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TO THE BEAT OF A DIFFERENT DRUMMER[♣]....FREEDOM, ANARCHY AND CONFORMISM IN RESEARCH

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[♣] As in Thoreau's enchanting elegy to the independent thinker, in Walden. He reminded the `conformist' that `If a man does not keep pace with his companions', it may well be because `he hears the beat of a different drummer.'

[♦] As almost always in my idle speculations, I have been fortified and sustained in these endeavours by the enlightened wisdom of my friend and colleague, Stefano Zambelli. He is more wedded to the validity of the Church-Turing Thesis than I. I suspect this has to do with age and the increasing awareness of the fragility and fallibility of my mind. Unfortunately, I cannot blame him for any of the many remaining infelicities in this paper.

Abstract[1]

In this paper I attempt to make a case for promoting the courage of rebels within the citadels of orthodoxy in academic research environments. Wicksell in Macroeconomics, Brouwer in the Foundations of Mathematics,Turing in Computability Theory, Sraffa in the Theories of Value and Distribution are, in my own fields of research, paradigmatic examples of rebels, adventurers and non-conformists of the highest calibre in scientific research within University environments. In what sense, and how, can such rebels, adventurers and non-conformists be fostered in the current University research environment dominated by the cult of 'picking winners'? This is the motivational question lying behind the historical outlines of the work of Wicksell, Brouwer, Hilbert, Bishop, Veronese, Gödel, Turing and Sraffa that I describe in this paper. The debate between freedom in research and teaching and the naked imposition of 'correct' thinking, on potential dissenters of the mind, is of serious concern in this age of austerity of material facilities. It is a debate that has occupied some the finest minds working at the deepest levels of foundational issues in mathematics, metamathematics and economic theory. By making some of the issues explicit, I hope it is possible to encourage dissenters to remain courageous in the face of current dogmas.

Keywords: Non-conformist research, macroeconomics, foundations of mathematics, intuitionism, constructivism, formalism, '*Hilbert's Dogma*', *Hilbert's Program*, computability theory

[1]I aimed to finish the first draft of this paper on the *15th of October*, the *82nd anniversary of Hilbert's unfortunate letter to Einstein*, seeking support to dismiss Brouwer from the editorial board of **Mathematische Annalen** ([65], pp. 602-3). Hilbert failed to enlist Einstein's support. In any case, this is a very preliminary draft report of an ongoing research project on 'freedom in research and teaching in academic environments'. The aim is to study the evolution of the debates in the foundations of mathematics – metamathematics – and nonlinear dynamics in the past century and a quarter and try to learn lessons about the way these subjects have influenced the mathematization of economics.

1 'The permanent bottleneck of his highmindedness.....[2]'

> "You have not converted a man because you have silenced him."
> Viscount Morley, *On Compromise*, 1874.

Brouwer was *silenced* by Hilbert[3], but refused to be converted from *Intuitionism*; Bishop was silenced, but continued his courageous task of refounding much of classical mathematics on constructive grounds; Wicksell was repeatedly thwarted from a permanent academic post, but did not turn away from voicing his rebellious opinions on every available platform; 'to keep Senator McCarthy off his back', Paul Samuelson, no less, had to coin a pointless phrase, t*he neoclassical synthesis*, which took an orthodox life of its own to subvert the Keynesian revolution[4]; Sraffa's rigorous – yet elegant – prose was silenced by mindless mathematical economists, yet he was not converted, even though he remained (largely) silent in the face of repeated misrepresentations of his economics and his mathematics; Dirac's delta function was 'silenced' by von Neumann, in the name of mathematical rigour, yet did not succeed in preventing its ultimate success, exactly on the grounds of mathematical rigour; Veronese's valiant attempt to develop a non-Archimedean theory of the infinitesimal was silenced by his great contemporary, Giuseppe Peano, supporting Cantor and supported by Russell, yet – half-a-century later - it was the Veronese who was vindicated.

The examples can be multiplied in all sorts of ways.

In every case, orthodoxy and conformism triumphed - albeit in the short-run; the visionaries triumphed, eventually, mostly after their time, but not always. The hallmark of each example of orthodoxy's apostles ruthlessly silencing heretics was the unbending, unflinching, conviction with which the official heretics held their visions, and refused to be converted, even if their temporary silences – in the face of the triumphal noise made by the ostensible silencers – may have been construed as a conversion.

[2] The *last line* of Louis MacNeice's thoughtful, soft, tribute to the honest intellectual, *Bottleneck.* The poem begins unambiguously, by stating the credo of the honest intellectual: '*Never to fight unless from a pure motive, And for a clear end was his unwritten rule.*' The closing lines, inevitably are: '*For compromise with fact, longing to be combined, Into a working whole but cannot jostle through, The permanent bottleneck of his highmindedness.*'

[3] As noted by van Dalen, in his superbly fair and detailed outline of the 'The Crisis of the *Mathematische Annalen*', when Hilbert resorted to every possible means – both fair and foul – to remove Brouwer from its editorial board, [63], p. 31:

> "After the *Annalen* affair, little zest for the propagation of intuitionism was left in Brouwer; .. .. Actually, his whole mathematical activity became rather marginal for a prolonged period."

[4] In his unpublished '**Perugia Lectures**', Robert Clower confessed that he had taken it for granted for several years that there was some kind of analytical content in the neo-classical synthesis. "Oh, not at all", said Paul Samuelson, "don't you remember what was happening in those years?" And I said "no". "*Well, McCarthy was after me and I put in the neo-classical synthesis and suggested that it was just a matter of point of view in order to get him off my back*". And I said "You mean that you actually invented this term for those reasons?"

What does it take to hold on to a vision, against the teeth of every kind of subversion to which orthodoxy will resort to, not excluding personal attacks camouflaged in the veneer of professional pomposity? In the absence of institutional support, increasingly at a premium, when even the hallowed independence of Universities is being pawned at the altar of orthodox funding criteria, dominated by the cult of 'picking winners' on the basis of absurd and ahistorical criteria, the only way of sustaining an unorthodox vision is to have the courage to remain the 'unrewarded amateurish conscience' of the intellectual world, in the sense made wonderfully clear by Edward Said in his Fourth BBC-Sponsored *Reith Lecture* (**The Independent**, 15 July, 1993):

> "Every intellectual has an audience and a constituency. The issue is whether that audience is there to be satisfied, and hence a client to be kept happy, or whether it is there to be challenged, and hence stirred into outright opposition, or mobilised into greater democratic participation in the society. But in either case, there is no getting around the intellectuals relationship to them. How does the intellectual address authority: as a professional supplicant, or as its unrewarded, amateurish conscience?"

Brouwer and Bishop, Veronese and Levi-Civitta, Wicksell and Sraffa, Gödel and Turing, Dirac and Samuelson, had the courage to be '*authority's unrewarded, amateurish conscience*'. It is this that we need to make clear to the young, idealistic, enquiring, fresh minds that enter our Universities with hopes and expectations of unbiased education, intellectual adventure and a path towards the frontiers of research, without too many compromises to authority – of whatever form.

With these aims in mind the paper is structured as follows. In the next section, a kind of succinct statement of the credo I want to subscribe to, is outlined. In section 3, very briefly and, perhaps, somewhat romantically, I try to outline the broad aim of the paper through a very brief summary of an aspect of the special story of Knut Wicksell and the courageous way he pursued his focused research agenda, despite coordinated actions to prevent him from doing so – most blatantly by denying him access to any kind of secure University appointment, till very late in his life.

The paper's main focus, however, is to discuss, via, the way the a particular vision of the foundations and the practice of mathematics was systematically subverted on non-scientific grounds, the way an orthodoxy in any one epoch tried to act as censorious Commissars on what is right and what is proper in mathematical activity; but also to go beyond and do their utmost to banish anything that smacked of an alternative vision – usually by appealing to undefined notions of 'rigour', but not always. Every kind of pressure was brought to bear on alternative visions and to subvert them and make it impossible for the alternative visionaries to get a hearing via the ordinary channels of communication. This issue is discussed in section 4. The penultimate section is a simple story of the kind of unintended consequences of free thinking that could undermine even the most meticulously devised systems of foresight. The concluding section summarizes the lessons in the form of speculative reflections.

2 'Oh! What a Tangled Web We Weave ...[5]

"I sought a theme and sort for it in vain,

I sought it daily for six weeks or so.

......

What can I but enumerate old themes ...

William Butler Yeats: *The Circus Animals' Desertion*

Intellectual history is replete with claims of complete solutions, definitive codifications, unambiguous 'final' resolutions of paradoxes, almost all and every one of which have turned out to be illusory. I want to state three such examples, just to place the idea of eternal vigilance against this dogma of 'final solutions', but also to suggest that visionaries with conviction should persevere, even against the most formidable odds, particularly in intellectual contexts. Their time will come, perhaps too late for them to savour, but posterity has a way of resurrecting vintage ideas, rather like the way great wines mature with grace and evolve into silken tastes.

In the context of the issues treated in this paper the most significant example is the obituary of the 'paradoxes' of the infinitesimals, the infinite and the continuum, announced by no less an authority than Bertrand Russell, [14], pp. 1-2 (bold emphasis, added):

> "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, 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[6]."

[5].. *When first we practise to deceive*', in: **Marmion**, Canto VI, stanza 17, by Sir Walter Scott.

[6]In [47], p. 337 (italics added), Russell is equally merciless in dismissing any role for the infinitesimal in mathematics (not just in mathematical philosophy):

> "[W]e may, I think, conclude that these infinitesimals are *mathematical fictions*."

Now, a little over a century after Russell's initial obituaries, the infinitesimal, the infinite and the continuum are very much alive, well and even routinely applied in economics, too!

Theological excommunications, intolerant arrogance, are some phrases that come to mind, when reading these premature obituaries. Why do even advocates of liberal, tolerant, attitudes to public life and sociological attitudes become intolerant in the purely intellectual domain?

My two other examples refer to equally celebrated but, mercifully, even more immediately falsified prophetic pontifications by two almost saintly intellectual giants of the 19th century: Lord Kelvin and John Stuart Mill. The former is reputed to have suggested, on the eve of the works by Planck and Einstein that changed the intellectual map of the natural scientist as a physicist, that all the problems of physics had been solved : 'except for just two anomalies: that of the Michelson-Morley experiment, on the one hand, and Black Body radiation on the other'! The one led to the relativistic revolution; the other to the quantum intellectual cataclysms[7].

As for the great and saintly John Stuart Mill, in what can only be called an unfortunate moment of weakness, he etched for posterity these (in)-famously un-prophetic thoughts on the 'end of the theory of value', [33], Bk. III, Ch. I., p. 266; italics added:

> "*Happily, there is nothing in the laws of Value which remains for the present writer to clear up; the theory of the subject is complete*: the only difficulty to be overcome is that of so stating it as to solve by anticipation the chief perplexities which occur in applying it: and to do this, some minuteness of exposition, and considerable demands on the patience of the reader, are inevitable."

These words were coined on the eve of Marx's great and revolutionary works and not many years before the even more significant *marginal revolutions in value theory*.

Research and intellectual adventures can and must always be open-ended and the institutions that underpin open-ended research have, themselves, to be founded on structures with flexibilities – rather like the way Herbert Simon advocated organisations to be semi-decomposable so as to facilitate evolution. I cannot do better than to recall – and re-record – the kind of attitude to research that I grew up with, the kind of approach to research that was fostered by my own Cambridge maestro, Richard Goodwin, as reported, with first-hand experience, by one of his own most illustrious students, the *Nobel Laureate* Robert Solow, [56], pp. 32-3 (italics added) :

> "There was something more important, however. It is clear in my mind that when I asked what I must have thought were devilishly

Most of the frontier mathematical models in macroeconomic theory are based on variables defined on the continuum; or, it is claimed that the most rigorous way to model a competitive economy, with price taking behaviour, should be on the basis of non-standard analysis.

[7] The actual statement, made in an address to an assemblage of physicists at the **British Association for the advancement of Science** in 1900, seems to have been: "*There is nothing new to be discovered in physics now. All that remains is more and more precise measurement.*"

> clever or profound questions, Mr Goodwin did not take a high and mighty – and defensive – line. His answers made it plain that he had plenty of sceptical doubts of his own. I may be inventing this, but I seem to recall that he sometimes suggested that, well, one could not actually believe this or that, but it was an ingenious line of thought, perhaps worth following just to see where it came out. Once could always reject it later, and then one would have a better idea of what one was rejecting. If that actually happened, then I was getting my introduction to the theorist's frame of mind.
>
> ....
>
> ...I continued to learn from [Goodwin], both in the substance of economic theory .... and in a more subtle way that I do not know how to describe except as a matter of intellectual style. *The unspoken language was that if a thing is worth doing it is worth doing playfully. Do not misunderstand me: 'playful' does not mean 'frivolous' or 'unserious'. It means, rather, that one should follow a trail the way a puppy does, sniffing the ground, wagging one's tail, and barking a lot, because it smells interesting and it would be fun to see where it goes.*"

I am convinced that this spirit of enlightened 'playfulness' in research is being discouraged in the current intellectual environments of research institutions, plagued and harassed as they are with pressures to produce results that are measurable in the marketplace, *as if ideas can be produced without speculations, failures and traumas.*

Moreover, even while espousing the virtues of globalisation of the market for goods and services in the conventional sense, academic institutions – in particular – are increasingly parochial in the way they are administered, hoping to outline strategies to 'pick winners', nationally, and out-compete other nations in the so-called market-place for ideas and their immediate application for monetary rewards. Crass cost-benefit analysis, without the slightest understanding of the kind of assumptions required for such methods to make serious sense, motivates evaluations and ordinary promotions.

Moreover, the ahistorical notion that ideas can be generated and evolve in an environment simulating the competitive market model is at least doubly nonsensical: firstly, because the formalization of the competitive market model is seriously deficient in its mathematical underpinnings, especially with respect to numerical meaning in the presence of any kind of 'scale effects'; secondly, the implicit assumption that ideas that have been outcompeted are definitively hollowed-out. History does not provide any substantiation of this blinkered vision. The mania for claiming completeness of theories, thereby also banishing from discourse possible alternatives, permeates intellectual history in all its domains. That it is even significantly present in the purest recesses of mathematics and its foundations is somewhat more surprising than its presence in a subject infused with ideological over – and under – tones, such as economics.

3 Dante and Thorild at *The Gate of Silence*[8]

"And the preacher wept,
Because that he himself, being born of man,
Dreamed dreams,
And because the great dreams, the mighty visions, the noble fantasies,
The great illusions that had made man great,
Were now over-past, thrown down, ruined, fallen."
W. T. Stace, ([58]), *The Temptation in the Desert*, p. 31

At the graduate school of economics to which I am affiliated, in the department of economics at the University of Trento, graduate students – and young non-tenured researchers – are encouraged, even admonished, to pursue lines of research that do not deviate from orthodoxy, 'to fish only in well fished waters', as it is uncompromisingly, and repeatedly, stated. Second, there is the utterly deplorable preoccupation to evaluate the performance of colleagues in terms of criteria that deaden any adventures in intellectual exploration. This is implemented, in criteria for promotion, for example, by the accumulation of 'brownie points' for publication in officially 'highly rated' journals, as classed by one or another bibliometric criterion. These two criteria – to fish only in well-fished waters and to aim to publish only in officially sanctioned journals – encourage a culture of conservatism that makes it very difficult for young minds, even if they had intrinsic propensities to do so, to explore all but the well-trodden path of research. Eventually, fishing in well-fished waters will only lead to an exhaustion of supply and, then, one has to stock artificial lakes and cordoned off portions of the sea with fish, so that would-be fishermen and fisher-women are able to simulate the sensation of being natural versions of the once adventurous profession.

An example of particular relevance for the theme and content of this paper may highlight the problem. The article that initiated, and even provided the encapsulating name for, the *Grundlagenkrise* in mathematics, during the decade of the 1920s, was Hermann Weyl's classic: *Über die neue Grundlagenkrise der Mathematik* ([77]). This was not published in the leading Mathematical Journal – at least in Continental Europe – of the time, *Mathematische Annalen* (*MA*), in spite of the fact that Weyl was, at that time, still very close to Hilbert, the main editor of *MA.* Hesseling, in his admirably exhaustive study of the *Grundlagenkrise* conjectures, I think correctly, 'that Weyl *wanted to speak freely*' ([22], p. 132). Naturally, this conjecture, if correct, presupposes that Hilbert would have acted as a censoring Commissar, and not as an impartial editor,

[8]**The Gate of Silence** ([58]) is the title of a book of poems by W.T. Stace, written during his later Princeton years as a Professor Philosophy, but reflecting on his earlier years as a colonial civil servant in old Ceylon, where he was a dedicated scholar of Buddhist and Hindu philosophy. W.T. Stace was the undergraduate thesis supervisor of the distinguished philosopher of law and justice, John Rawls.

contrary to Felix Klein's original aims for the *Mathematische Annalen* to be an outlet for alternative views and visions of Mathematics and its foundations[9].

Essentially, 'Weyl wanted to speak *freely*', but may have feared that 'Hilbert would have wanted him to speak *correctly*', and chose – since he could - the former alternative. How many young researchers, in today's environment, are straitjacketed and frog-marched into 'speaking correctly', by being forced to collect brownie points for publishing in officially rated Journals, than thinking freely and expressing fresh and original thoughts, unencumbered by the shackles of orthodoxy's censorious Commissars, who hide behind the mantra of 'peer reviewing'?

The point I wish to make is no better illustrated than a seemingly amusing episode in the life of one of the great original thinkers in economic theory, Knut Wicksell. a gadfly to every purveyor of orthodoxy if ever there was one, although, by now, his work and personality have been diluted and distorted to such an extent that he is even considered a precursor of macroeconomic orthodoxy.

With great reluctance[10], he applied for exemptions from certain archaic regulations that prevented him from seeking teaching posts – as a Docent – in Faculties of Humanities; they were all rejected. He, then, applied for a similar post in the Law and Philosophy Faculties at the University of Uppsala, with predictable rejections. It was widely known that the rejections had more to do with Wicksell's 'politically incorrect' opinions – as we would now refer to them, perjoratively – and his way of expressing them, than with any scientific objections to his work (cf, [20], in particular, pp. 185-6)[11].

It is against this highly stylised summary of the background to the series of rejections the great Wicksell experienced, that was, nevertheless, symptomatic of official stances then – and even now, although stated and held with more apparent finesse – that one must try to interpret Gårdlund's genuine attempt to give content to the obduracy and shenanigans of orthodoxy against a fiercely independent scholar of impeccable integrity. He – Gårdlund – did so with admirable clarity, simplicity and brevity, by using a famous, though melancholy, cartoon, which was underpinned by a deep sense of the futility of free-thinking and the inevitability of the dominance of the deadening forces of conformism.

[9] In many and precisely documentable ways, it will not be an exaggeration to say that Weyl's unexpected conversion to a version of intuitionism and constructive mathematics – especially in his advocacy of impredicativism – set the stage for the initiation of the *Grundlagenkrise* of the 1920s. Even more than Brouwer's own fundamental contributions, it may have been Weyl's famous book on *Das Kontinuum* ([75]), and his two subsequent articles, [76] and [77], that set the tone and themes, at least in the first instance, for the *Grundlagenkrise*. If not anything else, at least the two phrases that became common currency in the debates, were coined by Weyl in the above book and articles: *Der circulus vitiosus* and *Grundlagenkrise*.

[10] Because it may imply 'compromises with enemies in the fight for truth and justice', [20], p.182, Wicksell was reluctant to apply for conventional or official positions.

[11] By this time, Wicksell was the author of two of the great classics of capital theory and public finance, respectively – and was well on the way to completing the defining work of Monetary Theory that set the stage for the emergence of a revolutionary *Macroeconomics* ([78], [79], [80] and [81]).

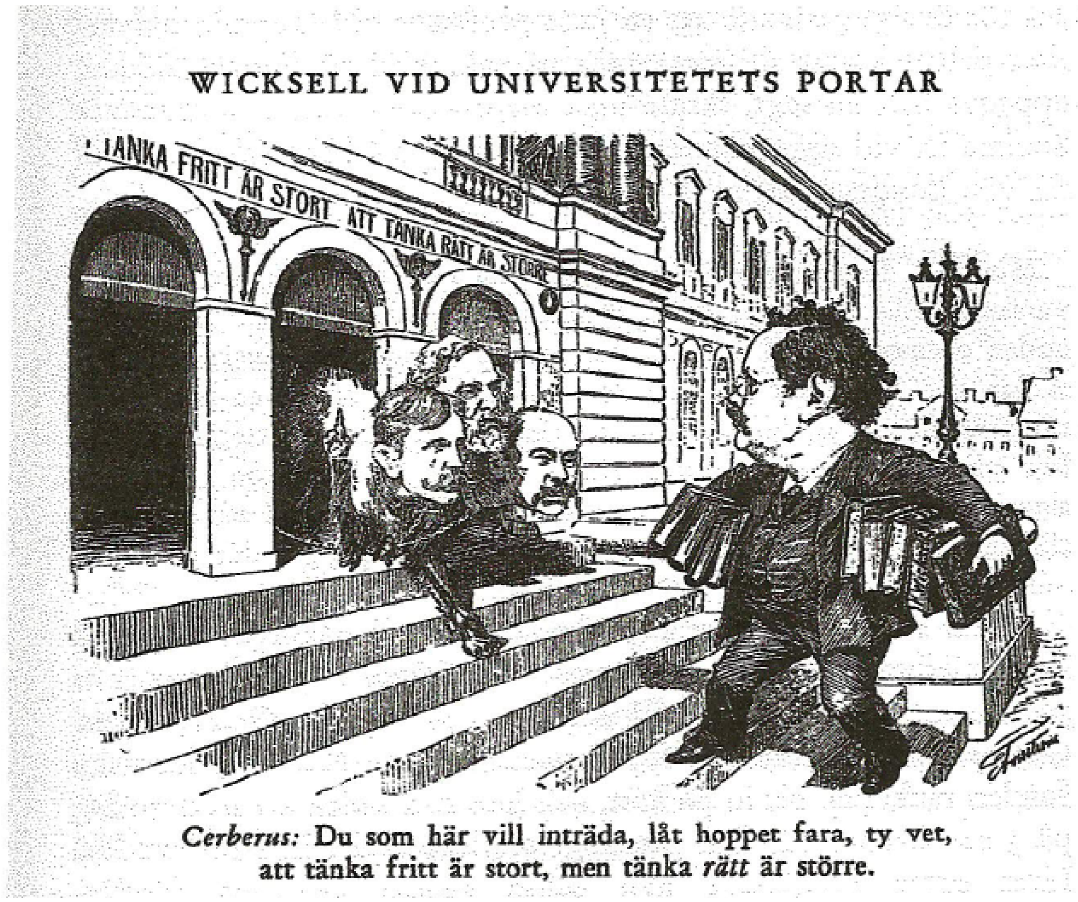

Figure 1: Wicksell at the Entrance to the University of Uppsala

In his magisterial biography of Knut Wicksell[12], Torsten Gårdlund reproduces the following sketch (see Fig. 1) by Edward Forsström, which had first appeared in the Swedish satirically humorous magazine Söndags-Nisse (1862-1924), in 1896:

The caption above, in Fig. 1, states: '*Wicksell at the Portal of the University*'. The caption below, in Fig. 1, has *Cerberus* admonishing Wicksell, on his appointment as a Docent in the Law Faculty of the University of Uppsala[13]: '*You, who will enter [through these gates], give up all hope, since thou*

[12] Subtitled **REBELL I DET NYA RIKET** ([20]), The '*Nya Riket*' is a reference to the culturally, socially, scientifically and politically emerging modernism that was enveloping Sweden, despite its continuing economic backwardness, of the late 19th and early 20th century.

[13] *Cereberus* is the three-headed hound, in Greek and Roman mythology, guarding the Gates of *Hades* from those who have crossed the river *Styx*, demarcating the bounds between the

Figure 2: Thomas Torild's 1794 Aphorism

shall know that freedom in thinking is important, but correct thinking is more important'. The phrase on the walls of the arched entrance, guarded by the caricatured three-headed Cereberus, is given in Fig. 2, as a photograph on the actual wall of the Aula at Uppsala University.

Obviously, Cereberus invokes Dante's famous admonishment in the **Inferno**: '*Lasciate ogni speranza , voi ch'entrate*', on the one hand, and the tragic – but noble – words of Thomas Thorild[14], a courageous poet, philosopher and an advocate of press freedom and gender equality. He was subsequently exiled for these punishable beliefs, when expressed in public, in 1793. He never returned to mainland Sweden, after the being exiled, ending his days in Greifswald, then part of Swedish Pomerania. Wicksell did not bend, did not give up hope, did not compromise with his passion for truth and for trying to reach it – whatever he may have conceived it to be – with a sense of freedom of thought, and he persisted against all odds and eventually triumphed, but mostly after his lifetime. Sadly, these aspects of his struggles to forge a macroeconomic theory, challenging orthodox monetary theory and its sanguine methodology of stable equilibria, have been submerged to the dusty heaps of works by arcane scholars

Earth and the underworld. My own background in Hindu mythology enables me to recall *Yaman* – the god of death – and his two guard dogs, *Sarvara*, sometimes referred to as providing the origins of the word *Cereberus*, or as in *Hesiod*, $\kappa\acute{\varepsilon}\rho\beta\varepsilon\rho o\varsigma$ (*Kerberos*).

[14] The Thorild aphorism, in the original Swedish is:

> "Tänka Fritt är Stort
> Men Tänka Rätt är Större."

My (free) translation would be:

> "Freedom in thinking is important;
> But correct thinking is more important."

of the history of economic thought. They should be part of the study of the sociology of academic politics, so that every generation renews itself and inspires itself by a knowledge of the nobility of Wicksell's integrity.

4 Brouwer (and Bishop) – Towards the *Grundlagenkrise* (and its Perennial Resurrections)

> "It may be remarked here that Hilbert was too pessimistic about a *Tertium non datur*-free mathematics. Work in the *intuitionistic school* and above all the results of the school of Errett Bishop gave a powerful impetus to *constructive mathematics* by actually rebuilding large parts of analysis in a constructive manner."
>
> Dirk van Dalen, [65], p. 576; italics added.

There have been many foundational crises in mathematics, but the one we refer to here as the *Grundlagenkrise* is that which was associated almost exclusively with the debate surrounding the positions taken by the two protagonists for two foundational views on Mathematics: Hilbert and Brouwer, and which blossomed and then wilted in acrimony of the most unexpectedly personal sort, during the whole of the 1920s, reaching a kind of climax in 1928. As mentioned above, it may have said to have crystallized and been initiated by the explicit stance taken by Weyl, and stated clearly in his three foundational works between 1918 – 1921 (cf. [75], [76] and [77]). Weyl's stance was somewhere in between the pure intuitionism of Brouwer and the finitist formalism of Hilbert, although much closer in philosophical adherence to the former than the latter, despite the fact that he was one of the latter's outstanding direct pupils. Weyl's intuitionism was closer in spirit to Poincaré's impredicativism, later taken up with great finesse by Solomon Feferman ([17]).

In subsection 2, below, I try to outline the main issues that characterised the formal issues in the *Grundlagenkrise*. Here I want to place on record, in the context of the aims and themes of this paper – freedom from dogma and coercion in research thinking and activity, in the academic world – two examples of intolerance, that continue to bedevil research of the most fundamental sort. If even pure foundational research is subject to such dogmatic pressures of intolerance what hope is there for applied subjects that are closer to social and political sensitivities?

Both Brouwer and Bishop, separated by forty years between the beginning of the end of the *Grundlagenkrise* in October 1928 and the publication of Bishop's classic *Foundations of Constructive Analysis*, in 1967 ([2]) suffered remarkably similar fates: the orthodox mathematician's indiscriminate victimization of alternative visions of the foundations of mathematics. This was partly due to the way the mathematicians misunderstood – or simply were ignorant of – the way Brouwer and Bishop tried to develop an intuitive mathematics, entirely consistent with the practice of the applied mathematician, without any reliance on, or appeal to, mathematical logic. Their's was a fate and a drama that was reenacting that which was played at the turn of the 19th century, into the 20th, between Cantor and Veronese, with Peano firmly on Cantor's side, on the way infinitesimals were to be considered in the foundations of the real number system and on non-Archimedean systems, in general. Ostensibly, Cantor won the intellectual battle, but only 'temporarily'; Veronese was vindicated,but only more than half-a-century later, after Abraham Robinson, Detlef Laugwitz, Paul Lorenzen, Jerome Keisler and others rejuvenated research into non-standard analysis in a systematic way to place infinitesimals on firm logical foundations.

4.1 Hilbert's Dogma, – 'consistency $\Leftrightarrow$ existence' – Becomes the Mathematical Economist's Credo

> "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 by Heyting and Brouwer in Benacerraf and Putnam (1983)."
>
> Efe. A. Ok ([39], p. 279; italics added.

Unfortunately, the beginning of the end of the *Grundlagenkrise* coincided almost exactly with the re-birth of mathematical economics, in a precise, and precisely datable, sense. The von Neumann paper of 1928 ([73]), introduced, and etched indelibly, to an unsuspecting and essentially non-existent Mathematical Economics community and tradition what has eventually come to be called '*Hilbert's Dogma*'[15], 'consistency $\Leftrightarrow$ existence'. This became – and largely remains – the mathematical economist's *credo*. Hence, too, the inevitable schizophrenia of 'proving' existence of equilibria, first, and looking for methods to construct them at a second, entirely unconnected, stage. Thus, too, the indiscriminate appeals to the *tertium non datur* – and its implications – in 'existence proofs', on the one hand, and the ignorance about the nature and foundations of constructive mathematics, on the other.

[15] In van Dalen's measured, studied, scholarly, opinion, [65], pp. 576-7 (italics added):

> "Since Hilbert's yardstick was calibrated by the continuum hypothesis, *Hilbert's dogma, 'consistency $\Leftrightarrow$ existence'*, and the like, he was by definition right. *But if one is willing to allow other yardsticks*, no less significant, but based on alternative principles, then Brouwer's work could not be written off as obsolete nineteenth century stuff."

But it was not as if von Neumann was not aware of Brouwer's opposition to 'Hilbert's Dogma', even at that early stage, although there is reason to suspect – given the kind of theme I am trying to develop in this paper – that something peculiarly 'subversive' was going on. Hugo Steinhaus observed, with considerable perplexity, [59]:

> "[My] inability [to prove the minimax theorem] was a consequence of the ignorance of Zermelo's paper in spite of its having been published in 1913. .... J von Neumann was aware of the importance of the minimax principle [in [73]]; it is, however, *difficult to understand the absence of a quotation of Zermelo's lecture in his publications*."
>
> ibid, p. 460; italics added

Why didn't von Neumann refer, in 1928, to the Zermelo-tradition of (*alternating*) *games*? van Dalen, in his comprehensive, eminently readable, scrupulously fair and technically and conceptually thoroughly competent biography of Brouwer, [65], p. 636, noted (italics added), without additional comment that[16]:

> "In 1929 there was another publication in the intuitionistic tradition: an intuitionistic analysis of the game of chess by Max Euwe[17]. It was a paper in which the game was viewed as a spread (i.e., a tree with the various positions as nodes). Euwe carried out *precise constructive estimates* of various classes of games, and considered the influence of the rules for draws. When he wrote his paper he was not aware of the earlier literature of Zermelo and Dénès König. *Von Neumann called his attention to these papers, and in a letter to Browuer von Neumann sketched a classical approach to the mathematics of chess, pointing out that it could easily be constructivized*."

Why didn't von Neumann provide this 'easily constructivized' approach – then, or later? Perhaps it was easier to derive propositions appealing to the *tertium non datur,* and to 'Hilbert's Dogma', than to do the hard work of *constructing estimates* of an algorithmic solution*, as* Euwe did? Perhaps it was easier to continue using the axiom of choice that to construct new axioms – say

[16] At the end of his paper Euwe reports that von Neumann brought to his attention the works by Zermelo and Konig, after he had completed his own work (ibid, p. 641). Euwe then goes on (italics added):

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

[17] In a strange lapse, van Dalen refers to Euwe, 1929, without giving the exact details of the reference in his excellent bibliography! The exact reference is [15]. Max Euwe was the fifth World Chess Champion, between 1935-1937, having defeated Alexander Alekhine, on December 15, 1935.

the *axiom of determinacy*[18] – as Steinhaus and Mycielski did ([34])? Whatever the reason, the fact remains that the von Neumann legacy was indisputably a legitimization of 'Hilbert's Dogma' and the indiscriminate use of the axiom of choice in mathematical economics.

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

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. [4][19], [5] and [6]); for the record, even the fixed-point theorem came earlier than 1912. It is nonsensical to claim that Brouwer did not consider '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 ([7]). On that occasion he identified the reason why his original theorem was unacceptable in intuitionistic constructive – indeed, in almost any kind of constructive – mathematics, for example, in Bishop-style constructivism, which was developed without any reliance on a philosophy of intuitionism:

[18]For the aims of this paper, the introduction of this axiom is particularly relevant. The point I wish to make is best described in Gaisi Takeuti's important observation ([60], pp. 73-4; italics added):

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

[19]Brouwer could not have been clearer on this point, when he wrote, in his 1907 Thesis (*ibid*:, p. 45; bold emphasis, added):

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

"[T]he validity of the Bolzano-Weierstrass theorem [in intuitionism] would make the classical and the intuitionist form of fixed-point theorems equivalent." ([7], p.1).

Note how Brouwer refers to a 'classical ... form of the fixed-point theorem'. The invalidity of the Bolzano-Weierstrass theorem[20] in any form of constructivism is due to its reliance on the *law of the excluded middle*[21] *in an infinitary context* of choices (cf. also [13], pp. 10-12). The part that invokes the Bolzano-Weierstrass theorem entails *undecidable disjunctions* and as long as any proof invokes this property, it will remain *unconstructifiable*.

It is worse than nonsense – if such a thing is conceivable – to state that 'an intuitionist would not accept the existence of an irrational number'. Moreover, the law of the excluded middle is *not* a *mathematical* axiom; it is a *logical* law, accepted even by the intuitionists so long as meaningless – precisely defined – infinities are not being considered as alternatives from which to 'choose'[22]. 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* (cf. [8]), there is the uncompromising requirement for constructive mathematics to be independent of 'theoretical logic' and to be 'languageless':

"FIRST ACT OF INTUITIONISM *Completely separating mathematics from mathematical language and hence from* the phenomena of language described by *theoretical logic*, recognizing that intuitionistic mathematics is an essentially languageless activity of the mind having its origin in the perception of a move of time."
ibid, p.4; italics added.

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, [2], p.9:

[20] For the absolute novice, I state here the simplest possible statement of this theorem:

Bolzano-Weierstrass Theorem: Every bounded sequence contains a convergent subsequence

[21] *Tertium Non Datur.*

[22] Even as early as in 1908, we find Brouwer dealing with this issue with exceptional clarity (cf., [6], 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."

> "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*[23]. Applications of the axiom of choice in classical mathematics either are irrelevant or are combined with a sweeping appeal to the principle of omniscience[24]. 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 choose the phrase 'even worse' most deliberately. The above inanities, 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. It is 'even worse' in the citadel of mathematical economic theory for the following reason: what is called *computable general equilibrium theory* (CGE) forms the foundational core of one frontier of macroeconomic theory: *Recursive Competitive Equilibrium* (RCE) which, in turn, forms the basis for the *Stochastic Dynamic General Equilibrium* (SDGE) model. The claim in these parts of mathematical economics is that CGE is computable – as is evident even from the appellation 'computable' in CGE – because it is constructive (in the sense of Brouwer). A representative example of such a claim, made by two distinguished applied general equilibrium theorists, is the following:

> "The major result of postwar mathematical general equilibrium theory has been to demonstrate the existence of such an equilibrium by showing the applicability of mathematical fixed point theorems to economic models. ... Since applying general equilibrium models to policy issues involves computing equilibria, these fixed point theorems are important: It is essential to know that an equilibrium exists for a given model *before* attempting to compute that equilibrium. .....
>
> ...
>
> The weakness of such applications is twofold. First, they provide *non-constructive rather than constructive proofs of the existence of equilibrium*; that is, they show that equilibria exist but do not provide techniques by which equilibria can actually be determined. Second, existence per se has no policy significance. .... Thus, fixed point

[23] See, also, [3], p. 13, 'Notes'.

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

> theorems are only relevant in testing the logical consistency of models prior to the models' use in comparative static policy analysis; such theorems do not provide insights as to how economic behavior will actually change when policies change. *They can only be employed in this way if they can be made constructive* (i.e., be used to find actual equilibria). *The extension of the Brouwer and Kakutani fixed point theorems in this direction is what underlies the work of Scarf .... on fixed point algorithms ....*"
>
> [51], pp12, 20-1; italics added

But this claim is false, as I have shown rigorously in a series of articles in respectable applied mathematical Journals (cf., [69], [70]). And it is 'even worse' because these claims are made in the context of policy models and are used to justify the derivations of policy propositions, with the accompanying claims that they are computationally feasible with any prespecified numerical accuracy. Even in respectable graduate game theoretic textbooks ([40]), there are claims about constructible algorithms and constructive proofs that are blatantly false.

A perceptive reader would also notice the schizophrenia exhibited between 'proving existence' and 'computing it' – i.e., separating the existence problem from that of a construction. Thus, without batting an eyelid, these two advocates of the schizophrenia could state that '*it is essential to know that an equilibrium exists .... before attempting to compute that equilibrium.*' It never seems to have occurred to them that this separation is precisely the one that is avoided in constructive mathematics.

Why does orthodoxy get away with such impunity? Why are obvious falsehoods allowed to persist and perpetuate themselves, quite apart from distorting alternative methodologies, especially mathematically rigorous ones?

Before I try to forge conjectural answers for these queries, I would like to return to Brouwer and Bishop – but also to Richard von Mises and his valiant efforts to define, rigorously, a notion of probability – and the way various orthodoxies subverted, often by foul means and disgraceful methods, their noble efforts to challenge the foundations of classical mathematics (and probability theory) on the basis of impeccably rigorous philosophical, epistemological and, above all, metamathematical, grounds.

4.2 The *Grundlagenkrise – Then, Now & Always*

> "Hilbert's program .... was driven by dual beliefs. On the one had, 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'[25]"

Carl Posy, [45], pp. 294-5; italics added

Summarising the tortuous personal and professional relationship between Brouwer and Fraenkel, van Dalen ([64], 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' – ([19], p. 154). Although 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[26], 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"

[25] The exact quote is as follows, [24], (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: [82], (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." '

[26] I am slightly unsure about the exact date, for which I am relying on [65]. 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!

This letter[27], written at the tail end of the *Grundlagenkrise*, marked the beginning of the end of it, and silenced Brouwer[28] 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.

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 t*ertium non datur* in infinitary mathematical reasoning;
- The problem of *Hilbert's Dogma* - i.e., 'existence $\Leftrightarrow$ 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*, whose time seems to have come only in recent years;

Carl Posy, reflecting on '*Brouwer versus Hilbert: 1907 – 1928*' ([45]), from a Kantian point of view[29] – both Brouwer and Hilbert had been deeply influenced

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[27] 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.

[28] In van Dalen's poignant description, the once effervescent, immensely productive, and active Brouwer ([65], 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 wise aphorism, quoted above: '*You have not converted a man because you have silenced him.*'

[29]

> "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.

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 (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* ([2]). Bishop, too, faced similar personal and professional obstacles to those that Brouwer and his followers faced – although not to the same degree 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', [36], 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. ....

Hilbert won."

ibid, p. 292.

Incidentally, Fraenkel's Lectures ([19]) were delivered under the auspices of the *Kant-Gesellschaft, Ortsgruppe Kiel*.

> *In the next dozen years his students and diciples 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 Veronse'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*', [18], while acknowledging the 'tortured and ungrammatical style' of the writing (of a massive book of no less than 630mpages, [67]), noted that Peano's review of 1892 ([43]) was 'especially scathing' ([18], 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', ([31], 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, as mentioned above, also Peano and Russell.

Finally, in this *genre* of intolerant pontifications – that is the only way I can now describe these so-called foundational criticisms – there is also a sad place to be accorded to the systematic dismissal of Richard von Mises's valiant attempts to axiomatise the foundations of probability on frequency theoretic grounds using his highly innovative idea of a *place selection function* to define what he called a 'Kollektive'. *A galaxy of 'eminent'* mathematicians, led by people like Fréchet and Knopp[30], met in Geneva, in 1937, [66], and dismissed off hand the von Mises theory, especially in the light of Kolmogorov's measure-theoretic axomatization of probability ([29]). Ironically, von Mises was strongly influenced by Brouwer's development of *choice sequences* in providing content for the *intuitive continuum*, when he came to try to formalise the idea of 'lawlike selections'.

It is a particular irony of history that the very same Kolmogorov - together with Martin-Löf, Chaitin and Solomonof – revived to a splendid research frontier the idea of algorithmic probability and, in that process, also resurrected to a new vigour and life the frequency approach to the foundations of probability ([30]. But this is a story that became possible only after computability theory came into being – as a result of the death-knell struck on *Hilbert's Program*, by Gödel, Church, Turing and Post. Hilbert may have won a battle 'politically, mathematically and polemically'; but he lost his soul – philosophically and epistemologically.

It is a sad commentary on the *Grundlagenkrise* to realise that:

> "It is very likely that Hilbert never read Brouwer's basic papers ... . All of Hilbert's attacks at Brouwer consisted of rather superficial comments on hearsay bits of Brouwer's repertoire. Brouwer, on the other hand, repeatedly put his finger on the crucial spots of Hilbert's programme; (1) consistency of induction requires induction .... , (2) consistency does not prove existence."
>
> [65], p. 637.

Much the same can be said of the experiences faced by Bishop and von Mises.

5 Gödel and Turing – Beyond the *Grundlagenkrise* and Towards Computability Theory

> "It was typical of [Turing] ... to seek to outdo Bell Telephone Laboratories *with his single brain*, and to build a better system *with his own hands*. ... Turing's wording indicates authoritative judgement, and not the submitting of a proposal for the approval of superiors. ..... As Newman ([37]) put it, Turing was 'at heart more an applied than a pure mathematician'. It might be more true to say that Turing had resisted this Cambridge classification from the outset. *He attacked every kind of problem* – from arguing with Wittgenstein, to the characteristics of electronic components, to the petals of a daisy. He did so on the basis of immense confidence in the power of mathematical analysis, in whatever field he chose."
>
> Andrew Hodges, [26], p. 4; italics added

[30] The latter also played a part on Hilbert's side, against Brouwer in the *Grundlagenkrise*.

In 1928 the first International Congress of Mathematicians since World War I – since, in fact, 1912 – was held in the Italian city of Bologna. Since that tragic war[31] German mathematicians had not been invited for international meetings. Italians, in those heady Fascist – and, yet, pre-Nazi days – were determined to make the occasion in Bologna truly international and invited the Germans. Despite opposition by some leading mathematicians in Germany, such as Bierbach (and by Brouwer), David Hilbert, led a delegation of 67 German mathematicians to the Bologna congress.

In 1925 and 1927 Hilbert had begun to crystallise his program for the foundations of mathematics in a system which came to be called *Formalism*, in contrast to, and in response to, Brouwer's sustained development of Intuitionism as an alternative foundation for pure mathematics[32]

Partly as a result of the so-called antinomies of set theory - one of the most celebrated of which was Russell's paradox of the 'set of all sets that do not contain themselves as members' – mathematicians at the turn of the 19th century to the 20th had begun to be more circumspect of arbitrary definitions and untrammelled methods of *proof.* Hilbert, notwithstanding the known antinomies and the dangers of unconstrained methods of proof, particularly in proving the existence of a mathematical object as a consequence of not being able to derive a contradiction in the defining criteria i.e., '*Hilbert's Dogma*'. had *seemed* to promote the idea of mathematical formalism as a symbol manipulation game, with its own rules without any discipline on the nature, contents and structure of thought. *This is the popular view, although it is largely inaccurate.*

Brouwer, at a kind of polar opposite end was convinced, in developing the foundations of mathematics on the basis of intuitionism, that mathematical objects were the autonomous creations of the human mind, and endeavoured to discipline the allowable techniques of demonstrating the existence of mathematical objects and their definitions in ways that respected the architecture, philosophy and epistemology of the mind. In this sense there was a direct link to what came to emerge as recursion theory, but that is not a story I can expand upon at this point.

The demonstration of the existence of a mathematical object - say even an abstract one such as the equilibrium price configuration of an economy, the prices at which market supply equals market demand – should be accomplished by constructive methods of proof; i.e., methods that could, in principle, be used by an 'engineer' actually to construct such an object with ruler, compass, chisel,

[31] Incidentally, it was the trauma generated by the meaningless slaughter of a whole generation of mathematicians during that tragic war that led to the philosophy underlying the formation of the influential French group of mathematicians who called themselves the 'Bourbakians'. Their influence had far reaching consequences even for mathematical economics and the education of economists at some of the best graduate schools of economics, all over the World, as a result of the influence of Gerard Debreu, who was himself deeply influenced by the Bourbakist's vision of mathematical methodology and structure.

[32] *Logicism*, the third of the tiresome trilogy, was a foundational system that was essentially the outcome of the message of the monumental program to reduce mathematics to logic that was represented in the great three-volume work by Russell and Whitehead. Brouwer, in contrast, was determined, via Intuitionism, to free mathematics from logic (and language).

lathe and so on. Thus, to say that a mathematical object exists if the decimal representation of π, say, contains a particular sequence of 9's at a particular place in the expansion, is to say nothing. Thus, for the formalist mathematician to claim that even if s/he does not know whether such a statement is true of the object π, *God will know*, is an equally vacuous assertion.. This kind of metaphysical answer would bring forth the retorts from Brouwer that he *did not have a pipeline to God* and if God had mathematics to do, he can do it himself; man's mathematics was not necessarily that of God's. In other words, Brouwer and the Intuitionists would restrict the allowable methods of proof for mathematicians to those that did not appeal to untrammelled infinities, undecidable disjunctions and so on – almost banning magic and metaphysics from mathematical practice. Strange, then, that Brouwer himself was accused of 'psychologism' for his belief in the autonomy of the mind and the constructions of the mind of an ideal mathematician, especially in the context of his work on choice sequences to provide foundations for the intuitive continuum.

To these Brouwerian objections and constructions, Hilbert (would) reply: 'With your [Brouwer's] methods, most of the results of modern mathematics would have to be abandoned, and to me the important thing is not to get fewer results but to get more results.' But why? And at what cost?

By the time of the Bologna meetings of the International Congress of Mathematicians, Hilbert had given two lectures[33] building towards a final crystallization of his position, such that when formulated as challenges to mathematicians in the form of well-posed problems, and answers given, **debate would forever be silenced** and mathematicians would be allowed to go on with their normal activities, untrammelled by any kind of constraints by a thought-police of any sort, however enlightened in method, epistemology or philosophy. Hilbert had stated his credo, not only by his outstanding mathematical works as examples of the philosophy he was advocating – as, indeed, was the case with Brouwer – but also by explicitly stating in his influential address to the Paris *International Congress of Mathematicians* in August, 1900, titled famously and simply: *Mathematical Problems* ([23], p.444, 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. .....
>
> 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

[33] The first, titled: *On the Infinite*, was delivered in Münster on 4 June, 1925 at a meeting organised by the Westphalen Mathematical Society to honour the memory of Karl Weierstrass, the quintessential formalist, The second was titled: *The Foundations of Mathematics* and delivered in July 1927 at the Hamburg Mathematical Seminar.

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*."

Even as far back as 1900, in that same famous lecture, Hilbert had also stated[34], 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.

These were the methodological and epistemological backdrops against which, in Bologna in 1928, Hilbert threw down the gauntlet to his foundational detractors, in the clear conviction that the answers to the questions he was posing would be forthcoming – surely, also, to substantiate his own philosophy of mathematics:

- Is mathematics *complete* – in the sense that every mathematical statement could be rigorously – rigour interpreted in the above finitary sense – proved or disproved;
- Is mathematics *consistent* – in the sense that it should not be possible to derive, by valid proof procedures, again in the sense of finitary rigorous proof stated above, universally false mathematical statements within a formal mathematical system;
- Is mathematics *decidable* – in the sense of using a definite finitary method, it was possible to demonstrate the truth – or falsity, as the case may be – of a mathematical assertion.

On 8 September 1930 Hilbert gave the opening address to the German Society of Scientists and Physicians, in Königsberg[35], titled: *Naturkennen und Logik*. This lecture ended famously echoing those feelings and beliefs he had expressed in Paris, thirty years earlier, [12], p. 71 (italics added):

[34] 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*.

[35] Where he was also honoured, in those enlightened pre-Nazi days, by being presented, by the Königsberg Town Council, with an 'honorary citizenship'.

> "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."[36]

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 mathematics was inconsistent[37]. Two of the pillars on which Hilbert was hoping to justify formalism had been shattered.

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.* In one of the celebrated confluences and simultaneous discoveries that the history of science and mathematics seems to be littered with, Alan Turing and Alonzo Church came up with definitions that, *ex post*, came to be accepted by mathematicians, logicians, etc., as encapsulating the *intuitive notion* of definite finitary method, now routinely referred to as 'algorithms'.

Once this was done, the unadulterated genius of Alan Turing devised, entirely with the aim of answering the question of *decidability* posed by Hilbert, the now celebrated Turing Machine, ([62]).

Thus came to an end Hilbert's pyrrhic victory over Brouwer; thus will come to an end the sustained hostility to Bishop's constructivism - whilst Veronese has already been copiously vindicated, although many generations after his own lifetime.

The development of computability theory is, in a strong sense, an outgrowth of the *Grundlagenkrise*. In many ways it stands, as an epistemology and a mathematical philosophy, midway between pure Intuitionistic Constructivism and Hilbert's kind of formalism. For example, the *tertium non datur* is freely invoked in recursion theory. Hence it is quite possible to prove the existence of algorithms to solve well-posed mathematical problems with almost no hope of

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[36] 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."

[37] 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.**

ever constructing them for implementation – or, at least, not knowing whether it can or cannot be done: i.e., undecidable.

Above all, there is one basic 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 ([61], pp. 3-4):

> "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 identitification '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 'shilded' 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.'[38]

[38] As reported in [65], p. 638, and van Dalen goes on, p.639 (ibid):

> "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."

In an even greater twist of fate – or what may felicitously be refferred to as a noble unintended consequence of dogma – Veronese was resurrected (implicitly) by an invoking of Gödel's incompleteness results:

> "For a long time the incompleteness of axiomatic systems was regarded by mathematicians as unfortunate. It was the genius of Abraham Robinson, in the early sixties, to turn it to good use and show that thnks to it a vast simplification of mathematical reasoning can be achieved."
>
> Nelson ([35]), p. 15

The icing on this twisted cake was the award of the second *Brouwer Medal*, in March, 1973, to Abraham Robinson![39]

Should we not applaud these wonderful unintended consequences of the cracks in dogmas?

I would like to end this section with a counterfactual thought: suppose Hilbert had not 'thrown down the gauntlet' and challenged mathematicians and mathematical philosophers to resolve, by finitary means, the triptych of completeness, consistency and decidability, would the genius of a Gödel, the innocent brilliance of a Turing, or the deep speculations of a Church have concentrated on the extraordinary work that led to the emergence of recursion theory? Connoisseurs of the foundations of mathematics may, of course, be able to say that Post's work in his doctoral dissertation ([44]) and Skolem ([54]) would, in good time, have been (re-)discovered and the mathematical foundations of computer science could have been erected on similar foundations. Others, like myself, like to think that a recursion theory more finessed and attuned to the strictures of constructive mathematics may have become the foundations of computer science. Either way, eventually, Hilbert's victory – at least in some senses – proved to be, and would have proven to be, pyrrhic.

6 Harvesting the Lessons, Weaving a Pattern

> "In the end we search out the beginnings. ...
>
> Genius will out, but how and why and what serves to nurture it? What consonance is there with the personality, what determines the particular channels taken by the intellect and the distinctive character of what is achieved?"

[39] In his *Brouwer Lecture*, on the occasion he received the *Brouwer Medal*, he paid handsome tribute to Brouwer, Intuitionism and the key difference between invention and discovery in mathematics, [11], p. 461:

> "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."

Solomon Feferman, [16], p. 2.

In economics we expect self appointed Commissars of varieties of ideologies to act as gate keepers, censoring or approving access to the gates of plenty, at the expense of visions and freedom of thought. It is not seldom we hear the phrase self-censorship in departments of economics aspiring to climb the rungs of official reputation, as measured by counters of orthodox bibliometric criteria. Graduate students are nurtured, implicitly and explicitly, on the nature of research that would mean anything for promotion, funding and research facilities.

That such a state of affairs has persisited in the purest recesses of mathematics – at its deepest levels of foundational research – came as a complete surprise to me. I embarked on trying to understand the status of proof in mathematical economics and the role of computation in applied economics and emerged with perplexities beyond explicabilities, initially. But with hindsight, and reflections on a particular episode in economic theory, it became possible for me to interpret the events I have tried to describe, however briefly, above.

Piero Sraffa's elegant, terse, *Production of Commodities by Means of Commodities,* ([57]; henceforth, *PCC*), has, in its 50th anniversary year of publication, reached the status of a classic: viz, often quoted, rarely read (although in this case it may also be the case that it is even more rarely read)[40]. *From a purely mathematical point of view*, *PCC lacks nothing.* The concerns in *PCC* are the *solvability of equation systems* and, whenever existence or uniqueness proofs are considered, they are either spelled out in completeness, albeit from a *non-formal*, *non-classical*, point of view or detailed hints are given, usually in the form of examples, to complete the necessary proofs in required generalities. Standard economic theory, on the other hand, is naturally formalized in terms of inequalities. A case can even be made that this is so that fix-point theorems can easily be applied to prove the existence of equilibria. A case made elegantly by Steve Smale:

> "We return to the subject of equilibrium theory. The existence theory of the static approach is **deeply rooted to the use of the mathematics of fixed point theory**. Thus one step in the liberation from the static point of view would be to **use a mathematics of a different kind**. Furthermore, proofs of fixed point theorems traditionally use difficult ideas of algebraic topology, and this has obscured the economic phenomena underlying the existence of equilibria. Also the economic equilibrium problem presents itself most directly and with the most tradition not as a fixed point problem, but as an *equation*, supply equals demand. **Mathematical econo-**

[40] In a letter to Piero Sraffa, dated 3 September 1960, Sir John Hicks wrote to Sraffa, referring to [57]:

> "Economic theory (teachable economic theory, at least) was getting just a bit boring lately; for the second time in your life you have livened it up again. Thank you."

mists have translated the problem of solving this equation into a fixed point problem.

I think it is fair to say that for the main existence problems in the theory of economic equilibrium, **one can now bypass the fixed point approach and attack the equations directly to give existence of solutions, with a simpler kind of mathematics** and even **mathematics with dynamic and algorithmic overtones**."

[55], p.290; bold emphasis added.

Sraffa, in *PCC,* 'bypassed the fixed point approach and attacked the equations directly to give existence of solutions, with a simpler kind of mathematics', one with 'algorithmic overtones' - essentially by relying on 'existence as construction', rather than appealing to *Hilbert's Dogma.* One of the finest scholars of classical economic theory, who was also an accomplished master of mathematical economics, Sukhamoy Chakravarty, summarised one aspect of Sraffa's mathematical method with complete fidelity:

"Sraffa's austere prose of *Production of Commodities by Means of Commodities* can prove more daunting to most students of economics than the use of matrix algebra. In recent years, an increasing number of textbooks have, therefore, made liberal use of the basic tools of linear algebra, including some results on non-negative square matrices to derive the analytical results which *Sraffa largely demonstrates constructively with the help of English prose.*"

Chakravarty ([10], p.122; second set of italics, added)

For over thirty years I have been making the case for proving one of these famous theorems on non-negative square matrices – in particular the Perron-Frobenius theorems – using the constructive framework Sraffa has provided, rather than the other way about. There are gradual stirrings and hints that some devotees of Sraffian economics may have begin to think along these lines, although they are – so far as I have been able to gauge – entirely unversed in serious constructive analysis (or even computable analysis).

Instead of reading Sraffa's book directly, most mathematically minded economists read it with a background in classical mathematical economics. In a repetition of the fate that befell Bishop and his students, at the hands of journal referees who were unable to see beyond the methods of classical mathematical economics, Sraffa's book, and its mathematics, was condemned to mathematical oblivion simply because familiar notation, orthodox mathematical tools and standard proof techniques were not harnessed by him, in deriving his impeccably rigorous results. Thus, the profession simply recast the economics of *PCC* in the mathematics of linear algebra and proceeded to assure itself, as in the gratuitous words of a leading exponent of this *genre*, Frank Hahn ([21], p.353):

"Sraffa's book contains no formal propositions which I consider to be wrong ....."[41]

The simplest of examples of how he and legions of others satisfied themselves that *PCC* 'contains no formal propositions [that they] consider to be wrong' can be given by taking one of Hahn's own renderings of a 'formal proposition', ostensibly from *PCC*. According to Hahn's reading of *PCC*, Sraffa in *PCC*, when *constructing* the standard system, is looking for a positive vector $\mathbf{x}^*$ and a (positive) scalar G^* such that the following vector-matrix equation is satisfied(op.cit, p.355)[42]:

$$x^* = G^* A x^* \tag{1}$$

where the $n \times n$ matrix A consists of elements $a_{ij} > 0, i, j = 1, ..., n$

It is at this point that the usual 'distortion' and misreading of *PCC* enters the fray. Having formulated the problem of the construction of the standard system as one of finding particular eigenvalues and eigenvectors of a system of linear equations, Hahn goes on to claim, with almost dismissive disdain (ibid):

> "We now have a purely mathematical problem for which there is a standard mathematical result. ... The [vector x^*] is a pure construct as of course is [1] used in its derivation."

He even helps the reader by referring to the appendix in his own book (written jointly with Arrow, but he refers to the wrong appendix) for the 'standard mathematical result'. He does not, of course, tell us in the article or in the appendix of the book with Arrow, what assumptions were needed to prove the mathematical result he invokes. Nor does he add any caveat on the care with which *PCC* avoids any matrix formalizations. Above all, he does not warn the reader that (1) is not used in the derivation of the *construction* of x^* in *PCC*.

To be more precise, we are not informed, either in the above article by Hahn or in the book with Arrow to which he refers for 'the mathematical result', of the assumptions, frameworks and the methods of proof used in the derivation of those results. Perhaps they were derived by hand-waving or *undecidable disjunctions*. In fact, the Perron-Frobenius theorems are generally proved by an appeal to the *Brouwer fixed point theorem* (although there are other ways to prove them, too) where, at a crucial stage of its proof, as pointed out above, appeal

[41] The completion of the sentence reads: '...although here and there it contains remarks which I think to be false'. (ibid, p.353). This is, in my opinion, a statement that is not easy to substantiate about 'remarks' in a rigorous book, where there is not a single categorical statement - as remarks or in any other form whatsoever - without rock solid logical underpinnings. There are, of course, suggestions, with impeccable caveats - the prime example being the famous one to end the penultimate paragraph of p.33 in PCC:

> "The rate of profit, as a ratio, has a significance which is independent of any prices, and can well be '*given*' *before* the prices are fixed. It is accordingly *susceptible* of being determined from outside the system of production, in particular by the level of the money rates of interest." (italics added)

[42] Not all of the assumptions in Hahn's rendering are faithful to the economics of *PCC*, in particular the assumption of a strictly positive A; but let that pass.

is made to the Bolzano-Weierstrass theorem, which is provably impossible to constructivise. Whether Sraffa was aware of this particular infelicity in deriving the 'mathematical result' which Hahn and others wave with a flourish whenever they mention the standard system and its construction is not the issue. The point really is that uncritical appeal to standard mathematical results means the mathematical and logical baggage underpinning it comes with it and could make a mockery of the economic rationale for the result and, most importantly, for the way its validity is demonstrated - i.e., proved.

Richard Quandt' review of *PCC* (op.cit), is slightly more explicit about appealing to the *Brouwer fixed-point theorem* - so beloved of the mathematical economists and the game theorists, but the curse of the constructivists and the intuitionists, with Brouwer himself leading the curse from the front[43]:

> "The existence of positive prices and the uniqueness of the standard system is proved. One feels that the existence proof would, under somewhat different assumptions, be amenable to a fixed point argument. In particular, if the price vector were required to be non-negative only, the Brouwer Fixed Point Theorem might be utilized."
>
> [46], p.500

One cannot help wondering why, if 'existence .. and uniqueness of the standard system is proved', there is any need to make 'different assumptions' just so as to make it possible to use 'a fixed point argument'? Was *PCC* an exercise in teaching or exhibiting the use of alternative 'mathematical results' and 'theorems'? For that purpose one can turn to the great and good mathematics texts themselves. Moreover, even 'if the price vector were required to be non-negative', it is entirely feasible to prove its existence by means of wholly constructive methods, without any invoking of the *intrinsically non-constructive Brouwer Fixed Point Theorem.*

Burmeister ([9]) traverses the same worn out path, a little more explicitly than Hahn and Quandt - and a thousand others - so that it might be useful to have him state his case, too:

> "In Production of Commodities by Means of Commodities Mr Sraffa demonstrates that there exists a 'Standard System'.... . [A]pparently it is not widely recognized that the proposition can be easily established from well-known theorems in linear algebra. Here a straightforward proof is given; it circumvents much of Mr Sraffa's discussion in chapters III, IV and V, and hopefully will be enlightening to the mathematical economist."
>
> [9] p. 83.

[43]Obviously Professor Quandt does not realise that any appeal to the standard version of the Brouwer Fixed Point Theorem means also an appeal to the *Bolzano-Weierstrass Theorem.* This latter theorem, because of its intrinsic reliance on *undecidable disjunctions*, cannot be *constructified* by anything less than pure magic - a fact recognized by Brouwer quite soon after he had enunciated it and, therefore, rejected it. *See the discussions in the earlier sections of this paper.*

More importantly, what was the advantage in 'circumventing Mr Sraffa's discussion in chapters III, IV and V'? And how will it be 'enlightening to the mathematical economist' to establish the same propositions demonstrated by a faultless and innovative logic of mathematics by Sraffa 'from well-known theorems of linear algebra'? Surely, a competent mathematical economist would be curious to learn new methods of proof rather than simply rehash 'well-known theorems in linear algebra'? Or is Professor Burmeister suggesting that the economic propositions in *PCC* are so important and innovative that establishing them - of course without violating the assumptions in *PCC* - with the more familiar mathematics of the mathematical economist might serve a higher purpose? But that, too, will not make sense - because the economics of *PCC* is inextricably intertwined with the mathematical methods devised for proving the propositions on existence and uniqueness and 'circumventing' the three mentioned chapters would be like removing the good *Prince of Denmark* from that tragic drama played out in Elsinore.

The same drama played out in the foundations of mathematics, epoch after epoch, was repeated in the purest parts of economic theory – but to that tale was added an ideological twist, at least in my opinion. By declaring that Sraffa's mathematical method was less than rigorous and, moreover, that it was only a special case of the framework developed by von Neumann ([74]), the important economic message in the book was effectively subverted. Similar to the way the classically trained mathematician, refereeing the works by Bishop and his students, could not understand the point of 're-proving' classically derived results, the less than competent mathematical economist reduced *PCC* to a special case of this or that version of some orthodox version of economic theory.

This kind of insidious thought censorship, by self appointed Commissars of correct thinking, plague not only the foundations of mathematics. They are alive and well in economics – and I guess in evey domain of the pure sciences and in the theoretical recesses of every kind of academic discipline. Unless and until we unshackle ourselves from the perceptive Thorkildian aphorism, we will remain intellectual neanderthals.

A A Personal *Nonstandard* Odyssey

> "The very first model of nonstandard analysis, due to Schmieden and Laugwitz ([49]), was in fact completely constructive. ... We emphasise that the development [in this paper] is done in compliance with Bishop's strict constructivism ([2]), and that it may indeed be formalised within Martin-Löf's type theory [see, for example, [38]], which will be the official metatheory in case of doubt. Thus *it is in principle possible to extract algorithms from all the existence results we establish.*"
>
> [41], p. 233, 235; italics added.

Just to make my position on the role, indeed the importance of non-standard analysis, in computable economics clear, I shall quote an early stance I took on this important issue. In [68], I pointed out (footnote 1, p. 587):

> Although it may appear paradoxical, I am of the opinion that *non-standard analysis* should be placed squarely in the constructive tradition - at least from the point of view of practice. Ever since Leibniz chose a notation for the differential and integral calculus that was conducive to *computation*, a notation that has survived even in the quintessentially *non-computational* tradition of classical real analysis, the practice of non-standard analysis has remained firmly rooted in applicability from a computational point of view. Indeed, the first *modern* rejuvenation of the non-standard tradition in the late 50s and early 60s, at the hands of Schmieden and Laugwitz (cf. [49]), had constructive underpinnings. I add the caveat 'modern' because Veronese's sterling efforts (cf.[67]) at the turn of the 19th century did not succeed in revitalising the subject due to its unfair dismissal by Peano and Russell, from different points of view. The former dismissed it, explicitly, for lacking in 'rigour'; the latter, implicitly, by claiming that the triple problems of the *infinitesimal, infinity* and the *continuum* had been 'solved'.

However, I have not made - as yet - a sustained case for non-standard analytic computable economics. The reason is that I have not been able to come to terms with the full philosophical and epistemological force of the *Löwenheim-Skolem theorem* and the *Skolem paradox*[44]*.* Skolem gave two proofs of what is now

[44] For the absolute novice, a loose statement of the *Löwenheim-Skolem theorem* goes something like this: *Every formal system expressed in the first order functional calculus has a denumerable model.* The *Skolem paradox*, on the other hand, although not a 'genuine' paradox in the same sense of the other logical antinomies, is also philosophically and epistemologically disturbing. Again, informally phrased, the Skolem paradox states that there is a first order theory, such that if it has an intended model, it has both a countable and an uncountable model. Hunter ([27], Part Three), is a reasonably clear and accessible reference to a formal approach to these important issues. Shapiro ([50]), on the other hand, has a clear, albeit concise, discussion of the paradoxical implications of the theorem and the paradox. The rigorous versions of the theorem and the paradox, together with a characteristically illuminating discussion, can be found in *the Kleene 'classic'* ([28], especially, pp. 425-7).

called the Löwenheim-Skolem theorem, one in 1920, where he used the axiom of choice and, later, in 1922, *without the axiom of choice*. He concluded the beautiful 1922 Lecture[45] with an observation that is of particular relevance for the way I believe algorithmic mathematical economics should be formalized. viz., without any reliance or foundations in set theory (in particular, *ZFC*):

> "....I believed that it was so clear that *axiomatization in terms of sets was not a satisfactory ultimate foundation of mathematics*, that mathematicians would, for the most part, not be very much concerned with it. But in recent times I have seen to my surprise that so many mathematicians think that these axioms of set theory provide the ideal foundation for mathematics; therefore it seemed to me that the time had come to publish a critique. "
>
> [53], pp. 300-1; italics added.

It was not that Skolem was sceptical about the need for – and the possibility of – foundations for mathematics; but he desired it to be 'recursive', possibly based on inductive definitions – he did not think the foundations could be found, or should be sought, in axiomatic set theory. Orthodox mathematical economics – even when using non-standard analysis – seems to pride itself in the fact that the mathematics it uses is founded on *ZFC*.

I have not had time to sort these issues in a clear and simple way, so far, but hope to come to a view in the near future. Till, then, I have confined myself to using non-standard analysis in bridging the gap between the use of *ad hoc* discontinuities in standard nonlinear dynamics and rigorous continuity in non-standard analysis, using infinitesimals imaginatively, even on digital computers.

Related to this is my particular personal satisfaction to note that the *non-standard* proof of Peanos' existence theorem for ODE's avoids the use of the Ascoli lemma. In [71], chapter 1, I mentioned the non-constructive nature of the Ascoli lemma and its suspicious use in the *standard* proof of Peano's existence theorem for ODEs [46].

I would like to take this opportunity to add a very 'personal note' on the way I came to become familiar with non-standard analysis, particularly because mathematical economists seem to think the revival of the noble tradition of infinitesimals owes *everything* to Abraham Robinson's undoubtedly significant contributions. Moreover, very few mathematical economists can even imagine that the infinitesimals of non-standard analysis make it possible to dispel with the *ad-hoc* discontinuities even in so-called rigorous non-linear dynamics, via relaxation oscillations (see [72] on the non-standard analysis of the existence (sic!) of multiple limit cycles in the van der Pol equation, ubiquitous in endogenous business cycle theories).

Economists routinely reason in terms of infinitesimals, without, of course, realizing it. Every time mathematical economists cavalierly invoke 'price taking'

[45] Delivered to the Fifth Congress of Scandinavian Mathematicians, held in Helsinki, in August, 1922.

[46] The elegant exposition of the non-standard proof is given on pp. 30-1, Theorem 1.5.1, in [1].

behaviour due to the insignificance of individual agents in a perfectly competitive market, they are 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 Robert 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 Hobson referred to Giuseppe 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[47] 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 the following 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, [67], '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, 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 real analysis rigorous. Veronese's book was severely criticised[48] for falling foul of

[47] I did not, at that time (1977), know of the work of Ehrlich, ([14]), and others, which came out, in any case, much later.

[48] See, in particular, Peano's 'open letter' to Veronese, [42], in the very first volume of the Journal Peano founded in 1891, *Rivista di Matematica*. I may add that my interest, as a Trento economist, has a regional patriotic flavour in favour of Veronese. He was from

the emerging *orthodox standards of 'rigour'* and fell off the backs of the official mathematical community like water off of a *duck*'s 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(see [72])!

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!

B Homage to Brouwer

On the Shoulders of Kant, Husserl, Gödel and Turing

From the *Maya* to the *Vedas*,
Tertium non datur; in
Madhyamaka's depths
Unknowable vistas.

Kant and *Brouwer*,
Brahma's intuitive creators.
Unfolding sequences
Emerging – chosen and unchosen.

Husserl with Gödel
Survey the *phenomena*
Vishnu preserves
Turing morphes.

For Shiva to dance
The constructive destruction
The cycle repeats
Hilbert, too, came and went.
The search for foundations
Ever renewed,
Always a chimera
Only Nagarjuna's sunya remains.

Vela Velupillai
Christmas day, 2007
Revised, September 11, 2010

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