightarrow r)] \rightarrow [(p \rightarrow q) \rightarrow (p \rightarrow r)]$$

$$p \rightarrow (q \rightarrow p \cap q)$$

$$p \cap q \rightarrow p \qquad p \cap q \rightarrow q$$

$$p \rightarrow p \cup q \qquad q \rightarrow p \cup q$$

$$(p \rightarrow r) \rightarrow [(q \rightarrow r) \rightarrow (p \cup q \rightarrow r)]$$

---

[32] This result, in its full formal version, is known as Gödel's *Second Incompleteness Theorem*: the consistency of a mathematical system cannot be proved within that system itself.

[33] Incidentally, Arend Heyting, too, was present at the conference, giving a talk on *Intuitionism*, while Rudolf Carnap and John von Neumann gave their talks on logicism and formalism, respectively.

[34] One obtains classical logic, from the above Heyting system, by adding either the law of the excluded middle or the law of double negation (which means that one is sanctioned, in orthodox mathematics, to invoke and use, indiscriminately, transfinite induction). This is, of course, the reversal of the procedure Heyting claims to have adopted, when he devised his scheme (see above).

$$(p \to q) \to [(p \to \neg q) \to \neg p]$$

$$\neg p \to (p \to q)$$

$$p(t) \to \exists x p(x) \qquad \forall x p(x) \to p(y) \ (x \ free \ in, and \ t \ free \ for \ x \ in, p)$$

$$x = x \qquad p(x) \cap x = y \to p(y)$$

Rules of Inference

$$\frac{p, p \to q}{q} \ (all \ variables \ free \ in \ p \ also \ free \ in \ q)$$

$$\frac{q \to p(x)}{q \to \forall x p(x)} \qquad \frac{p(x) \to q}{\exists x p(x) \to q} \qquad (x \ not \ free \ in \ q)$$

§4. Hilbert's Program

> "Hilbert's program .... was driven by dual beliefs. On the one hand, Hilbert believed that mathematics must be rooted in *human intuition*. ... It meant that intuitively bounded thought (*finitary* though, he called it) is trustworthy, and that mathematical paradox can arise *only when we exceed those bounds to posit unintuitable* (i.e., *infinite*) *objects*. For him, *finite arithmetic* and *combinatorics* were the paradigm *intuitable* parts of mathematics, and thus *numerical calculation was the paradigm of finitary thought*. All the rest -- set theory, analysis and the like - he called the 'ideal' part of mathematics. ..... On the other hand, Hilbert also believed that this ideal part was sacrosanct. No part of mathematics was to be jettisoned or even truncated. 'No one will expel us.' he declared, 'from the paradise into which Cantor has led us'[35]"
>
> Carl Posy, 1998, pp. 294-5; italics added

So, where was the difference between Hilbert's Program and *Brouwer's Program*[36]? Let me first outline a version of the *Grundlagenkrise* from the point of view of the (phyrric?) triumph of the *Hilbert Program* – at least in mathematical economics.

Summarising the tortuous personal and professional relationship between Brouwer and Fraenkel, van Dalen (2000, p. 309) concluded that:

> "Fraenkel also should be credited for pointing out a curious psychological hypocrisy of Hilbert, who to a large extent adopted the methodological position of his adversary – 'one could even call [Hilbert] an intuitionist' - (Fraenkel, 1927 p. 154). Although

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[35] The exact quote is as follows, (Hilbert, 1925, p. 191):

'No one shall drive us out of the paradise which Cantor has created for us.'

To which the brilliant 'Brouwerian' response, if I may be forgiven for stating it this way, by Wittgenstein was: (Wittgenstein, 1939, p.103):

"I would say, 'I wouldn't dream of trying to drive anyone out of this paradise.' I would try to do something quite different: I would try to show you that it is not a paradise - so that you'll leave of your own accord. I would say, 'You're welcome to this; just look about you.'"

[36] To the best of my knowledge, no one has referred to Brouwer's 'program of research' on refounding mathematics as an *intuitionistically based constructive activity* as 'Brouwer's Program'.

the inner circle of experts in the area ... had reached the same conclusion from time before, it was Fraenkel who put it on record."

So, why was there a *Grundlagenkrise*? Why, in early October, 1928[37] did Hilbert write Brouwer as follows:

> "Dear Colleague,
> Because it is not possible for me to cooperate with you, given the incompatibility of our views on fundamental matters, I have asked the members of the board of managing editors of the Mathematische Annalen for the authorization, which was given to me by Blumenthal and Carathéodory, to inform you that henceforth we will forgo your cooperation in the editing of the Annalen and thus delete your name form the title page. And at the same time I thank you in the name of the editors of the Annalen for your past activities in the interest of our journal.
> Respectfully yours,
> D. Hilbert"

This letter[38], written at the tail end of the *Grundlagenkrise*, marked the beginning of the end of it, and silenced Brouwer[39] for a decade and a half. Why, if they were both `intuitionists' did Hilbert and his 'Göttinger' followers, former students and admirers `silence' him in this deplorably undemocratic way? Were they afraid of an open debate on the exact mathematical meaning of intuitionism and constructive mathematics? Did they take the trouble to read and understand Brouwer's deep and penetrating analysis of mathematical thinking and mathematical processes? There is sad, but clear evidence that Hilbert never took the trouble to work through, seriously, with the kind of foundational case Brouwer was making; contrariwise, Brouwer took immense pain and time to read, work through an understand the foundational stance taken by Hilbert and his followers.

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[37] I am slightly unsure about the exact date, for which I am relying on van Dalen (2005). There seems to be a slight discrepancy in this connection. van Dalen (ibid, p. 599) reports that a telegram from Erhard Schmidt was delivered to Brouwer on 27 October, 1928, asking him 'not to undertake anything before' talking to Carathéodory, who was on his way to meet Brouwer. This referred to two letters, from Göttingen, that had already been delivered to Brouwer before the arrival of Carathéodory, who duly arrived in Laren, where Brouwer was living, on 13 October, 1928. One or the other dates has to be slightly incorrect!

[38] This battle between the two protagonists in the *Grundlagenkrise*, Hilbert and Brouwer, was referred to as the *'Frosch-Mäusekrieg'* by Einstein in his letter to Max Born on 27 November, 1928. Einstein, who was also a member of the editorial board of the *Mathematische Annalen*, did not support Hilbert's unilateral and extraordinary action to remove Brouwer from the board.

[39] In van Dalen's poignant description, the once effervescent, immensely productive, and active Brouwer (van Dalen, 2005, pp. 636-7):

> "[F]elt deeply insulted and retired from the field. He did not give up his mathematics, but he simply became invisible. ... Even worse, he gave up publishing for a decade .. . His withdrawal from the debate did not mean a capitulation, on the contrary, he was firmly convinced of the soundness and correctness of his approach."

This is precisely the point of Viscount Morley's famous aphorism: '*You have not converted a man because you have silenced him.*' (**On Compromise**, 1874).

What were the issues at the centre of the *Grundlagenkrise*, leaving aside the personality clashes? As I see it there were three foundational issues, on all of which I believe Brouwer was eventually vindicated:

- The invalidity of the *tertium non datur* in infinitary mathematical reasoning;
- The problem of *Hilbert's Dogma* - i.e., `existence ⇔ consistency' vs. the constructivist credo of `existence as construction', in precisely specified ways;
- The problem of the continuum - and, therefore, the eventual place of Brouwer's remarkable introduction of *choice sequences* and the *ideal mathematician*[40] whose time seems to have come only in recent years;

Carl Posy, reflecting on '*Brouwer versus Hilbert: 1907 – 1928*' (*op.cit*), from a Kantian point of view[41] - both Brouwer and Hilbert had been deeply influenced by Kant, and Hilbert, after all, grew up in Königsberg, which Kant never left!! - summarised the outcome of the *Grundlagenkrise* in an exceptionally clear way, as follows (*ibid*, pp. 292-3):

> "[Hilbert] won politically. Although a face-saving solution was found, the dismissal [from the Editorial Board of the *Mathematische Annalen*] held. Indeed, Brouwer was devastated, and his active research career effectively came to an end.
> [Hilbert] won mathematically. Classical mathematics remains intact, intuitionistic mathematics was relegated to the margin. .... .
> And [Hilbert] won polemically. Most importantly ... Hilbert's agenda set the context of the controversy both at the time and, largely, ever since."

Quite apart from whether Hilbert actually 'won', at least on the third front, - especially in the light of the subsequent quasi-constructive and partly-intuitive 'revolutions' wrought by recursion theory and non-standard methods - there is also the question of *how* he won.

To suggest a tentative answer to this question, let me 'fast-forward' forty years, to the trials and tribulations faced by Errett Bishop who re-constructed (sic!) large parts of classical mathematics, observing constructive discipline on the invalidity of the *tertium non datur* and non-admissibility of 'Hilbert's Dogma' in his classic and much acclaimed Foundations of Constructive Analysis (Bishop, 1967). Bishop, too, faced similar personal and professional obstacles to those that Brouwer and his followers faced -- although not to the same degree

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[40] Not dissimilar to the Turing Machine, at least in some senses.
[41] As perceptively observed by Posy (*ibid*, p. 292)
"From the start Hilbert and Brouwer -- Kantian constructivists both -- differed sharply about the foundations of mathematics. Brouwer was prepared to revise radically the content and methods of mathematics, while Hilbert's Program was designed constructively to secure and preserve all of 'classical' mathematics. Hilbert won."
Incidentally, Fraenkel's Lectures (Fraenkel, 1927) were delivered under the auspices of the ***Kant**-Gesellschaft, Ortsgruppe Kiel*.

and not from the kind of officially formidable adversary like Hilbert. Anil Nerode, George

Metakides and Robert Constable summarise the sadness with which Bishop, too, felt

'silenced', (Nerode, et.al., pp. 79-80):

> "After the publication of his book Constructive Analysis [in 1967], Bishop made a tour of the eastern universities.... . He told me then that he was trying to communicate his viewpoint directly to the mathematical community, rather than through the logicians. ... After the eastern tour was over, he said the trip may have been counterproductive. He felt that his mathematical audience were not taking the work seriously. ....
> After the lecture [at Cornell, during the 'tour' of the eastern universities] he mentioned tribulations in the reviewing process when he submitted the book for publication. He mentioned that one of the referee's reports said explicitly that it was a disservice to mathematics to contemplate publication of this book. He could not understand, and was hurt by such a lack of appreciation of his ideas. ....
> In the next dozen years his students and disciples had a hard time developing their careers. When they submitted papers developing parts of mathematics constructively, the classically minded referees would look at the theorems, and conclude that they already knew them. They were quite hesitant to accept constructive proofs of known classical results; whether or not constructive proofs were previously available. .....
> Nowadays, with the interest in computational mathematics, things might be different. Bishop said he ceased to take students because of these problems. ...
> When Bishop was invited to speak to the AMS Summer Institute on Recursion Theory, he replied that the aggravation caused by the lecture tour a decade earlier had contributed to a heart attack, and that he was not willing to take a chance on further aggravation."

What is it about the adherence to the *tertium non datur* and to 'Hilbert's Dogma' that makes a

whole profession so intolerant? But obviously it is not only here that intolerance resides.

Equally dogmatic, intolerant, voices were raised against Giuseppe Veronese's, admittedly

somewhat less 'rigorous' - at least in comparison with the works of Brouwer and Bishop --

pioneering work on the non-Archimedean continuum. In particular, Veronese's great Italian

contemporary, Peano, mercilessly - and as intolerantly as Hilbert was against Brouwer -

criticised and dismissed this work on the non-Archimedean continuum. Gordon Fisher, in his

masterly summary of '*Veronese's Non-Archimedean Linear Continuum*', (Fisher1994), while

acknowledging the 'tortured and ungrammatical style' of the writing of a massive book of no

less than 630 pages, (Veronese, 1891), noted that Peano's review of 1892 (Peano, 1892) was

'especially scathing' (Fisher, 1994, p. 127). Detlef Laugwitz, who did much to revive non-

standard analysis, described the 'open controversy that blazed up', in 1890, 'when Veronese

announced his use in geometry of infinitely large and small quantities', (Laugwitz, 2002, p.

102). When the German translation of the 1891 Italian edition appeared in 1894:

> "Cantor was doubly irritated. There was another approach to infinitely large integers; and, moreover, Veronese re-established the infinitely small which Cantor believed to have proved contradictory."
> ibid, pp. 102-3; italics added.

A massive two decade-long campaign against what has since become the eminently respectable field of non-standard analysis was launched by many of the mighty scholars of the foundations of mathematics: Cantor, of course; but also Peano and Russell.

§5. The *Brouwer Program* - Languageless, Logicless Mathematical Activity

> "Brouwer's intuitionism is closely related to his conception of *mathematics as a dynamic activity of the human intellect* rather than the discovery of an immutable abstract universe. This is a conception for which I have some sympathy and which, I believe, is acceptable to many mathematicians who are not intuitionists."
> Abraham Robinson (quoted in Dauben, op,cit., p. 461; italics added)

Brouwer's Program for an Intuitionistic Constructive Mathematics was underpinned, eventually, by:

- A variant of Kantian Intuition of Time and its 'dynamics', as perceived by the human intellect;
- The 'construction' of an *Ideal Mathematician*, implementing the dynamics of time as a *mental construction*;
- An intutionistically grounded, wholly constructive, *set theory* (in terms of *spreads* and *species*);
- *Choice* and *Lawless* Sequences;
- An intutionistic *continuum*;
- An abandonment of the *tertium non datur* in infinitary contexts;

A mathematics underpinned by these concepts and *mental constructions* did not require logic or language; mathematics was independent of, and prior to, logic and language (which had, to be sure, both mnemonic and communication value, as even emphasized by Brouwer's own enthusiastic involvement in the 'Significs' movement in Holland). Intuitionistic Constructive Mathematics, as conceived in Brouwer's Program, was an autonomous mental activity, which did not require, as in Hilbert's Program, any reliance on extraneous logical or linguistic props to bridge the gulf between finitary and non-finitary processes.

Where does computability theory fit in, within the Hilbert-Brouwer divide? Or, what is the difference between recursion theory (computability theory) and constructive mathematics

(especially of the Brouwer-Bishop variety): in the former the cardinal disciplining precept is the Church-Turing Thesis; this is not accepted in the Brouwer-Bishop variant of constructive mathematics. Why not? I think an answer can be found along the lines suggested by Troelstra (Troelstra, 1977, pp. 3-4; italics added):

> "Should we accept the intuitionistic form of Church's thesis, i.e., the statement
> '*Every lawlike function is recursive*'?
> There are two reasons for abstaining from the identification `*lawlike = recursive*':
> (i) An axiomatic reason: ... [A]ssuming recursiveness means carrying unnecessary information around. In the formal development, there are many possible interpretations for the range of the variables for *lawlike sequences* .... .
> (ii) A second reason is 'philosophical': the (known) informal justifications of 'Church's thesis' all go back to Turing's conceptual analysis (or proceed along similar lines).
> Turing's analysis strikes me as providing very convincing arguments for identifying 'mechanically computable' with 'recursive', but as to the identification of 'humanly computable' with 'recursive', extra assumptions are necessary which are certainly not obviously implicit in the *intuitionistic* (*languageless*) approach ..."

The path opened up by the foundational results of Gödel, Church, Turing and Post, made obsolete *Hilbert's Program*, without completely resolving the ambiguities surrounding '*Hilbert's Dogma*'. I suspect, in view of Gödel's epistemology and his metamathematical results, we will forever remain unable to resolve its status unambiguously - also because Brouwer and the Brouwerians, as well as non-Intuitionistic Constructivists like Bishop, refuse to compromise with *logic and language*.

The extent to which Hilbert was wedded to his mathematical ideology can be gauged from the fact that those who were close to Hilbert 'shielded' him from Gödel's remarkable results, presented at the very meeting where Hilbert had enunciated yet another of his paens to the Hilbert Program and to Hilbert's Dogma. He - Hilbert - came to hear of Gödel's Königsberg results 'only months later' and 'when he learnt about Gödel's work, he was angry' (van Dalen, 2005, p. 638), and van Dalen goes on, (*ibid*, p.639;second set of italics, added):

> "Gödel's incompleteness theorems brought the second ending of the *Grundlagenstreit*. Where Hilbert had won the conflict in the social sense, he had *lost it in the scientific sense*."

Why, if Hilbert 'lost it in the scientific sense' do we, minor purveyors of the *Hilbert Program* as Mathematical Economics (*pace* the Bourbakians!) continue the advocacy of a scientifically lost cause? Perhaps we, as mathematical economists are, after all, more enamoured of social approval than an adherence to a scientifically sanctioned norm!

§6. Towards an Intuitionistically Constructive Mathematical Economics

> "Where is the wisdom we have lost in knowledge?"
> T.S. Eliot: '*Rock*'

Purely by 'chance' I became aware, trying to familiarize myself with the rudiments of category theory, as part of my development of constructive foundations for computable economics, that the foundations of *smooth infinitesimal analysis* was to be found in a version of *First-Order Intuitionistic Logic* and, in particular, there was a 'natural' abandonment of the *tertium non datur* (cf. Bell, 2008, chapter 8).

Perhaps there is, here, the pedagogical bridge to introduce intuitionistic constructive mathematics to serious graduate students, so that they can take the baton for the next leg of the relay race in mathematical economics. Many in economics, even in the citadels of orthodoxy, indiscriminately compute everything in sight – replacing the old habit of linearising everything in sight. To them, it is possible to introduce computability theory, via numerical analysis; thereby, opening a small window of opportunity to teach the rudiments of category theory, which will make it possible to bring in, by some 'backdoor', a modicum of intuitionistic logic. This may well be the only way to make irrelevant the indiscriminate use of, and appeal to, the *tertium non datur*.

A 'victory' on this front, I think, is the best one can home for, within one or two generations of graduate education.

There is another way to interest the skeptical, able, young economist.

Consider the following. There are at least 31 propositions[42] in Debreu's *Theory of Value* (Debreu, 1959), not counting those in chapter 1, of which the most important[43] are theorems 5.7 (existence of equilibrium), 6.3 (the 'optimality' of an equilibrium) and 6.4[44] (the 'converse' of theorem 6.3). *None of these are algorithmic*, let alone *intuitionistically*

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[42] Some, but not all, of them are referred to as theorems; none of the 'propositions' in Sraffa (1960) are referred to as theorems, lemmas, or given any other formal, mathematical, label.
[43] I hope in saying this I am reflecting the general opinion of the mathematical economics community.
[44] Debreu refers to this as a 'deeper theorem', without suggesting in what sense it is 'deep'. Personally, I consider it a trivial - even an 'apologetic' - theorem.

constructive, and, hence, it is impossible to implement their proofs in a digital computer, even if it is an ideal one (i.e., a Turing Machine, for example). As a matter of fact *none* of the proofs of the 31 propositions are algorithmic (in either the Church-Turing based computability theoretic sense or in any of the constructive mathematics senses).

On the contrary, there are at least 22 propositions in Sraffa (1960), and all – except possibly one – are endowed with algorithmic[45] proofs (or hints on how the proofs can be implemented algorithmically). In this sense one can refer to the theory of production in this slim classic as an algorithmic theory of production. The book, and its propositions are rich in algorithmic – hence, numerical and computational – content[46].

Is this not the way to do analytical economics?

But there was left a nagging feeling, expressed – as I now see it, incorrectly – by Bishop (1967, p. 6), that *the continuum was a personal 'bugaboo' of Brouwer*. Moreover, given my own suspicion of any reliance on the *Axiom of Choice* in the mathematisation of economic theory, I was not sure of the relevance of the role the continuum could or should play in mathematical economics.

But suddenly – serendipitously – I came to realize that viewing the *Grundlagenstreit* as *Hilbert vs. Brouwer* was a misleading approach! I think it should be viewed, at least with hindsight, as a battle for the intellectual integrity of a program of research between different interpretations of Kantian intuitive foundations for epistemology. This vision opens up a pathway, at least in mathematical economics, for a reconciliation with phenomenology, of the Husserlian variety. I see this also as a serendipitous gateway towards an understanding of the ethics of economics, from the vantage point of Nagarjuna's Madhyamaka philosophy (of Buddhism).

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[45] For many years I referred to Sraffa's proofs as being constructive in the strict mathematical sense. I now think it is more useful to refer to them as algorithmic proofs in the intuitionistically constructive mathematical sense.

[46] Herbert Simon, together with Newell and Shaw (1957), in their work leading up to the monumental work on *Human Problem Solving* (Newell & Shaw, 1972), and Hao Wang (1960), in particular, automated most of the theorems in first ten chapters of *Principia Mathematica* (Whitehead & Russell, 1927). Surely, it is time one did the same with von Neumann-Morgenstern (1947)? I am confident that none of the theorems of this classic are proved constructively, in spite of occasional claims to the contrary. If I was younger – but, then, much younger – I would attempt this task myself!

I don't think Brouwer would be displeased with this line of interpretation!

My generation was educated from the wisdom and wit embodied in Samuelson's *Foundations of Economic Analysis*. Paradoxically, it was the first serious book in economic analysis I was exposed to, and I remain firmly attached to it in intellectual fondness, even while discarding its mathematical philosophy. Like the dangerous epitaph to Marshall's **Principles**, 'borrowed', indirectly, from Huxley[47], the one Samuelson borrowed from Willard Gibbs for *his* epitaph, set the tone and pace for the mathematisation of economics (see above, the discussion in the Introduction).

Somehow, I detect the source for this vision in Frege's masterpiece, **Begriffsschrift**, *a formula language, modeled upon that of arithmetic, for pure thought*. Brouwer's whole point is that 'pure thought' does not need – indeed is positively harmed by – a 'formula language', particularly one that seeks to found a 'concept script' for 'pure thought'.

A commitment to an *Intuitionistic Approach to Mathematical Economics* means an explicit rejection of this vision. It is always difficult to choose the path 'less travelled by' (*pace* Robert Frost)

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[47] *Natura non facit saltum.*

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