The Ignorance of Bourbaki

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If one looks at the history of mathematics, one sees periods of bursting creativity, when new ideas are being developed in a competitive and therefore very hasty spirit; and periods when people find that the ideas so recently in vogue are inexact, incoherent, possibly inconsistent; in such periods there is an urge to consolidate past achievements.

I said "the history of mathematics": but mathematics is a complex sociological organism, and its growth takes place in different branches and in different countries, even different universities, in different ways and at different speeds. Sometimes national groups feel that mathematics in their country is in a bad way: you find an expression of that in the *Introduction* to later editions of Hardy's *Pure Mathematics*, where he remarks that it was written with an enthusiasm intended to combat the insularity of British mathematics of the turn of the century, which had taken no account of the development of mathematics in France in the nineteenth century.

Indeed in 1910 France could be proud of her succession of mathematicians such as Legendre, Laplace, Lagrange, Fourier, Cauchy, Galois, Dirichlet,[1] Hadamard, Poincaré — a most impressive list of scholars of the highest distinction.

But after the first World War, the feeling in France changed, and the young French mathematicians of the day began to consider that the torch of mathematical research had passed to Germany — where there were many great mathematicians building on the past work of Riemann, Frobenius, Dedekind, Kummer, Kronecker, Minkowski and Cantor, such as Klein, Hilbert, Weyl, Artin, Noether, Landau, and Hausdorff, — and that French mathematics had gone into a decline.

So in 1935, a group of young French mathematicians[2] resolved to restore discipline to their subject by writing a series of textbooks, under the joint pseudonym of Nicolas Bourbaki, that aimed to give definitive expositions with full French rigour to what they deemed to be the most important areas of pure mathematics.

Now the question of mathematical rigour was very topical, a greater disaster than usual having occurred at the beginning of the twentieth century with the discovery by Russell of a major flaw in Frege's proposed theory of classes.

Frege wanted to form for any property $\Phi(y)$ the class $\{y \mid \Phi(y)\}$ of all objects y with the property Φ, and at the same time to count all such classes as objects to which such membership tests might be applied. If we write "$a \in b$" for "a is a member of b" and "$a \notin b$" for "a is not a member of b", we may express Frege's broad principle as follows. Denote $\{y \mid \Phi(y)\}$ by C: then for any object a, $a \in C$ if and only if $\Phi(a)$. Russell, developing an idea of Cantor, noticed that if $\Phi(y)$ is taken to be the property $y \notin y$, of not being a member of oneself, then a contradiction results. For let B be the class of those objects that are not members of themselves; in symbols, $B = \{y \mid y \notin y\}$: then for any y, $y \in B$ iff $y \notin y$; and so for the particular case when y is B, $B \in B$ iff $B \notin B$.

In response to this, there were some who wished to ditch all the more speculative areas of mathematics, which made use of the infinite and particularly of Cantor's theories of cardinals and ordinals. Kronecker, Poincaré, Brouwer and Hermann Weyl should be mentioned here.

But there were others — notably Hilbert — who wished to resist this wholesale amputation, and a programme was proposed aimed at formalising mathematics — the language, the axioms , the modes of reasoning etc — and at proving, by means the soundness of which could not possibly be doubted, that the resulting system was free of contradiction, that is, was *consistent*.

A paper read to an undergraduate mathematical society, the Quintics, in Cambridge on October 29th,1986, and published in the Cambridge undergraduate magazine *Eureka* in 1987. This revision has been made in the light of helpful criticisms from Sir Peter Swinnerton-Dyer, Bar[t], Professor Saunders MacLane, Dr Francisco Corella, Dr Paolo Mancosu, Dr Gérard Bricogne and many others.

[1] who married Mendelssohn's sister and settled in Göttingen.

[2] listed by Chevalley in an interview [M7] as H. Cartan, C. Chevalley, J. Delsarte, J. Dieudonné, Sz. Mandelbrojt, R. de Possel, and André Weil. In a letter cited in the biography [H3] of Cavaillès by his sister, a further mathematician, Ehresmann, is mentioned as belonging to the group.

I said "formalise mathematics" but that is vague: how much mathematics can we or should we include ? Hilbert certainly would wish to keep Cantor's work on ordinals in his formalisation of mathematics, as it was Cantor who made Hilbert possible: Hilbert leapt to fame with his *Basis Theorem* that in modern terminology asserts that if every ideal in the commutative ring R is finitely generated, the same is true of the ring $R[X_1, \dots X_n]$ of polynomials in the indeterminates $X_1, \dots, X_n$ with coefficients in R; and recent studies have shown that the proof of this theorem not only relies on but is in an exact sense[3] equivalent to the wellfoundedness of the order-type ω^ω.

Thus when Hilbert spoke of Cantor's paradise, it was no idle tribute: he acknowledged the creation of a conceptual framework of transfinite induction within which algebraic geometry could advance.

Russell's own ideas on avoiding the paradoxes led to his *ramified theory of types*; this was cumbersome, and a simpler system was proposed by Zermelo in the first decade of the century. Fraenkel, and Skolem, in the third decade proposed *the axiom of replacement* as a strengthening of Zermelo's system; the resulting system is known as Zermelo–Fraenkel. With the addition of the *Axiom of Choice*, first articulated by Zermelo and of great importance in functional analysis and higher algebra, and the *Axiom of Foundation*, proposed by von Neumann, ZFC has proved a very serviceable system.[4]

There are two elements to Hilbert's programme: the creative side, proposing a system within which to work; the critical side, testing the adequacy and consistency of the system proposed. Naturally the Bourbaki group, or *Bourbachistes*, mindful of the possibility of contradiction in mathematics, were determined that their textbooks would be free of such problems, and indeed an early volume in their series, *La Théorie des Ensembles*, was devoted to establishing the foundations necessary for the later ones.

The other day, I thought I would read it.

I was shocked to the core: it appeared to be the work of someone who had read *Grundzüge der Mathematik* by Hilbert and Ackermann, and *Leçons sur les nombres transfinis* by Sierpiński, which were both published in 1928, but nothing since.

Puzzled both by Bourbaki's attitude to foundations and by his attitude to set theory, I started to probe the background and found that the Bourbachistes had published several articles in the thirties and forties expounding the group's position on foundational issues.

Henri Cartan and Jean Dieudonné, wrote essays under their own names on the foundations of mathematics. After the second World War, Nicolas Bourbaki himself addressed the Association for Symbolic Logic in America, and his talk was printed in the *Journal of Symbolic Logic*. Further, he wrote an essay on *L'Architecture des Mathematiques*, which was translated into English and appeared in the *American Mathematical Monthly*.

There is a uniformity to these essays: on the creative side, the set theory they propose is that of Zermelo — not, let me emphasize, Zermelo–Fraenkel — and declare it to be adequate for all of mathematics; and on the critical side, they all show the influence of Hilbert's formalist programme. None of them mention Gödel.

In view of their commitment to Hilbert's programme, that is very remarkable; and some comment on the Incompleteness Theorems is in order.

There was a meeting at Königsberg in September 1930, during which honorary citizenship was conferred on Hilbert, who had retired from his Chair at Göttingen on January 23rd of that year. The famous and powerful address, *Naturerkennen und Logik*, that he gave on this happy occasion is informed by his *credo* that there are no insoluble problems[5] and ends with his resolute battlecry

"Wir müssen wissen;
wir werden wissen."

— we must know, we shall know. With the delicate irony of history, Gödel had the very day before, with von Neumann but not Hilbert[6] in the audience, announced his incompleteness proof, with its applications

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3 See [M21].

4 Zermelo included the Axiom of Choice in his list of axioms in 1908. The present custom is to mention that axiom explicitly as an extra.

5 The penultimate sentence of Hilbert's address [M13] runs *"Der wahre Grund, warum es Comte nicht gelang, ein unlösbares Problem zu finden, besteht meiner Meinung nach darin, dass es ein unlösbares Problem überhaupt nicht gibt."*

6 who presumably was preparing his talk for the morrow.

to any system such as Peano arithmetic or Zermelo–Fraenkel.[7]

One might expect this to cause a sensation: Hilbert had presented a very positive response to the paradoxes, and disciples such as Herbrand had in the Hilbertian spirit established cases of the decision problem. Gödel showed that there were serious limitations to Hilbert's proposal.[8] He showed that no system satisfying certain minimal conditions, such as the clearly desirable requirement that there should be an algorithm telling you of any sentence whether it is one of the axioms or not, — no system of this kind captures all of mathematics, and that proofs of the consistency of such a system can only be given in systems more likely to be inconsistent than the one under discussion.

Given the importance of this result for foundational studies, and given the eager response of von Neumann and others to Gödel's ideas, it is natural to ask what effect Gödel had on the Bourbachistes; and the strange thing is that one searches their publications in vain for mention of his name. One might almost say that they ignored him, except that the tone of certain of their works suggests a conflict between an uneasy awareness that something has happened and a desire to pretend that it has not. It is as though they had discovered that they were on an island with a dragon and in response chose to believe that if the dragon were given no name it would not exist.

For instance, Henri Cartan, in a piece entitled *Sur Le Fondement Logique des Mathématiques*[9] presents the system of Zermelo, including the Axiom of Choice. Though he says he takes some account of the modifications introduced by Fraenkel, he does not include the main one, the axiom of replacement; he commments that Zermelo's system is inconvenient, lacking as it does suitable definitions of ordered pair, *& c.*; and he reveals ignorance of the distinctions that Gödel stressed by saying "true" where he means "provable", "false" where he means "refutable" and "doubtful" (douteuse) where he means "undecided".

He talks of contradictory theories, and says the problem of deciding whether a given theory is contradictory leads to the Entscheidungsproblem, which consists of finding a general method for deciding whether a given relation (i.e. formula) is a logical identity (i.e. theorem). This problem, he says, has only been resolved in particular cases. In general one does not know how to do it. He then says, "But these problems, important though they be, are outside our subject."

He mentions Herband's thesis, Sierpiński's *Leçons sur les nombres transfinis*; adopts a view he credits to Dieudonné, mentioning that these ideas, though published in 1939, "*remontent à* 1938" and makes this statement:

> "*une théorie mathématique n'est pas autre chose qu'une théorie logique, determinée par une système d'axiomes ... les êtres de la théorie sont definis* ipso facto *par le système d'axiomes, qui engendre en quelque sorte le matériel auquel vont pouvoir s'appliquer les propositions vraies; definir ces êtres, les nommer, leur appliquer les propositions et relations, c'est en cela que consiste la partie proprement mathématique de la théorie logique.*"[10]

He mentions Cantor, Kronecker, Zermelo, Brouwer, Skolem's paradox, Poincaré and Lebesgue, **but not Gödel !**

Clearly Cartan was thinking about foundational questions: why then does he not mention Gödel's results ? Among the French speakers I have been able to consult, there is some disagreement, turning on the meaning in 1942 of the phrase *est tout idéal*, as to whether Cartan's article reveals an awareness of the Incompleteness results and a desire to communicate this awareness, which one presumes he must have possessed, to the reader. The passage in question reads thus:

> "*Le problème de décider si une proposition donnée est vraie dans une théorie se ramène à celui-ci: une relation donnée est-elle une identité logique ? De même pour le problème de décider si une théorie est ou n'est pas contradictoire. Ces problèmes se ramènent*

[7] Gödel's announcement at Königsberg was followed by the communication of an abstract to the Vienna Academy on October 23rd 1930, and the receipt on November 17th 1930 of the text of his paper for publication.

[8] For recent appraisals of Hilbert's programme, see *e.g.* [M11], [M18] and [M20].

[9] [M5]: the manuscript was received on January 15, 1942 and published in 1943.

[10] "A mathematical theory is simply a logical theory determined by a system of axioms. The entities of the theory are defined *ipso facto* by the system of axioms, which generates in some way the material to which true propositions may be applied; the mathematical part proper of the logical theory consists of defining these entities, naming them, and applying propositions and formulas to them."

donc, en définitive, à l'Entscheidungsproblem, qui consiste à trouver une méthode générale permettant de décider si une relation, explicitement donnée, est ou n'est pas une identité logique. Ce problème n'est resolu que dans des cas particuliers.

"*De sorte que, jusqu'à nouvel ordre, le partage en trois catégories dont nous venons de parler (propositions vraies, propositions fausses, propositions douteuses) est tout idéal: dans une théorie dont on saurait qu'elle n'est pas contradictoire, il y a des propositions dont on a prouvé qu'elles sont vraies, d'autres dont on a prouvé qu'elles sont fausses (les negations des précédentes), d'autres dont on ignore à la fois si elles sont vraies ou si elles sont fausses. Et encore, généralement, ne saura-t-on même pas prouver qu'une théorie donnée n'est pas contradictoire.*"

Similarly equivocal attitudes are to be found in the 1939 piece, cited by Cartan, by Jean Dieudonné: *Les Méthodes Axiomatiques Modernes et les Fondements des Mathematiques*. He describes the achievements of Cantor, which Hilbert had found so useful, as "*resultats si choquants pour le bon sens !*"[11] He regards the foundational crisis of the beginning of the century as having been resolved by Hilbert's formalist doctrine that the correctness of a piece of mathematics is a question of its following certain rules, and not a question of its interpretation; comments that

"*le principal mérite de la méthode formaliste sera d'avoir dissipé définitivement les obscurités qui pesaient encore sur la pensée mathématique*";[12]

and says that

"*Il reste naturellement à montrer que la conception de Hilbert est réalisable*".[13]

Again, he makes no mention of Gödel, but Dieudonné does, however, hint at a sceptical awareness of Gödel's results in these words:

"*En outre, il semble, d'après les travaux les plus récents, que, contrairement à ce que croyait Hilbert, les règles qu'il serait nécessaire d'adopter en métamathématique, pour aboutir à une démonstration de la non-contradiction des mathématiques, seraient d'un degré d'abstraction aussi élevé que les règles mathématiques elles-mêmes, ce qui amoindrit encore la portée que pourrait avoir une telle 'démonstration'.*"[14]

He confirms this awareness a few years later in his obituary of Hilbert, but still cannot bring himself to mention the dreaded name:

"*Il semble que l'intuition de Hilbert l'ait, pour une fois, entraîné à des espoirs quelque peu exagérés, et on a actuellement de bonnes raisons de douter de la possibilité de telles 'démonstrations'.*"[15]

Nicolas Bourbaki,[16] in *The Foundations of Mathematics for the Working*[17] *Mathematician*, again presents Zermelo set theory plus the Axiom of Choice, and concludes

"On these foundations, I state that I can build up the whole of the mathematics of the present day; and if there is anything original in my procedure, it lies solely in the fact that, instead of being content with such a statement, I proceed to prove it in the same way as Diogenes proved the existence of motion; and my proof will become more and more complete as my treatise grows."

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11 [M8]: "an affront to common sense !"

12 "the principal merit of the formalist approach will be to have definitively dispelled the obscurities that still cloud mathematical thought"

13 "it remains to be proved, naturally, that Hilbert's conception can be realised."

14 "It appears according to very recent work that contrary to what Hilbert believed the metamathematical rules that it would be necessary to adopt in order to prove the consistency of mathematics would be of as high a degree of abstraction as the mathematical rules themselves, which much reduces the usefulness or significance of such a proof."

15 [M9]:"It seems that Hilbert's intuition had, for once, led him to slightly exaggerated hopes, and there are to-day good reasons for doubting the possibility of such [consistency] 'proofs'."

16 See [M4] and [M2] or its translation, [M3].

17 Is this the first occurrence in history of this odious phrase ?

As you might by now expect, there is no mention, or even hint in that paper of the existence of Gödel's work, which in 1948 had been in print for 17 years.

In Bourbaki's other essay, *L'architecture des mathématiques*, there is again no mention of Gödel but on this occasion there is a hint of "difficulties".

The questions I want now to address are:

> **why did Bourbaki make no mention of Gödel ?**

and

> **why did Bourbaki not notice that his system of Zermelo set theory with AC was inadequate for existing mathematics ?**

I think these questions important because the Bourbaki group have had great influence; I do not dispute the positive worth of their books nor the magnitude of their achievement; but I suggest that their attitude to logic and to set theory, which has been passed on to younger generations of mathematicians,[18] is harmful because it excludes awareness of perceptions of the nature of mathematics that are invigorating; and I almost venture to suggest that if, as some say, Bourbaki is now dead, he was killed by the sterility of his own attitudes.

Before attempting necessarily speculative answers to these questions, let us probe a little further the comments of the Bourbachistes on these matters.

Bourbaki in *L'Architecture des Mathématiques* distinguishes carefully between logical formalism, which he is against, and the axiomatic method, of which he approves:

> "What the axiomatic method sets as its essential aim, is exactly that which logical formalism by itself cannot supply, namely the profound intelligibility of mathematics."

So by the axiomatic method, he means not a grand deductive scheme for all of mathematics, but simply the mental discipline of pruning areas to their skeletons, to make similarities clear and theory portable.

> "The unity which [the axiomatic method] gives to mathematics is not the armor of formal logic, the unity of a lifeless skeleton

> "Many mathematicians have been unwilling to see in axiomatics anything else than futile logical hairsplitting not capable of fructifying any theory whatsoever.

> "Nothing is farther from the axiomatic method than a static conception of the science. We do not want to lead the reader to think that we claim to have traced out a definitive state of the science.

> "It is quite possible that the future development of mathematics may increase the number of fundamental structures, revealing the fruitfulness of new axioms or of new combinations of axioms."

André Weil puts the Bourbachist view of logic as the grammar of mathematics more diplomatically:[19]

> "*Mais, si la logique est l'hygiène du mathématicien, ce n'est pas elle qui lui fournit sa nourriture; le pain quotidien dont il vit, ce sont les grands problèmes.*"[20]

thus of course revealing a belief that there are no great problems in logic. He does, though without mentioning Gödel, go on to suggest an awareness that the last word on logic might not have been said:

> "*Il se peut sans doute qu'un jour nos successeurs désirent introduire en théorie des ensembles des modes de raisonnement que nous ne nous permettons pas.*"[21]

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[18] Lectures on set theory given to undergraduates of an ancient University in 1988 by a disciple of Bourbaki contained errors, in the form of false proofs of non-theorems, of which the spiritual ancestry may be traced to the Bourbachiste stance of forty-six years previously.

[19] in *L'Avenir des mathématiques*, [M24]

[20] "If logic is the hygiene of the mathematician, it is not his source of food: it is the great [mathematical] problems that form his daily bread."

[21] "It may well be that one day our successors will want to introduce into set theory modes of reasoning that we do not permit."

This vital view, which is reminiscent of the last paragraph quoted from Bourbaki above, is to be contrasted with the later ossification expressed by Dieudonné in his *Panorama of Mathematics*[22] that "Set theory is well worked out."

Bourbaki's general approach is stated quite clearly in his manifesto:

> "The organizing principle will be the concept of a hierarchy of structures, going from the simple to the complex, from the general to the particular.
>
> "the theory of groups, ... the theory of ordered sets, (including wellorderings), ... the theory of topological structures ..."

but it should noticed in passing that amid these unobjectionable statements is one which without further comment might mislead:

> "The first axiomatic treatments (Dedekind-Peano arithmetic, Hilbert Euclid geometry) dealt with univalent theories , i.e. theories which are entirely determined by their complete system of axioms, unlike the theory of groups."

It is true that Euclidean geometry both of two and of three dimensions as axiomatised by Hilbert are completely determined, so that a statement of plane geometry provable by use of solid geometry will have a proof in plane geometry; but Gödel tells us that arithmetic, as axiomatised by Peano or anyone else, is not; nor, curiously, is projective geometry of two dimensions, though it becomes so on the addition, as a single further axiom, of the statement of Desargues' theorem.[23] In saying that Peano arithmetic is univalent, Bourbaki probably has in mind some second-order characterisation of the standard model of arithmetic, which is, of course, to beg the question.

My reading of all these extracts is that Bourbaki had grasped the positive worth of the work of Hilbert and his school, and welcomed the idea of the reduction of the question of correctness of mathematics to a set of rules, but nevertheless persisted, even after Gödel's work showed that Hilbert's program could never be completed, in thinking of logic and set theory as stuff one settled in Volume One and then forgot about.

The later editions of Bourbaki's books shift ground so far as to mention Gödel, talk of the independence results, and give the axiom of replacement. But the pre-Gödelian attitudes, perception of which started me on this investigation, survive. Thus it appears that this major exposition of mathematics is written by people whose understanding of foundational work is that of 1929.

Turning now to my first question,

> **Why did the Bourbachistes not adapt their attitudes to take account of the supremely important contribution of Gödel to foundational issues ?**

we may well ask why the foundational understanding of Bourbaki did not advance as foundational studies advanced.

Answers may be sought at several levels, sociological, psychological or mathematical.

There may, for example, be a nationalist element in Bourbaki's posture. Compare it to Alexander Koyré's view[24] that

> "among the reasons why Hegel was ignored in France for a hundred years were the obscurity of Hegel's writing, the strength of Cartesian and Kantian philosophical traditions, Hegel's Protestantism, but above all the incredulity of the French towards Hegel's 'strict identity of logical synthesis and historical becoming'. For French rationalists, history was separate from reason or logic, which was eternal, outside time."

Examples of intellectual chauvinism are as readily found in France as elsewhere and would include the century-long resistance of the University of Paris to the ideas of Paracelsus,[25] and the resistance, under the influence of Descartes, to Leibniz' ideas concerning infinitesimals.[26]

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[22] [M10]: the work that omits the name of Shelah from a list of leading contributors to model theory. Shelah's first two books and first 322 papers are conveniently listed on pages 398–418 of [F1].

[23] For a thorough treatment of these points, see [M1].

[24] See [H4] and [H6].

[25] documented with all his customary relish in intellectual tussle by the evergreen Lord Dacre of Glanton [H2]

[26] This dispute, though, was settled comparatively quickly: see Mancosu [H5]

There were, though, in the late thirties French scholars who were well acquainted with and actively disseminating Gödel's work: see for example Albert Lautman's monograph *Les schémas de genèse* and those by Jean Cavaillès entitled *La problème du Fondement des Mathématiques* and *La non-contradiction de l'Arithmétique*,[27] so that any nationalist element in the anti-Gödelian stance may perhaps be local to the Bourbaki group.

Interestingly, Cavaillès sees the year 1929 as marking the transition between two periods, which he calls the *naïve* and the *critical*, in the development of modern logic. At a psychological level, therefore, that suggests an unwillingness of the Bourbachistes to move from the naïve conception of logic with which they had grown up, an unwillingness with which not a few European mathematicians are imbued.

The Bourbachistes' attitude to logic may derive from Poincaré's mocking attitude to the work of Cantor and Russell: though in his *Last Essays* Poincaré moved towards an understanding of his opponents and in an address, *The Moral Alliance*, delivered three weeks before his death, advocated mutual respect among those who with different ideas and methods pursue a common ideal, these conciliatory gestures may not have undone the harm caused by his earlier sardonic, savagely funny but ultimately unsound critical writings on logic.

In his Preface to the 1968 French edition of Herbrand's *Écrits Logiques*, van Heijenoort, commenting on the sad state of logic in France, remarks that the harm done by Poincaré was compounded by the early deaths of many French logicians such as Couturat, killed by a lorry in the mobilisation of 1914, Nicod, who died of tuberculosis in 1924 aged 31, Herbrand, killed mountaineering in 1931 aged 23, and Cavaillès and Lautman, who were shot, aged 41 and 36, by the Germans in 1944, for their part in the Resistance.

These last losses are part of a wider phenomenon: European logicians escaping from Hitler started schools of logic in the United States and in Israel which have flourished, leaving Europe behind.[28]

It may be that the Bourbachistes were led astray by Hilbert, whose commitment to his Programme made it at first very hard for him to accept Gödel's work: but as he recovered from the shock more rapidly than his much younger French disciples, some further explanation of their behaviour is necessary. It may be that, like many another scientist, they were prevented by their preconceptions from seeing the significance of facts that were known to them.

But whatever the reason, the fact remains that they did not accommodate Gödel's incompleteness theorems in their view of mathematics: and no sociological or psychological explanation of Bourbaki's resistance to Gödel's insights can resolve the mathematical and philosophical difficulties presented by Gödel's work to believers in Hilbert's programme.

My guess would be that at whatever level of their psyche the Bourbachistes were disabled, they were not ready to face the possibility, strongly suggested by Gödel's work, that there are *no* foundations of mathematics in the sense proposed by Hilbert and embraced by Bourbaki; that there are *no* ways of grounding mathematics in logic or classes or whatever so that once a basis has thus been given in some primitive ideas, no further thought need be given to them; that though there are indeed foundational issues, they *cannot* be confined to Chapter One of the Great Book, for they permeate mathematics.

The second question I put above was

> **"why did the Bourbaki group not notice the inadequacy of their chosen set theory as a foundation for mathematics ?"**

I suggest as an answer, that they were solely interested in areas of mathematics for which Zermelo *is* adequate, and that this area may broadly be described as geometry as opposed to arithmetic.[29]

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[27] I am grateful to Dr Mancosu of Wolfson College, Oxford, for putting me on the track of [M6] and [M14].

[28] For example, whereas students in four years in Cambridge might hear fifty lectures on logical topics, at Harvard or Princeton they may hear around two hundred and fifty, and at Berkeley, where logic is taken seriously, they may hear about four hundred.

[29] Dr Bricogne challenges my suggestion in view of the brilliantly successful cross-pollination between geometry and arithmetic found in the work of André Weil, Serre and others; nevertheless he shares Bourbaki's regrettable attitude that "it is unlikely that foundational questions might impinge directly on these areas of mathematics", and I wonder why persons so responsive to one form of cross-pollination should be so resistant to another. For recent instances of problems in algebraic geometry being solved by techniques from logic, see [F2] [F3] and [F5]

Leibniz wrote that there are two famous labyrinths in which our reason is often lost. One is the problem of freedom and necessity, and the other is concerned with continuity and infinity. Heedless of this second danger, I wish now to explore what I believe to be the underlying dualism of mathematics, namely the tension between these two primitive intuitions, the arithmetical and the geometrical.[30] This tension may be amusingly illustrated by the following conundrum:

> Can you describe a spiral staircase without moving your hands ?

That question is difficult, perhaps, because words are temporal, hence arithmetical; spirals are spatial. The source of the difficulty may be physiological in that there is a mounting body of medical evidence[31] that normally the left half of the brain handles temporal concepts while the right half handles spatial ones. [32]

Bourbaki is aware of the problem of the relationship of geometry to arithmetic, which is very ancient, and was discussed by the Eleatics, and in *The Architecture of Mathematics*, he writes:

> "Indeed, quite apart from applied mathematics, there has always existed a dualism between the origins of geometry and of arithmetic (certainly in their elementary aspects), since the latter was at the start a science of discrete magnitude, while the former has always been a science of continuous extent; these two aspects have brought about two points of view which have been in opposition to each other since the discovery of irrationals. Indeed it is exactly this discovery which defeated the first attempt to unify the science, *viz.*, the arithmetization of the Pythagoreans ("everything is number").

If we go back a century, we find Augustus de Morgan writing:

> "Geometrical reasoning and arithmetical process have each its own office; to mix the two in elementary instruction, is injurious to the proper acquisition of both."

Go back another thirteen hundred years and in the *quadrivium* of Boethius we find mathematics divided into arithmetic and geometry, music and astronomy, the second pair being the applied versions of the first pair; this therefore is also a division into two. On the other hand J.J.Sylvester, in *A probationary Lecture on Geometry*[33] delivered on 4 December, 1854, said:

> "There are three ruling ideas, three so to say, spheres of thought, which pervade the whole body of mathematical science, to some one or other of which, or to two or all of them combined, every mathematical truth admits of being referred; these are the three cardinal notions, of Number, Space and Order.
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> "Arithmetic has for its object the properties of number in the abstract. In algebra, viewed as a science of operations, order is the predominating idea. The business of geometry is with the evolution of the properties and relations of Space, or of bodies viewed as existing in space....

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[30] Dr Mancosu draws my attention to Chapter IV, *Géométrisme cartésien et arithmétisme leibnizien* of Belaval's book [H1], in which this dualism is used to interpret the opposition between Descartes and Leibniz.

[31] See [P1], [P2], [P3]. I am grateful to John Davis, Professor Emeritus of Pædiatrics at Cambridge, for drawing my attention to this research.

[32] Indeed, a friendly critic of an earlier draft of this article writes: "Which half of his brains did Bourbaki use ? My impression is, the left half. Perhaps I am projecting. The Bourbachistes were uncomfortable with the right-brain mathematics of the Italian geometers, and for good reason: significant portions were suspect and might, if one takes 'true' and 'false' to be left-brain notions and 'right' and 'wrong' to be right-brain ones, be justifiably described as right, but false.

"Rather than developing the analytical and topological tools that support the Italian mode of reasoning (Lefshetz, Hodge, *et al.*) the Bourbachistes chose the route of algebraization (Zariski, Chevalley, Weil, Grothendieck). This seems to me to be a revealing choice. In Weil's case, I wonder if he wasn't perverting his natural inclinations; I always had the impression he thought analytically, but was brilliant enough to adopt uncongenial modes of reasoning. This may be why his *Foundations of Algebraic Geometry* is generally felt to be awkward."

[33] See his *Collected Works* [M23].

"It is the province of the metaphysician to inquire into the nature of space as it exists in itself, or with relation to the human mind. The less aspiring but more satisfactory business of the geometer is to deal with space as an objective reality. ...

"But for the discovery of the conic sections, attributed to Plato, the law of universal gravitation might never to this hour have been elicited. Little could Plato himself have imagined that he was writing the grammar of the language in which it would be demonstrated in after ages that the pages of the universe are written.

"He who would know what geometry is, must venture boldly into its depths and learn to think and feel as a geometer."

But Sylvester's three divisions might be reduced to two by regarding Order as superstructure of the other two, and one might wonder whether a further, and final, reduction is possible.[34] I would speculate, though, that the physiological separation by the brain of the processing of spatial from the processing of temporal thought supports the thesis that a complete unification of mathematics is not possible.

Let us therefore consider these two intuitions, the arithmetical and the geometrical.

The two intuitions are not disjoint: the language of each is sufficiently rich to allow translations of the other: within set theory one can do a mock-up of the real line by building first the rationals and then (say) Dedekind cuts; and one can mark out equally spaced points as integral points along a line; but when such translations are made, paradoxes are prone to result, since the translations are of formal properties, not of underlying intuitions.

Thus the Pythagoreans wished to believe that all is number, but were dismayed by the demonstration that the diagonal of a square is incommensurable with its side. Here, importing a simple geometric construction generated an arithmetical paradox.

Stifel (1487 - 1567) asked what irrationals are: geometry suggests they are acceptable, but as lengths, not numbers. He wrote, "an irrational is not a real number because it lies under some cloud of infinity." He did not believe in $\sqrt{2}$.

In the other direction there is the Banach Tarski paradox that a sphere can be decomposed into finitely many parts which can be rearranged by spatial translations and rotations to form two spheres of the same size as the original one. The proof of this is derived from the Schröder–Bernstein argument, coupled with the axiom of choice (in the absence of which the Banach–Tarski theorem might fail).

Here arguments that are natural in a set theoretic context lead to conclusions that are paradoxical geometrically. This is similar in spirit to the result of Fibonacci in the thirteenth century that the solution of a certain cubic is not one of Euclid's irrationals.

The attitude taken by Bourbaki to the issue of geometry *versus* arithmetic is still relevant today, for recently the distinguished American mathematician Saunders MacLane has called for a revival of discussion of the philosophy of mathematics and has criticised what he calls the Grand Set Theoretic Foundation of Mathematics in phrases such as

"the Grand Set Theoretic Foundation is a mistakenly one-sided view of mathematics; set theory is largely irrelevant to the practice of most mathematics;

[34] On this topic the same friendly critic writes: "Freeman Dyson expatiates on the subject of unifiers and diversifiers in Chapter 3 of his book [F4]. Unifiers revel in unity, diversifiers in diversity. I have long believed that mathematicians tend naturally to be unifiers. On the other hand I believe set theorists with a strong interest in forcing almost never are.

"I think in the case of Bourbaki, Dyson's distinction is more vital than yours. Bourbaki was interested in unity above all. Some degree of unification can be achieved by laying out measure theory carefully, others by converting an analytical theory into an algebraic one, thereby extending its breadth of application and capturing more cases simultaneously. It does seem that algebra is the workhorse of the unifier. Bourbaki tries to make plain the combinatorial content, in a certain limited sense, of those branches of mathematics which were 'ripe' for this treatment."

How, I wonder, does Dyson's distinction differ from that made between creation and consolidation in the opening paragraph of this essay ?

> "logicism, formalism and Platonism have been too much dominated by the notions of set theory and deductive rigour."

There have also been criticisms such as that of Thom:

> "set theory seems to suppress geometry."

and, before all those, the delicious *Schlußbemerkung* of Skolem's 1922 paper, which runs, roughly translated, thus:

> "The most important result above is the relativity of the concepts of set theory. I mentioned this orally to Professor F Bernstein in Gottingen in the winter of 1915/16. There are two reasons why I have not published anything about it sooner: first, I have been occupied with other matters since then; the second is that I thought it so clear that this axiomatic set theory was unsatisfying as a final foundation for mathematics that the majority of mathematicians would not bother themselves about it. Recently I have noticed, to my astonishment, that very many mathematicians regard set theory as the ideal foundation of mathematics; it therefore seems to me that the time is ripe for the publication of a critique."[35]

I suggest that it is because Bourbaki fossilised mathematicians' knowledge of logic at its 1929 level[36] that this attack is now being renewed. MacLane, who, having been a pupil of Bernays in Göttingen from 1930 to 1933, has a much stronger grasp of logic than have the Bourbachistes, is, in other words, attacking a position from which logicians have been moving for the past sixty years but which mathematicians are still at.

This is not the place to begin a full discussion of the strengths and weaknesses of MacLane's foundational views set out in his book *Mathematics: Form and Function*, but some brief remarks are in order.

Against Skolem I would hold that there is no final foundation for mathematics, but that set theory captures a substantial side of mathematics. I would agree with MacLane's first comment and with Thom's, and relate them to my idea that set theory is on the arithmetical rather than the geometrical side of mathematics. I would qualify MacLane's second comment, by saying set theory is not particularly relevant to the practice of geometry, but is very much relevant to arithmetic in its broadest sense.

Though I agree with much of MacLane's third criticism, I question his use of the phrase *set theory and deductive rigour*. He thinks of these as hand-in-hand, and objects to the pair of them masquerading as the final solution for mathematics. I would want to separate the two. Logic is the study of our use of language:[37] set theory is the study of well-foundedness, and not, as MacLane thinks, the study of the process of set formation.

That is the great difference between Zermelo–Fraenkel and Zermelo. Zermelo — more particularly the subsystem of it one may call MacLane set theory in view of MacLane's support for it in his books and articles — is a system to support *set formation*, and is adequate for geometrical considerations; Zermelo–Fraenkel is a system that contains in addition support for *definitions by recursion*, that is, building structures into the unknown. This element, which is the focal point of Kripke–Platek set theory, is suited to the arithmetical side of mathematics.

In Zermelo set theory, one cannot prove that every well-ordering is isomorphic to a von Neumann ordinal; one cannot prove the existence of the von Neumann ordinal $\omega + \omega$, though one can prove the existence and well-foundedness of linear orderings of that order-type; one cannot justify recursion on ordinals or on arbitrary well-founded relations. Thus induction, which is at the heart of arithmetic, is missing from (large parts of) geometry. On the other hand, spatial intuition is missing from arithmetic; so we need both.

The *geometrical* conception of the integers as equally spaced points on a line suggests that all natural numbers are on an equal footing; in Russellian terms they are of the same type. In the *arithmetical* conception, 0 is the simplest natural number, and larger positive numbers are generated from, and are therefore

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35 See [M22].

36 Indeed, according to legend, a member of the Bourbaki group said, in a lecture given at Princeton to an audience that included Gödel, that nothing had happened in logic since Aristotle. Can any reader tell me who ?

37 This statement would be hotly contested by many; but the contest would only reinforce my point that logic is not the same as set theory.

more complicated than, smaller ones, so that no two natural numbers are of the same type.[38] Violence is done to each of these intuitions by trying to subordinate it to the other: and we should perhaps seek a philosophy of mathematics that allows the two to thrive in healthy interaction.

The set theory that MacLane proposes in his book[39] as a basis for mathematics is a subsystem of Zermelo set theory plus the Axiom of Choice. His proposals therefore do nothing to answer the criticisms made here, that Bourbaki presents a pre-Gödelian view of mathematics, and of a portion of mathematics biassed towards geometry at that.

Let me end on a positive note by recalling a quotation from Jean Dieudonné:

> "We have not begun to understand the relationship between combinatorics and conceptual mathematics."

and suggesting that both the philosophy for which MacLane calls and the understanding which Dieudonné seeks will emerge from a renewed study of the interplay between arithmetic and geometry.

Bibliography

MATHEMATICAL WORKS

[M1] K. Borsuk and W. Szmielew, *Foundations of Geometry*, North Holland, 1960, pp 277, 435.

[M2] N. Bourbaki, L'Architecture des Mathématiques, in [M15], pp 35–47

[M3] N. Bourbaki, The Architecture of Mathematics. *American Mathematical Monthly* **57** (1950, 221–232

[M4] N. Bourbaki, Foundations of Mathematics for the Working Mathematician, *Journal of Symbolic Logic* **14** (1948) pp 1–14.

[M5] H. Cartan, Sur le Fondement logique des Mathématiques, *Revue Scientifique* **81** (1943), 3–11.

[M6] J. Cavaillès, Volumes 608 and 610 of the series *Actualités scientifiques et industrielles: le Progrès de l'Esprit*, Hermann, Paris 1938. See also *Sur la Logique et la Théorie de la Science*, Presses Universitaires de France, 1947.

[M7] C. Chevalley, *Mathematical Intelligencer* **7** (1985), (2) p 18; see also *Mathematical Intelligencer* **8** (1986), (2), p 5.

[M8] J. Dieudonné, Les Méthodes Axiomatiques Modernes et les Fondements des Mathématiques, *Revue Scientifique* **77** (1939), pp 224–232.

[M9] J. Dieudonné, David Hilbert, in [M15], pp 291–297.

[M10] J. Dieudonné, *A Panorama of Pure Mathematics*, Academic Press, New York 1982.

[M11] S. Feferman, Hilbert's program relativized: Proof-theoretical and foundational reductions, *Journal of Symbolic Logic* **53** (1988) pp 364–384.

[M12] D. Hilbert and W. Ackermann, *Grundzüge der Mathematik*, Springer Verlag, Berlin, 1928.

[M13] D. Hilbert, Gesammelte Abhandlungen, 3. Band, 378-387, Berlin 1935; reprinted Chelsea, New York 1965.

[M14] A. Lautman, Volume 591 of the series *Actualités scientifiques et industrielles: le Progrès de l'Esprit*, Hermann, Paris 1938. See also his *Collected Works*, 1977

[M15] F. le Lionnais (ed.), *Les Grands Courants de la Pensée Mathématique*, Cahiers du Sud, 1948; reviewed by S.MacLane in *Mathematical Reviews* **10**, 230.

[M16] S. MacLane, *Mathematics: Form and Function*, Springer Verlag, New York 1986.

[M17] A. R. D. Mathias, Notes on MacLane Set Theory, submitted to the *Mathematical Proceedings of the Cambridge Philosophical Society.*

[M18] W. Sieg, Hilbert's program sixty years later, *Journal of Symbolic Logic* **53** (1988) 338-348

[M19] W. Sierpiński, *Leçons sur les nombres transfinis*, Collection Borel, Gauthier-Villars, Paris, 1928.

[M20] S. Simpson, Partial Realisations of Hilbert's Programme, *Journal of Symbolic Logic* **53** (1988) 349–363

[M21] S. Simpson, Ordinal Numbers and the Hilbert Basis Theorem, *Journal of Symbolic Logic* **53** (1988) 961-974

[38] This *succession* of 'arithmetical' integers is beautifully captured by von Neumann's definition of *ordinal number*; for this reason I cannot accept Professor MacLane's view that von Neumann's definition is merely a 'gimmick'.

[39] For a discussion of the logical status of the system of [M16] see [M17].

[M22] Th. Skolem, Einige Bermerkungen zur axiomatischen Begründung der Mengenlehre, to be found in *Mengenlehre*, an anthology of papers written since 1874 on the mathematical, metamathematical and philosophical aspects of set theory, selected and introduced by Ulrich Felgner, Wissenschaftliche Buchgesellschaft, Darmstadt, 1979.

[M23] J. J. Sylvester, *Collected Works*, Volume II, p 5, Cambridge 1908; reprinted Chelsea, New York, 1973.

[M24] A. Weil, *L'Avenir des mathématiques*, in [M15], 307-320; also in his *Collected Works*, 1947a.

Historical Works

[H1] Y. Belaval, *Leibniz critique de Descartes*, Gallimard, Paris, 1960, 1978.

[H2] H. R. Trevor-Roper, *Baron Dacre of Glanton*, The Paracelsian Movement, in *Renaissance Essays*, Secker and Warburg 1985.

[H3] G. Ferrières, *Jean Cavaillès: un philosophe dans la guerre, 1903-1944,* Éditions du Seuil, Paris, 1982.

[H4] A. Koyré, Rapport sur l'état des études hegeliennes en France, *Revue d'histoire de la philosophie*, **5**:2 (April-June 1931) p 147.

[H5] P. Mancosu, The Metaphysics of the Calculus: a Foundational Debate in the Paris Academy of Sciences, 1700–1706: to appear in *Historia Mathematica* **16** (1989).

[H6] M. Poster, *Existential Marxism in Postwar France from Sartre to Althusser*, Princeton University Press, 1975

Physiological Works

[P1] M. Annett, *Left, Right, Hand and Brain: the Right Shift Theory*, Erlbaum, New Jersey, 1985

[P2] A. Beaton, *Left Side, Right Side: a Review of Laterality Research*, Batsford 1985

[P3] S. P. Springer and G. Deutsch,*Left Brain, Right Brain*, W.H.Freeman, New York, 1981, 1985

Further Reading

[F1] J.T. Baldwin (editor) *Classification Theory,* Springer Lecture Notes in Mathematics **1292**.

[F2] J. Denef, p-adic semi-algebraic sets and cell decomposition *Journal für die reine und angewandte Mathematik* **369** (1986) 154–166.

[F3] J. Denef and L. van den Dries: p-adic and real subanalytic sets, *Annals of Mathematics* **128** (1988) 79–138.

[F4] F. Dyson, *Infinite in all directions,* Harper and Row 1988.

[F5] A. MacIntyre, On definable subsets of p-adic fields *Journal of Symbolic Logic,* **41** (1976) 605–610.