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FREEDOM, ANARCHY AND CONFORMISM IN ACADEMIC RESEARCH♠

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♠ This is a revised and considerably more focussed version of ASSRU DP 3-2010. This could be considered a 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* - for good and bad. My indebtedness to fellow ASSRU members, Kao Selda and V. Ragupathy, goes far beyond the usual expression of gratitude. It is easy to substantiate my feeling that this paper owes its existence, in this form, as much to their work and advice as to my own attempts. On the other hand the remaining infelicities are entirely due to me.

Abstract

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 caliber 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 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 of 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, economic theory, mathematical economics, '*Hilbert's Dogma*', *Hilbert's Program*, computability theory

Introduction

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

Brouwer was *silenced* by Hilbert[1], 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; Sraffa's rigorous—yet elegant—prose was *silenced*, and distorted, 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 Veronese who was vindicated.

The examples can be multiplied and enriched with episodes of *silencing* dissenters from many fields of research and learning.

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 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 may have been construed as conversions.

What does it take to hold on to a vision? In the absence of institutional support, 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

[1] 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, (van Dalen, 1990, 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."

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 Feynman, had the courage to be '*authority's unrewarded, amateurish conscience*' (till, orthodoxy embraced the vision of the heretics and made it part of a new orthodoxy, to be confronted by new heretics - <u>and</u> the cycle repeated itself endlessly). 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.

The paper's main focus, however, is to discuss, via, the way 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. These issues are discussed in sections 3. Section 4 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.

A Setting

If you can look into the seeds of time,
And say which grain will grow and which will not,
Speak then to me, who neither beg nor fear
Your favors nor your hate.

Macbeth, Act I, Sc. III

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, a 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, (Ehrilch, 2006), 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 be incoherent and was '**banish[ed] from mathematics**' through the work of Weierstrass and others[2]"

[2] In Russell (1937), p. 337 (italics added), he is equally merciless in dismissing any role for the infinitesimal in mathematics (not just in mathematical philosophy):

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 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, that all the problems of physics had been solved : except for just two anomalies: that of the *Michelson-Morley experiment* and *Black Body radiation*! The one led to the relativistic revolution; the other to the quantum intellectual cataclysms[3]

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', (Mill, 1848, 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*.

> "[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! 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."

[3] 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*."

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*"(Weyl, 1921). 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*' (Hesseling, 2003, p. 132). Naturally, this conjecture, if correct, presupposes that Hilbert would have acted as a censoring Commissar, and not as an impartial editor, 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.

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* (Weyl, 1918), and his two subsequent articles, (Weyl, 1919; 1921), 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*. 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'?

Towards the *Grundlagenkrise*

> "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."
>
> (van Dalen, 2005, p. 576); italics added.

There have been many foundational crises in mathematics, but the one I 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. 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. Weyl's intuitionism was closer in spirit to Poincaré's *impredicativism*.

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*, (Bishop, 1967) 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. Theirs was a fate and a drama that was reenacting that which was played at the turn of the 19^{th} 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, more than half-a-century later, after a rejuvenated research into non-standard analysis in a systematic way succeeded in placing infinitesimals on firm foundations.

Hilbert's Dogma

> "I admire the elegance of [a] p*roof of existence*; but I still do not think that for my purpose I needed it. Existence, from my

> point of view, was a part of the hypothesis; I was asking, if such a system existed, *how would it work?"*
>
> (Hicks, 1983, p.375), (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 (von Neumann, 1928), 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*' (van Dalen, 2005 pp. 576-7), '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.

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(1965) observed, with considerable perplexity:

> "[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 (von Neumann, 1928)]; 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, van Dalen (2000, p. 636), noted (italics added), without additional comment that[4]

[4] 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):

> "In 1929 there was another publication in the intuitionistic tradition: an intuitionistic analysis of the game of chess by Max Euwe (Euwe, 1929). 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 than to construct new axioms—say the *axiom of determinacy*—as Steinhaus and Mycielski (1964) did? 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.

Unfortunately, core areas of mathematical economics and game theory, with impeccable orthodox sanction, are replete with false claims and assertions about constructivity, intuitionism and computability. 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).

But this claim is false. And it is 'even worse' because these claims are made in the context of economic policy models and are used

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"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."

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 mathematical economic and game theoretic textbooks, 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.

The *Grundlagenkrise*

> "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'[5]"

(Carl Posy, 1998, pp. 294-5); italics added

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

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

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

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

To which the brilliant 'Brouwerian' response, if I may be forgiven for stating it this way, by Wittgenstein (1976, p. 103) was:

> '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." '

This letter[6], written at the tail end of the *Grundlagenkrise* , marked the beginning of the end of it, and silenced Brouwer[7] 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 ⇔ 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;

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[6] 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.

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

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

Carl Posy (1998), reflecting on '*Brouwer versus Hilbert: 1907—1928*', from a Kantian point of view (*ibid*, p. 292) – both Brouwer and Hilbert had been deeply influenced by Kant, and Hilbert, after all, grew up in Königsberg, which Kant never left!!—summarised the outcome of the *Grundlagenkrise* in an exceptionally clear way, as follows (pp. 292-3):

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

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

To suggest a tentative answer to this question, let me 'fast-forward' forty years, to the trials and tribulations faced by Errett Bishop who re-constructed (sic!) large parts of classical mathematics, observing constructive discipline on the invalidity of the *tertium non datur* and non-admissibility of 'Hilbert's Dogma' in his classic and much acclaimed *Foundations of Constructive Analysis*, (Bishop, 1967), Bishop, too, faced similar personal and professional obstacles to those that Brouwer and his followers faced—although not to the same degree and not from the kind of officially formidable adversary like Hilbert. Anil Nerode, George Metakides and Robert Constable summarise the sadness with which Bishop, too, felt 'silenced', (Nerode et.al, 1985, pp. 79-80):

> "After the publication of his book Constructive Analysis [in 1967], Bishop made a tour of the eastern universities.... . He told me then that he was trying to communicate his viewpoint directly to the mathematical community, rather than through the logicians. ... After the eastern tour was over, he said the trip may have been

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

What is it about the adherence to the *tertium non datur* and to '*Hilbert's Dogma*' that makes a whole profession so intolerant? But obviously it is not only here that intolerance resides. Equally dogmatic, intolerant, voices were raised against Giuseppe 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 (1994), in his masterly summary of '*Veronese's Non-Archimedean Linear Continuum*', while acknowledging the 'tortured and ungrammatical style' of the writing (of a massive book of no less than 630 pages, Veronese (1891), noted that Peano's review of 1892 (Peano, 1892) was 'especially scathing' (*ibid*, p. 127). Detlef Laugwitz, who did much to revive non-standard analysis, described the 'open controversy that blazed up', in 1890, 'when Veronese announced his use in geometry of infinitely large and small quantities', (Laugwitz, 2002, p. 102). When the German translation of the 1891 Italian edition appeared in 1894:

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

A massive two decade-long campaign against what has since become the eminently respectable field of non-standard analysis was launched by many of the mighty scholars of the foundations of mathematics: Cantor, of course; but, 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 (who also played a part on Hilbert's side, against Brouwer, in the *Grundlagenkrise*), met in Geneva, in 1937, (van Lambalagen, 1987), and dismissed off hand the von Mises theory, especially in the light of Kolmogorov's measure-theoretic axomatization of probability. 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 (Kolmogorov, 1963). 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."
> van Dalen, 2005, p. 637.

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

Towards *Computability Theory*

> "The Proustian equation is never simple. The unknown, choosing its weapons from a hoard of values, is also unknowable."
> Beckett: Proust

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[8].

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

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

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, 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: 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. They formed the building block 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* (Hilbert, 1900, 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 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, clearly and unambiguously, the acceptable criteria for the ‘solution of a mathematical problem’ (among which was the validity of the *tertium non datur*):

> [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, titled: *Naturkennen und Logik*. This lecture ended famously echoing those feelings and beliefs he had expressed in Paris, thirty years earlier, (Dawson, 1997, p. 71, italics added):

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

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. 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.** 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, (Turing, 1936-7).

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 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 (1977, 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 identification ‘lawlike =recursive’:
> (i) An axiomatic reason: ... [A]ssuming recursiveness means carrying unnecessary information around. In the formal development, there are many possible interpretations for the range of the variables for lawlike sequences .... .
> (ii) A second reason is ‘philosophical’: the (known) informal justifications of ‘Church's thesis’ all go back to Turing's conceptual analysis (or proceed along similar lines).
>
> Turing's analysis strikes me as providing very convincing arguments for identifying ‘mechanically computable’ with ‘recursive’, but as to the identification of ‘humanly computable‘ with ‘recursive’, extra assumptions are necessary which are certainly not obviously implicit in the intuitionistic (languageless) approach ... "

The path opened up by the foundational results of Gödel, Church, Turing and Post, made obsolete *Hilbert's Program*, without completely resolving the ambiguities surrounding ‘*Hilbert's Dogma*’. I suspect, in view of Gödel's epistemology and his metamathematical results, we will forever remain unable to resolve its status unambiguously – also because Brouwer and the Brouwerians, as well as non-Intuitionistic Constructivists like Bishop, refuse to compromise with logic and language. The extent to which Hilbert was wedded to his mathematical ideology can be gauged from the fact that those who were close to Hilbert ‘shielded’ him from Gödel's remarkable results, presented at the very meeting where Hilbert had enunciated yet another of his paeans to the *Hilbert Program* and to *Hilbert's Dogma*. He - Hilbert - came to hear of Gödel's Königsberg results 'only months later' and 'when he learnt about Gödel's work, he was angry' (van Dalen, 2005, p. 638).

In an even greater twist of fate—or what may felicitously be referred 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 thanks to it a vast simplification of mathematical reasoning can be achieved."
> Nelson, 1987, p. 15

The icing on this twisted cake was the award of the second *Brouwer Medal*, in March, 1973, to Abraham Robinson, on the occasion of which he paid handsome tribute to Browuer, Intuitionism and the key difference between invention and discovery in mathematics, (Dauben, 1995, 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."

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 (Post, 1921) and Skolem (1923) would, in good time, have been (re-)discovered and the mathematical foundations of computer science, not to mention the epistemology of metamathematics, 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 and metamathematics. Either way, eventually, Hilbert's victory—at least in some senses—proved to be, and would have proven to be, pyrrhic.

Harvesting Some Lessons

> "The possibility of the impossible, dreams and illusions, are the subject of my novels,"
>
> José Saramago

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 persisted 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,* (Sraffa, 1960; henceforth, *PCC*), has reached the status of a classic: viz, often quoted, rarely read. *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:

> "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*."
> Smale, 1976, p.290; italics 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*.

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.

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—because it did not invoke 'classical' mathematical results to 'prove' the theorems in *PCC*—and, moreover, that it was only a special case of the framework developed by von Neumann (1938), 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 every domain of the pure sciences and in the theoretical recesses of every kind of academic discipline.

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