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BOURBAKI'S DESTRUCTIVE[♥] INFLUENCE ON THE MATHEMATIZATION OF ECONOMICS[♣]

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[♥] I am using *destructive* as the antonym of *constructive* (in the Brouwer-Bishop senses rather than in its Russian sense). Of course I should have used *non-constructive*; but I am influenced in this choice by the way Sokal, in his famous 'Hoax' paper (1996; 2008) used *profound* as the antonym of *trivial*, when he knew (ibid, #98, p.36) he should have used *non-trivial*!

[♣] This is an ultra-condensed, summary, version of a more extensive paper, with *almost* the same title, to appear as Appendix II, in my forthcoming book: **Algorithmic Foundations of Computation, Proof and Simulation in Economics** (Springer-Verlag, 2012). Conversations with Adrian Mathias, whose lectures on the foundations of mathematics at Cambridge I attended, almost forty years ago, during his visit to Trento a few months ago were most helpful in the framing of some of the issues broached in this paper – as was his brilliant discussion of *The Ignorance of Bourbaki* (Mathias, 1992; 2012). As always, discussions with ASSRU colleagues, Stefano Zambelli, Selda Kao and V. Ragupathy, on topics of relevance to the subject matter tackled in this paper, were enlightening. Given the controversial stance I take, on *Bourbaki in Economics*, it is particularly important to emphasise that none of the above four worthies are even remotely responsible for any of the infelicities in it.

Abstract

The first appearance of a reference to a Bourbaki mathematical result was the spoof by D.D. Kosambi, published in the first volume of the **Bulletin of the Academy of Sciences of the United Provinces of Agra and Oudh**, eighty years ago, although it was not the first reference to Bourbaki in a mathematical context. In mathematical economics there seems to be an increasing identification of Debreu's mathematization of economics with Bourbakism, although the Post WW II mathematics of general equilibrium theory can be shown to be consistent also with the contributions of the Polish School of Mathematics in the interwar period. In this paper an attempt is made to summarise the story of the emergence of Bourbakism, originating in India, and its recent demise as well as how it played a *destructive role in mathematising economics* in one, uncompromisingly nonconstructive, mode.

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§1. An *Indian* Preamble on Bourbaki[1]

> **"It is regrettable that it is not explicitly mentioned [by Kosambi] how much [in his paper] originates from [D.] Bourbaki[2] and what the generalization consists of, since the mentioned work by the Russian is unavailable to most scholars."[3]**
> **Shouten (1932)**

Serendipitously, 2012 is the 80th anniversary of the publication of Kosambi's (1932) famous spoof on *Bourbaki*, marking the first appearance of this name in a mathematical publication. But it may not have been the last such *Indian grounded spoof*! Halmos (1957, p.89; italics added), in his elegant **Scientific American** piece on *Nicolas Bourbaki* pointed out:

> "At about the time Bourbaki was starting up, another group of wags invented *E.S. Pondiczery*, a purported member of the Royal Institute of Poldavia[4]. The initials (*E.S.P., R.I.P*) were inspired by a projected but never-written article on extrasensory perception. Pondiczery's main work was on mathematical curiosa. His proudest accomplishment was the only known use of a second-degree pseudonym. Submitting a paper on the mathematical theory of big-game hunting to **The American Mathematical Monthly**, *Pondiczery* asked in a covering letter that he be allowed to sign it with a pseudonym, because of the obviously facetious nature of the material. The editor agreed, and the paper appeared (in 1938) under the name H.Pétard[5]."

Whether Pondiczery was a pun on *Pondichery*, or whether the author was an Indian (from Cambridge or Princeton), has never been made clear. The Hilbert or Axiomatic Method, Axioms I and II, the

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[1] Professor Ramaswamy wrote me in July, 2010, with an early version of his tribute to Kosambi (*fils*); I wrote back (on 6 July, 2010):

> For a long time I have been 'campaigning' against the *Bourbakian influences in mathematical economics*. One of my Cambridge mathematics teachers, later a colleague at Peterhouse, Adrian Mathias, …. contacted me after a silence of thirty years, a few months ago. He had written an elegant piece titled: *The Ignorance of Bourbaki*, [op.cit]**,** about a decade and a half ago, which I have been using very regularly in my lectures. He is coming to Trento, from the island of **Réunion**, where he now lives and teaches, in late October, to give a talk on the mysteries of Bourbakian rope tricks!

[2] *D. Bourbaki* was eventually baptised as *Nicolas Bourbaki* by Éveline Weil (Weil, 1992, p. 101), the former wife of another original Bourbakian, René de Possel (the author of one of the earliest, if not *the* earliest books, on game theory). The saga of Éveline de Possel becoming Mrs Weil, and the early departure of René de Possel from the founding group of Bourbakians, is poignantly outlined in Schappacher (2006).

[3] My translation from the original German. Schouten's abstract seems to indicate that he took Kosambi's spoof seriously. Schouten was Professor of Mathematics at Delft and he is relevant in this story for two personal - from my point of view – reasons. His role in removing Brouwer's influence on the Journal **Compositio Mathematica** (cf., van Dalen & Remmert, 2006 and Velupillai, 2010 & 2011 for some history of these shenanigans against Brouwer by Hilbert and Schouten); and as the brilliant Dirk Struik's doctoral thesis advisor. In his last letter to me, dated 5 April, 1993, my own maestro, Richard Goodwin wrote me:

> Very many thanks for the piece by Dirk Struik: I followed with real interest and concern his horrible treatment [by HUAC in Washington D.C] because I knew that I was equally vulnerable, but being a self-centered bastard and a more totally convinced Marxist, I had never publicly been active and supportive of msc, left wing ventures. And, God bless than, Harvard had sacked me and so I sailed for Europe where one could be left and not left out! I could read without fear what was happening to [Dirk Struik] back there. When, thanks to dear friends like Dan Boorstin and Bob Davis, I was marked as a Marxist or worse, I came to the attention of MaCarthy and received his kind invitation to be his guest at a public lynching. I simply tore up the summons – with nothing worse happening except the cancellation of my passport.

[4] Perhaps not unrelated to the fact that Nicolas Bourbaki's 'origins' were in *Poldevia* (Weil, ibid, p. 102)!

[5] Pétard returned to the same scene, with the same pseudonym, with an equally facetious contribution, almost thirty years later, and the earlier classic spoof was even the subject of an even more amusing spoof, also in **AMM**, in 1967 (see Pétard, 1938, 1966 & Roselius, 1967), by a *Christian Roselius of Tulane University*. I leave it to the 'interested reader', John Kelley's (1955, p. vi) 'elusive creature', to speculate on *this* pseudonym!

Rule of Procedure and the Bolzano-Weierstrass Method, as defined on the very first page of Pétard (1938), aka *Pondiczery*, cannot but be read as a dig at the *destructive* mode of the Hilbert-Bourbaki mathematical method!

The ultimate spoof was, of course, the more recent, spectacular one by Alan Sokal (op.cit), but to the best of my knowledge there was no Indian angle to it, mathematical or otherwise.

Ever since the appearance of André Weil's autobiography (in French in 1991; in English as Weil, 1992), the *Indian Bourbaki connection* has been well known to scholars and students interested in the origins of this famous[6] pseudonym.

There is, therefore, no need for me to rehash known *facts*; but some *interpretation* of the facts may well be in order, especially since I am less than convinced of Weil's strong opinions on Indian and other personalities he met during his brief, two-year, sojourn in India, in 1930-32, the workings of Indian Institutions of higher learning and even of Indian history and mythology[7]. In particular, his caricature of the mercurial Sir Syed Ross Masood[8], whose personal generosity was instrumental in hiring him to come as Professor of Mathematics, at the tender age of 23, to Aligarh Muslim University. Sir Syed had assumed duties as its Vice Chancellor, in 1928, with a brief to revamp its scholarly underpinnings with competent and prestigious appointments.

Apart from Weil, there was also the appointment of Rudolf Samuel, an experimental physicist, to the Physics Professorship. Of this appointment Weil is scathingly condescending, forgetting that his own appointment to the Mathematics Professorship at AMU had been on the basis of a recommendation by his Strasbourg friend, Sylvain Lévi, the orientalist and Indologist, who was also a friend of Sir Syed[9].

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[6] Some would say 'infamous'; a representative sample of the better researched and 'insider' accounts can be found in Beaulieu (1999), Mashaal (2002) and Schwartz (1997), as well as various articles and interviews with the individual mathematicians who made up the collective that was – is – Nicolas Bourbaki, in **The Mathematical Intelligencer** (eg., Senechal, 1998) in the 1990s and the first decade of this century.

[7] The general tenor of Schappal (op.cit) on Weil's character, supports copiously, my skepticism of the reliability of Weil's opinions on his Indian 'adventures'. His justification for absconding from serving in WWII, invoking the Gita and Dharma, is most amateurish, even if that. I suspect, in fact, it is *Karma* he means, when he uses *Dharma*.

[8] Incidentally, **A Passage to India** was dedicated to Syed Ross Masood by E.M. Forster (mentioned also in Weil, op.cit., p. 59), who had tutored him at Cambridge. Forster visited India, in 1912/3 , at Masood's invitation.

[9] Weil's remarks on Rudolf Samuel's appointment on the basis of a recommendation letter from Einstein – who was yet to make his decisive break with Europe – is, to put it mildly, most condescending:

> In physics , Masood thought it a triumph when he pushed through the appointment of a German whose only qualification was a letter of recommendation from Einstein, and whose merit in Einstein's eyes could only have been that he was an unemployed Jew – for he never displayed any other qualities.

Were Weil's qualifications, after Cartan's appointment at Strasbourg, any better than Samuel's, especially since in the former case the recommendation did not even come from a Jewish mathematician? Weil is only slightly less scathing about R.F. Hunter's appointment to the Chemistry Professorship at AMU. Hunter later became the head of the Department of Botanical Studies at the Scottish Hill Farming Research Organization in Edinburgh.

Moreover, the facts about Sir C.V. Raman's visit to AMU do not substantiate the assertions by Weil (ibid, p. 81) on the appointment of Samuel – who, after all did stay on at AMU till 1936, trained some distinguished Indians in experimental chemistry, particularly spectroscopy, a field in which Indian science was not quite backward, even at that time[10]. Moreover, Samuel also collaborated productively with the Professor of Chemistry, the Englishman, R. F. Hunter, before both of them left AMU in 1936[11].

Weil's caricature of Babar Mirza's marriage to a German, and her alleged role in his support for Subhas Chandra Bose is, surely, an affront to the intelligence of this distinguished scientist, who served AMU nobly and for long.

As far as I am concerned the true Indian Bourbaki connection was provided by the active political and humane personality of that outstandingly critical, some-time Bourbakist, Laurent Schwartz, especially as brilliantly and sympathetically portrayed, in the review of **A Mathematician Grappling with His Century** (Schwartz, 1997), by Hermann Weyl's successor at ETH, the famous Indian mathematician K. Chandrasekharan[12] (1998). A point I shall make later, against economists who identify the work of a mathematician with and without his Bourbakist hat, is perceptively pointed out by Chandrasekharan, with respect to Schwartz's pioneering work on *distributions* (ibid, pp. 1143-4; italics added):

> "It is an extraordinary instance of *cerebral percolation* that, after such a trying period entirely taken up with problems of survival rather than mathematics, Schwartz should have come up with his idea of *distributions* [in 1944-45] .. . Bourbaki's influence on the *process of percolation* clearly is a moot point."

§2. Bourbaki's Methodology and Epistemology – Brief Reflections.

> **"*The intuitionist school, of which the memory is no doubt destined to remain only as an historical curiosity*, would at least have been of service by having forced its adversaries, that is to say definitely the immense majority of mathematicians, to make their position precise and to take more clearly notice of the reasons (the ones of a logical kind, the others of a sentimental kind) for their confidence in mathematics."**
> **Bourbaki (1984; 1994), p. 38.; italics added.**

[10] Incidentally, when Samuel, an ardent Zionist, renewed his efforts to emigrate to Palestine, Raman wrote a warm and appreciative letter of recommendation on his behalf (in 1935).

[11] An example would be *The Transition of Covalency to Electovalency*, by R.F. Hunter & R.Samuel, **Journal of the Society of Chemical Industry**, pp. 733-740, 25 September, 1936.

[12] In his e-mail of 6/7/2010, Professor Ramaswamy wrote me (italics added):

> Chandrasekharan is alive and not very well in Zurich. He has become a recluse, and has not allowed contact for several years now - and *he and Kosambi started the TIFR* [Tata Institute for Fundamental Research] *mathematics school in the 40's and 50's*. I think they eventually fell out quite badly.

One of my earliest introductions to *Brouwerian Intuitionism* was through Chandrasekharan's elegant pedagogical article, written in *his* youth and published in **The Mathematics Student** (Chandrasekharan, 1941). My introduction to this article was via a reference in the rare classic by Rasiowa & Sikorski (1963). The distinguished Indian Astrophysicist, Jayant Narlikar, who was a Professor at TIFR in Bombay, knew E.M. Forster very well during his Cambridge days at King's College. It was, by the way, Jayant Narlikar who introduced me to Luigi Pasinetti, during my first days as a PhD student at King's College, in October, 1973.

Robin Gandy (1959, p. 72; bold italics added), in his remarkably lucid and critical review of the first part of the original ***Théorie des ensembles*** by Bourbaki, reacted to the above premature *Obituary* of 'the intuitionist school' with much prescience:

> "[T]he logic that is developed is, right from the start, *completely classical*. (Indeed, in the historical note [see the above quote] .. it is asserted that Intuitionism will subsist only as an historical curiosity. … But it is nowadays[13] clear that classical theory (as here formalized) is too ambiguous to give a definite answer to some problems of analysis (e.g. the continuum problem). ***It is possible then that this book may itself soon have only historical interest***."

The 'possibility' that Gandy envisaged has been realized; and '*The Intuitionist school*' is alive and well, with the new lease of life provided by the exciting developments of providing foundations for mathematics via category theory[14].

My aim in this brief section is simply to point out *three* directions of contemporary mathematical research, both in foundational studies and in mathematical practice, that make Bourbakian methodology and epistemology to 'have only historical interest', especially for the mathematization of economics: (a). the obsolescence of set theory or, to be more precise, Bourbakian *structures* **-** as the sole provider of foundations for mathematics; (b). the practical relevance of undecidability, even in economically applicable dynamical systems theory; and, (c). the resurgence of intuitionistic logic and the relevance of varieties of constructive mathematics.

In the case of (a), it has been found, in the last fifty years, first gradually, but now more clearly and increasingly, that category theory offers a 'more fruitful foundational framework for mathematics than Bourbaki's structures.' (Aubin (2008), p. 825). Here, 'more fruitful' means, from the point of view of non-classical, computationally oriented logic, an encapsulation of whole fields 'officially' excluded from the Bourbakian 'algebraic' program (cf. Aubin, ibid; Schwartz, 2001, p. 163).

In the case of (b), even as enlightened a Bourbakist as Roger Godement, in his superbly pedagogical **Algebra**[15] (Godement, 1969), felt able to suggest that 'the reader is unlikely to meet [a relation that is **undecidable**] *in practice*' (ibid, p. 26; italics added). After the celebrated Paris-Harrington results (Paris & Harrington, 1977) and the further results on Goodstein's sequence, even for applicable – especially in economic dynamics – natural number constrained dynamical systems (Paris & Tavakol, 1993), this same reader is, surely going to be routinized into meeting *undecidable relations in practice*. The added bonus of bringing into the fold *Ramsey Theory* and questions on the valid mode of interaction between the finite and the infinite (cf. Ramsey, 1926 and Ragupathy & Velupillai, 2011 for a discussion from the point of view of economic theory and its formalization).

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[13] Gandy's review appeared, recall, in 1958. Incidentally, Robin Gandy was Alan Turing's only doctoral pupil and was Turing's literary executor. Alan Turing, by the way, was conceived in India, exactly a century ago!

[14] Itself developed by Eilenberg and MacLane (1945) , the former while *not* wearing his Bourbaki hat!

[15] Which was my own textbook on the subject in my postgraduate years.

Finally, in the case of (c), coupled to (a), there is the resurgence of interest in the possibilities for 'new' foundations for mathematical analysis[16] - in the sense of replacing the 'complacent' reliance on set theory supplemented by ZFC (i.e., Zermelo-Fraenkel plus the axiom of choice) – brought about by the development of *category theory*, in particular a category called a *topos*. The underpinning logic for a *topos* is entirely consistent with *intuitionistic logic*, hence no appeal is made either to the *tertium non datur* (the law of the excluded middle) or to the *law of double negation* in the proof procedures for topoi. This fact alone should suggest that categories are themselves computational in the precise sense of Brouwer-Bishop constructive analysis. As Martin Hyland[17] (1991, pp. 282-3; italics and 'quotes' added) emphasized:

> "Quite generally, the concepts of classical set theory are inappropriate as organizing principles for much modern mathematics and dramatically so for computer science. The basic concepts of category theory are very flexible and prove more satisfactory in many instances."

It is deficient scholarship at the deepest level of mathematics, and its frontier foundational developments, that allows a so-called mathematical economist to make senseless assertions like the following (Ok, 2007, p. 279)[18]:

> "If you want to learn about intuitionism in mathematics, I suggest reading – in your spare time, please … articles by Heyting and Brouwer… ."

No wonder economists have been, at best, 'spare time' mathematicians

§3. Bourbaki's *Destructive* Mathematization of Economics

> **"The doctrine mentioned in the title[19] is the assumption, implicit or explicit, that only formally defined notions and therefore only explanations in formal terms are precise. …. But, as is well known, mathematical practice continues to use the "rejected" notions, and, more important, it makes no attempt to eliminate these notions *even when this is possible*. In other words, there is a *conflict* between (mathematical) practice and (logical) theory on what is needed for precision."**
> **Kreisel, 1969, p.2.**

In the *Preface* to the **Theory of Value** (Debreu, 1959, p. viii), we are informed that:

> "The theory of value is treated here with the standards of rigor of the contemporary formalist school of mathematics. The effort towards rigor substitutes correct reasonings and results for incorrect ones, but it offers other rewards too."

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[16] Not that any serious student of Bishop (1967) – as I have been for at least thirty years - will feel the need for *any* foundations for analysis, let alone '*new*' ones! The brilliant review of this classic by Bishop on constructive analysis, contrasting it with the claims and aims of classical analysis, by Stolzenberg (1970), is still worth reading carefully, especially in this post-Bourbaki era.

[17] Indeed, it was Hyland's early result on filter spaces that debunked Bourbakian beliefs that their formalization of *continuous* encapsulated *completely* the intuitive notion of *continuity* (Gandy, 1991, Hyland, 1979). Hyland had been a pupil of Adrian Mathias, at Cambridge.

[18] The full quotation, in footnote 47 of p.279, is too full of incorrect assertions that I constrain myself to the least offensive part.

[19] *The Formalist-Positivist Doctrine of Mathematical* ***Precision*** *in the Light of Experience* (bold italics added).

Clower, (1995, p.311), in his wisdom, surmised that Debreu was 'referring, *presumably,* to the 'school' of Bourbaki'. The caveat 'presumably' is crucial. Since nowhere in the rest of the book do we find a precise definition of 'rigor', who or what consists of the 'contemporary formalist school of mathematics', or what consists of 'correct reasoning'[20]. Yet many *lesser* economists than Clower have rushed into equate this reference to 'the contemporary formalist school of mathematics' with the Bourbakists. Even a small, partial, list of such 'equators' would include Boylan & O'Gorman (2009), Düppe (2010), Weintraub (2002) and Weintraub & Mirowski (1994)[21].

Consider, however, the following counterfactual scenarios.

Imagine a mathematically competent economist, with supreme training in mathematics via the books of van der Waerden (1930; 1970), Birkhoff & Mac Lane (1941) and Mac Lane & Birkhoff (1967). Now imagine, also, another economist – equal in mathematical competence to the first one – trained admirably in Kuratowski & Mostowski (1968)[22] and Kuratowski (1958; 1966). Add to this the innocuous, even fairly 'realistic', assumption that neither of these two mathematically competent economists had ever heard of Bourbaki – even in Kosambi's spoofed version[23].

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[20] Would Debreu claim that any example of 'reasoning' in Bishop (1967) is 'incorrect' because it does not adhere to the definition of 'rigor' attributed to the 'contemporary formalist school of mathematics? After all, Bishop explicitly disavows allegiance to any 'formalist school of mathematics', 'contemporary' or otherwise!

[21] Although I endorse the philosophical message in Boylan & O'Gorman, the mathematical underpinnings, particularly Bourbakian claims are less than satisfactory. Düppe's claims are too absurd to warrant even a cursory discussion – but I do devote considerable space to debunking his ridiculous assertions in Velupillai (2012). One aspect of his mistake is the reliance on Giocoli (2002) for his constructivist assertion on von Neumann. Giocoli, in turn, relies on the thoroughly incorrect definition of constructivity in Punzo (1991) – which also mars the otherwise meticulous Weintraub (2002) discussion on Hilbert's formalism, for which he, too, relies on the completely false and unscholarly definition and discussion in Punzo (*ibid*). Düppe's extraordinary phrase, 'Even if [Debreu] is now peremptorily rejected or belittled as outmoded …', must, surely, make every Computable General Equilibrium theorist, Real Business Cycle theorists building on the foundations of Recursive Competitive Equilibria and Stochastic Dynamic General Equilibrium theorists writhe in intellectual pain! Finally, there is the following assertion, about which I am at a loss to say anything sensible (ibid, p.2):

> Furthermore, although the entire effort of Debreu was motivated by, and becomes intelligible only be reference to, his training in a specific school in mathematics – 'Bourbaki' – there are but a few economists who have ever heard of the name, and even less who have read it. Debreu did .. and everything he said about his own work … can be found almost word for word in Bourbaki.

There is no evidence that Düppe has even read Debreu (*Uncertainty* forms the subject matter of chapter *7, not* 6), let alone anything serious by Bourbaki! As for only 'a few economists who have ever heard of the name…', I'll just leave it at that, *pro tempore*! Also, 'keeping close contact with' Weil during his Chicago days (*ibid*, footnote 1) is one thing; to know, work with, and learn from *Weil as a Bourbakist* is quite another thing, as tirelessly emphasized by Schwartz, Cartier, Cartan, Diudonne and even Weil (see also footnote 14, above).

[22] I remember with crystal clarity the following candid signpost in this wonderful book – following *Principia Mathematica* practice (*ibid*, p.vi): 'In order to illustrate the role of the axiom of choice we marked by a small circle ° all theorems in which this axiom is used.' Would that mathematical economists did the same!

[23] This is not an entirely fanciful counterfactual. Richard Goodwin told me that he once took unpaid leave for one academic year, in the mid-1940s, and went 'away' with Birkhoff-Mac Lane & Courant-Hilbert, and came back and wrote *The Multiplier as Matrix* (Goodwin, 1949) – and much else. To his dying day he was blissfully ignorant of Bourbaki. He had, by the way, also attended lectures by Marston Morse.

Now suppose these two mathematically competent economists chance upon *The Theory of Value*. Why would they 'equate' Debreu's reference to the 'contemporary formalist school of mathematics' with the Bourbakists[24]? Economists who rush to equate all kinds of axiomatic formalizations in economics with Bourbakism, especially Debreu's work, forget that there were many 'formalist schools of mathematics', and almost all of them had begun to evolve – some, like the Polish School and the Algebraists, quite independent of Hilbertian formalism and the Hilbert Programme – long before the Bourbakists were even conceived, via spoofs or not.

Long before the Bourbakists had any influence in American Mathematics, especially in mathematical economics, Marshall Stone, a doyen of the field at US Universities, paid handsome tribute to the *Polish School of Mathematics* of the interwar period (quoted in Kuzawa, 1968, pp. 15-16; second set of italics, added:

> "The work of men who have founded and developed *Fundamenta Mathematicae* has had a deep influence on the mathematical progress of the past quarter-century. Starting with Jamiszewski and Sierpiński, there has grown up a fruitful movement with which American mathematicians have had intimate and effective relations. The work in Topology and in abstract spaces is now recognized throughout mathematics as of fundamental character; the *Polish School* under such men as Banach and Kuratowski constituted, before the present catastrophe (1939), one of the outstanding mathematical groups."

Stone's tribute, concentrating on set theory, topology and functional analysis, does not go far enough: he has forgotten the pioneering contributions made by the Polish School, in that heroic twenty-year period of 1919 - 1939, to recursion theory, recursive analysis and metamathematics.

If the economic theoretic *crown jewels* of orthodox mathematical economics are the *Arrow-Debreu equilibrium existence theorem* and the *fundamental theorems of welfare economics*, then the mathematical crown jewels that underpin them are, surely relevant *fixed point theorems* (Brouwer, Knaster-Kuratwoski-Mazurkiewicz [KKM], Kakutani) and *separating* and *supporting hyperplane theorems* (especially the *Hahn-Banach theorem*). All of these can be taught – and learned – in a completely *rigorous axiomatic framework* by anyone trained *only* in Polish Mathematics, *without* any help from the Bourbakists. Elementary texts on equilibrium theory pay at least lip service to what I have come to call the *Polish Fix Point Theorem* in proving the existence of an *Applied Dynamic General Equilibrium*, and thereby the crucial role it plays in the first fundamental theorem of welfare economics.

[24] Let me to add that I was advised to train myself in the methods of van der Waerden and Birkhoff-Mac Lane by my PhD supervisor, Richard Goodwin, and to read Mac Lane-Birkhoff by Professor Lickorish, who was lecturing on *Algebra* at Cambridge in the early 1970s. I began reading Kuratowski and Mostowski and, later, Kuratowski, also at about the same time – in any case, long before I knew anything about the Bourbakists.

Reflections by the pioneers of mathematical economics (even for example by Debreu, 1984, pp. 268-9) emphasise the crucial role played by the Hahn-Banach theorem in demonstrating the (non-constructive) validity of the second fundamental theorem of welfare economics. Yet, it is von Neumann and Bourbaki who are considered the founding fathers of the mathematics and the mathematical methods of orthodox mathematical economics and formal economic theory - not the legions of interwar Polish mathematicians who framed, codified and formalised set theory, topology and functional analysis (to which the contributions of von Neumann and Bourbaki is a proverbial ε, at least in my opinion).

Above all, the Polish School of mathematics did not neglect either constructive or computable mathematics, as we know from their pioneering contributions to both fields, all the way from Emil Post's work in 1921, via the classics by Banach and Mazur in the late 1930s and the threads that were taken up, on such foundations, to develop computable analysis by post WW II Polish mathematicians. Unfortunately, the constructive and computable analytic trends that enriched the formalistic part of the Polish School of mathematics were not part of the Bourbaki tradition. Thus, assuming *pro tempore* that the 'equators' are justified in identifying *Debreu's method* with the *Bourbaki method*, then it is easy to show its *destructive* content, both *methodologically* and *epistemologically*, which is what I shall do, briefly, for the rest of this section.

There are at least 31 'propositions'[25] in the **Theory of Value**, not including those in chapter 1, titled *Mathematics*. The Bourbakian mathematical method comprises the triptych of *axiomatization*[26], *proof* and *rigour*. I shall have to pass on axiomatization, purely for reasons of space (although the discussion in Velupillai, 2012 will deal copiously with this subject, essentially based on Kreisel-Krivine, 1971 and Kleene, 1952). Rigour, for the purposes of my interpretation of Bourbakian *destructive* economics, via the **Theory of Value**, is coupled to proof.

Thus, the *destructive* content of the Bourbakian mathematization of economics can be narrowed 'down' to methods of proof – and in this the Bourbakist's are squarely adherents of *Hilbert's Dogma*[27]. They are *destructive* simply because the methods of proof used in the **Theory of Value** are, all of them, *non-constructive*. Moreover, they are not of the destructive variety that 'constructivization of [them] is almost routine' (Bishop, op.cit, p.359, referring to Brouwer's fixed-point theorem as 'easy to constructivize' – 'routine' and 'easy' have to be interpreted in the appropriate context.

25 Some, but not all, of them are called 'theorems'.

26 More precisely, formalization by axiomatisation – but I shall return to these themes, in much greater detail, in Velupillai (2012).

27 Discussed in some detail in Velupillai (2011).

In particular, the two fixed-point theorems – Brouwer and Kakutani – the separating hyperplane theorems and the Bolzano-Weierstrass theorems are not *routinely* constructifiable, i.e., irreversibly destroyed, by the reliance on Bourbakian mathematical method. For example, in the Bolzano-Weierstrass theorem, in the Bourbakian version, adopted in the **Theory of Value**, there are *two* destructive steps, one of which is routinely constructifiable, the other is not, without adopting constructive mathematics. For the separating hyperplane theorem(s), more constructive calisthenics have to be indulged in, from the outset. For example, by starting from separable (metric) spaces[28].

§4. Towards a *Non-Destructive* Mathematization of Economics

> **"By the end of the 19th century .. the two major figures were Poincaré and Hilbert. … Poincaré's thought was more in the spirit of geometry, topology, using those ideas as a fundamental insight. Hilbert was more a formalist; he wanted to axiomatize, formalize, and give rigorous, formal, presentations. They clearly belong to two traditions.. .**
> **Bourbaki tried to carry on the formal program of Hilbert of axiomatizing and formalizing mathematics to a remarkable extent, with *some* success."**
> **Atiyah, 2001, pp.657-8; italics added.**

Not even Bourbaki has succeeded in 'axiomatizing and formalizing' Bishop's constructive analysis. As Harvey Friedman (1977, pp. 1-2) explained very clearly:

> "In [Bishop] there is no systematic attempt to delineate what constitutes *an admissible piece of constructive (analytic) reasoning*, nor to present and analyze an underlying conceptual framework necessary for such a delineation. ….
> Thus for Bishop, there is no need of a formal system to delineate the *admissible reasoning*, for borderline cases do not naturally arise; and there is no need for a philosophical analysis of a conceptual framework. There is a body of mathematics, called constructive analysis, which has an existence of its own *independent of any particular formal system* or conceptual framework in which it is cast."

The message, therefore, is the following: the path towards a non-destructive mathematization of economics entails a complete destruction of the Bourbakian mode of delineating admissible reasoning, appointing commissars to police valid modes of formalization and regimenting the notion of rigour to be utilized in proof procedures. It is to accept that rigour is *undefinable,* proof should not be subject to *Hilbert's Dogma*, nor axiomatization a desirable or necessary feature of a mathematized economics.

The exemplar of the kind of mathematization of economics, emulating Bishop's mode of doing constructive mathematics, that I have in mind is Sraffa's classic **Production of Commodities by Means of Commodities** (Sraffa, 1960). Like Bishop, it has resisted a 'complete' axiomatized formalization. Like Bishop, it employs a collection of admissible modes of reasoning, without seeing any need to embed them in any particular formal system.

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[28] I refer the 'interested reader' to Bishop (op.cit), appendix A and B for hints on non-destructive constructions of proofs of Brouwer's fixed-point theorem and the Hahn-Banach theorem and Mandelkram (1988), particularly for elucidation of the two *destructive* aspects of the Bolzano-Weierstrass theorem.

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