Hilbert, Bourbaki and the scorning of logic

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In memoriam
Brian Wormald et Maurice Cowling,
Domus Divi Petri apud Cantabrigienses sociorum,
auctoris olim collegarum amicorumque,
virorum et humanitate et doctrina praestantium,
hoc opus grato animo dedicat auctor.

THE ARGUMENT

Those who wish to teach in the *lycées* and *collèges* of France must obtain a *Certificat d'Aptitude au Professorat de l'Enseignement du Second Degré*, commonly called the CAPES. For that certificate, the first hurdle to be passed is a written examination. In at least ten recent years the syllabus for the mathematics section of that examination, as specified in the relevant special numbers of the *Bulletin Officiel*, has contained the line

Tout exposé de logique formelle est exclu.

That remarkable ban, still in force though latterly softened to *"Aucun exposé de logique formelle n'est envisagé"*, is a sign in the context of teacher-training of a more widely spread phenomenon. My main point will be pedagogical, that the teaching of logic, in France and elsewhere, at all academic levels, has over several decades been hampered, not to say crippled, by policies stemming from the lack, among members of the Bourbaki group, of interest in and understanding of the foundations of mathematics; to put it starkly,

1. widely-read texts contain falsehoods and misconceptions about logic;
2. the prestige of their authors means that these errors are meekly accepted;
3. correct teaching of logic is thereby blocked.

Bourbaki in many things took Hilbert as their model, and it appears that their difficulties with logic are rooted in Hilbert's pre-Gödelian misconception of the relationship of truth to consistency. The story that I shall tell is this:

Sections C, D, and E may be regarded as case studies, containing detailed criticisms of the logic portions of two algebra text books of the Bourbaki school and of various pronouncements on logic by Bourbaki's leading spokesman. The reader who wishes to defer reading such details should perhaps first read Sections A and B and then pass directly to Sections F, G, and H.

A: *Hilbert in 1922 proposes an alternative treatment of predicate logic ...*

THE CURTAIN RISES in Göttingen on 29·xii·1899,* to reveal Hilbert[1] writing to Frege[2] that

> *if the arbitrarily given axioms do not contradict each other with all their consequences, then they are true and the things defined by the axioms exist. This is for me the criterion of truth and existence.*[1]

A·1 It is hard to judge at this distance what Hilbert would have made of the many independence results found by set theorists in the later twentieth century. Though he knew very well that there are mutually inconsistent systems of geometry, he seems not to have considered that there might be two mutually contradictory systems of set theory, or even of arithmetic, each in itself consistent, so that the objects defined by the two sets of axioms cannot co-exist in the same mathematical universe.

Let us give some examples from set theory. Suppose we accept the system ZFC. Consider the following pairs of existential statements that might be added to it.

[A_1] the real number $0^\sharp$ exists.
[B_1] there are two Σ^1_1 sets of reals, neither of them a Borel set, and neither reducible to the other by a Borel isomorphism.

[A_2] there is a measurable cardinal.
[B_2] there is an undetermined analytic game.

[A_3] there is a supercompact cardinal.
[B_3] there is a projective well ordering of the continuum.

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* To place the events of 1922, and their consequences for our enquiry, in context, we must mention some of the milestones in the development of logic and set theory in the early twentieth century. A complete account would be impossible, as each of the giants involved stood on the shoulders of other, earlier, giants; I ask the reader, and the historians of science whom I have consulted, to forgive the crudities of my perforce simplified narrative.

[1] David Hilbert, 1862–1943; Ph. D. Königsberg 1885; there till his move to Göttingen in 1895.

[2] Gottlob Frege, 1848–1925; Ph. D. Göttingen 1873; in Jena from 1874. According to [OH], a reluctant *conferencier* but an indefatigable correspondent.

1 I quote the translation given in Kennedy [Ke], who there coins the apt phrase "Hilbert's Principle".

For each i, the statement A_i refutes the statement B_i in ZFC; but so far as is known each of the statements may consistently be added to ZFC, though some of them are stronger than others.

So Hilbert's Principle, that consistency is a ground for existence, should be taken to mean that a consistent theory describes *something.*

A·2 In 1900, at the Paris International Congress of Mathematicians, Hilbert said

> "This conviction of the solubility 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 ignorabimus.*"

A·3 On 16·vi·1902, Russell wrote to Frege to communicate his discovery, in 1901, of a contradiction in Frege's theory of classes; Frege acknowledged the contradiction in the second volume of his *Grundgesetze der Arithmetik*, published in 1903; on 7·xi·1903 Hilbert wrote to Frege to say that such paradoxes were already known in Göttingen, discovered by Hilbert and by Zermelo[3],[2] and that

> "they led me to the conviction that traditional logic is inadequate and that the theory of concept-formation needs to be sharpened and refined."

A·4 On 18·ix·1904, Zermelo wrote to Hilbert to communicate his first proof that every set can be well ordered; his letter was published as [Ze1]. In response to criticisms of his proof, Zermelo in 1908 published in [Ze2] a re-working of it and in [Ze3] a proposal for a system of axioms for basing mathematics on set theory, though it should be remembered that as the process of formalising such systems was still in its infancy, Zermelo had to leave undefined the concept of *definite Eigenschaft* invoked in his Separation scheme.

A·5 In lectures at Göttingen in 1905, Hilbert, after discussing set theory and the paradoxes, said[3]

> "The paradoxes we have just introduced show sufficiently that an examination and redevelopment of the foundations of mathematics and logic is urgently necessary."

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3 Ernst Zermelo, 1871–1953; Ph. D. Berlin 1894; in Göttingen 1897–1910, Zürich 1910–1916, (retirement, with a Cantonal pension, forced by ill-health); honorary Professor, Freiburg 1926–35 and 1946–53.

2 See [RaTh] and, for a more recent discussion, [Pec].

3 as translated in [Za1], page 333.

A·6 But from 1905 for twelve years or so Hilbert was absorbed in other projects, such as his work on the axiomatization of physics, described in [Cor4], and his work on integral equations. To illustrate the breadth of his interests we quote three trenchant remarks from various periods of his life.

On the eternally shifting balance between geometry and arithmetic: Weierstrass thought all reduced to number;[4] but Hilbert countered by saying in his lectures, in Königsberg in 1891, that "Geometry deals with the properties of space, [perception of which comes to us] through the senses."[5]

Hilbert's remark, in lecture notes of 1894,[6] that "Geometry is a science whose essentials are developed to such a degree that all its facts can already be logically deduced from earlier ones. Much different is the case with the theory of electricity" suggests that to him axiomatisation is the last stage of a process which begins with the amassing of data and ideas.

Hilbert's enthusiasm for physics is evident: "Newtonian attraction turned into a property of the world-geometry, and the Pythagorean theorem into a special approximated consequence of a physical law."[7]

1917: Hilbert returns to the foundations of mathematics

A·7 Fast forward now to Göttingen in the Winter Semester of 1917, to discover Hilbert giving a course of lectures on the foundations of mathematics.

That is in itself noteworthy. Many modern mathematicians of distinction would decline to devote time and effort to the foundations of their subject; but plainly Hilbert would not waste his time on things he judged unimportant. In his earlier days he had been thrilled to adopt the set-theoretical ideas of Cantor as a framework for mathematics; and his much-quoted remark of 1926 that no one shall drive us from the paradise that Cantor created, though directed against the intuitionist and constructivist reactions of Brouwer[4] and Weyl[5], of 1910 and 1918 respectively, may perhaps be seen as an affirmation that the foundations of classical mathematics have been re-built and rendered impregnable after the set-back of the discovery of paradoxes in Frege's detailed treatment of the theory of classes.

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4 [Cor5], p 168.

5 [Cor5], p 156-7.

6 translated in [Cor3], page 257.

7 In *Wissen und mathematisches Denken,* 1922-3, translated in [Cor5], p 173.

4 Luitzen Egbertus Jan Brouwer, 1881–1966; Ph.D. 1907, Amsterdam; extr. professor, Amsterdam 1912–1951.

5 Hermann Klaus Hugo Weyl, 1885–1955; Ph.D. Göttingen, 1908; there till 1913 and 1930–33; Professor, ETH, Zürich 1913–1930; IAS Princeton, 1933-52

As Hilbert's course progressed, typewritten notes of his lectures were prepared with scrupulous care by his assistant, Bernays[6]; these notes are preserved in the archives of Göttingen, and, some years later, were used by Ackermann,[7] a pupil of Hilbert, in preparing the text [HiA], to which we return below, for its publication in 1928. Their continued influence can perhaps be detected in the first volume of the much larger, two-volume, treatise [HiB1,2], discussed towards the end of this section.

The notes of 1917/18 then gradually disappeared from view, until in recent years Sieg and his collaborators Ewald, Hallett and Majer have started work on their re-appraisal.[8]

The reader of these lecture notes[9] will notice, underneath the period style, the modernity of the conception of logic that is being expounded, though of course many results, such as the completeness theorem for predicate logic, had not yet been proved and still had the status of open questions; and other results, particularly the incompleteness theorems for systems of mathematics, were undreamt of.

Some terminology

Indeed, the notion of a formal system of mathematics had made great strides since 1905, and it will be helpful, without going into either detail or history, to review some of the vocabulary of modern logic. We suppose that we have already specified an appropriate formal language (which means specifying the symbols of the language and the rules of formation of its formulæ), and that we have specified the underlying logic and rules of inference. A *sentence* of such a language is a formula without free variables. A *theory* in such a language will then be specified by choosing the sentences that are to be its axioms.

Four possible properties of such a theory are consistency, syntactic completeness, semantic completeness and decidability.

A·8 Definition A theory is *consistent* if no contradiction can be derived in it; a consistent theory is said to be *syntactically complete* if no further

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[6] Paul Bernays, 1888–1977; Ph. D. Berlin 1912; Zürich 1912–17 (Habil. 1912; assistant to Zermelo) and from 1934; Göttingen 1917–33, (Habil. 1918, Extraordinary Professor from 1922).

[7] Wilhelm Ackermann, 1896–1962; Ph.D. Göttingen 1925; from 1929 schoolmaster at Burgsteinfurt & Lüdenscheid.

[8] A critical edition [ES] of these notes is being prepared (2009) by Ewald and Sieg for publication; pending their publication, Sieg's paper [Si1] may be consulted for much historical and mathematical detail.

[9] I am grateful to Professor Sieg for placing a copy at my disposal.

axioms can be added without an inconsistency resulting. Thus in a syntactically complete theory every sentence of its language is either provable or refutable.

The above properties of a theory can be understood knowing only its axioms and rules of inference without knowing anything about its intended interpretations.

The semantic counterparts to those properties are defined in terms of the intended interpretations of the theory in question, which must therefore be specified; often it is enough to consider interpretations in finite non-empty domains and in a countably infinite domain.

A·9 DEFINITION A sentence of the theory is *universally valid* if true in all its intended interpretations. A theory is *sound* if all its theorems are universally valid. The theory is *semantically complete* if its every universally valid sentence is a theorem. Put another way, if a sentence is irrefutable it is true in at least one of the theory's intended interpretations.

The above are thus properties of the theory relative to its specified family of intended interpretations.

The *completeness theorem* of Gödel, proved in his dissertation of 1929 and published in 1930, says that every consistent first-order theory in a countable language has a countably infinite model (or possibly a finite one, if one has undertaken to interpret the equality predicate = as identity.)

Thus if all such models are counted as intended interpretations, the theory is semantically complete; and the completeness theorem for such theories yields Hilbert's Principle.

A·10 EXAMPLE The theory of non-empty endless dense linear orderings. There are no finite models of this theory, and Cantor proved that any denumerable such must be isomorphic to $\mathbb{Q}$, the set of rational numbers with its usual ordering; so the theorems of this theory are precisely the sentences in the language of linear orderings true in $\mathbb{Q}$; as every sentence is either true or false in that model, the theory is syntactically complete.

A·11 REMARK The reader should be warned that "complete" was often used by Hilbert to mean "covers everything known so far"; so a theory thought to be complete in his sense today might be seen as incomplete tomorrow.

A·12 DEFINITION A theory is *decidable* if there is an algorithm which given any sentence of the theory will decide in finite time whether or not it is a theorem. For classical propositional logic, such an algorithm exists; but that is not true for classical first order predicate logic.

1928: publication of the treatise of Hilbert and Ackermann

We mention this text here as it belongs to a stage in the development of logic rather earlier than its date of publication: Hilbert in his foreword, dated 16·i·1928, states that the sources used are the 1917/18 notes, together with notes on courses given in the winter semesters of 1920 and 1921/2. The delay in its publication may perhaps be attributed to Hilbert's absorption in the ε-operator that he defined in 1922: for he remarks that the book should serve as preparation and lightening of a further book that he and Bernays wish to publish soon which will treat the foundations of mathematics using the epsilon symbol.

The contents of the book are well summarised in [Bu]; there is a thorough treatment of propositional logic in Chapter One, using essentially the axioms given in *Principia Mathematica*; simplifications due to Bernays are used in the axiomatisation of predicate logic given in Chapter III, and its consistency and syntactic incompleteness proved; but the rest of the book largely consists of examples, as the semantic completeness of predicate logic had not yet been proved. Though that form of completeness is indeed mentioned as an open problem, Hilbert's Principle would seem to have been an article of faith: Hilbert thought in 1919 that "things cannot be otherwise";**[10]** true of a complete theory, but not of an incomplete one, which might have more than one completion.

Logic in the twenties

The early years of the twentieth century were a time of intense activity in foundational research, and a sense of the variety of proposals for predicate logic may be obtained from Goldfarb's 1979 paper [Gol].**[11]**

Besides those of Hilbert, Goldfarb discusses the accounts of predicate logic offered by Frege (1882, 1892), Russell (1903, 1919), Schröder (1895), Löwenheim (1915), Skolem[8] (1920), (1922), Herbrand[9] (1928, 1930), and Gödel[10] (1930). He sees the twenties as a period in which the ideas of two schools of logic originating in the nineteenth century:

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10 [Cor5], page 156.

11 Goldfarb was writing without knowledge of the 1917/18 notes of Hilbert, and therefore, as shown by Sieg in [Si 1], his chronology in places requires correction.

8 Thoralf Skolem, 1887–1963; Göttingen 1915; Oslo 1916–1930 (late Ph. D. 1926) and from 1938; Bergen 1930–38.

9 Jacques Herbrand, 1908–1931; Ph. D. Sorbonne, 1930; visited Berlin, Hamburg, Göttingen 1931; killed climbing.

10 Kurt Gödel, 1906–1978; in Vienna from '24 (Ph.D. '30, Habil. '32); visited US '33, '35; IAS, Princeton from '39.

1) the algebraists : de Morgan (1806–71), Boole (1815–64), Peirce (1839–1914), Schröder (1841–1902), Löwenheim (1878–1957);
2) the logicists : Frege, Peano (1858–1932), Russell (1872–1970).

merged to yield the modern theory of quantification. He writes:

> *The deficiencies in the two early traditions I have been discussing may be summarized thus. To arrive at metamathematics from Russell's approach we must add the "meta", that is, the possibility of examining logical systems from an external stand point. To arrive at metamathematics from the algebra of logic we must add the "mathematics", that is, an accurate appreciation of how the system may be used to encode mathematics, and hence of how our metasystematic analyses can be taken to be about mathematics.*

One might add that the idea, so well-established today, that one might wish to interpret a formula in many different structures, came more easily to the algebraists than to the logicists. The two schools worked in partial knowledge of each other's efforts;[12] the discovery of the *quantifier* is generally attributed to Frege (1879) but was made independently and slightly later ([Mi], 1883) by O. H. Mitchell, a student of Peirce, who further developed the idea in a paper [Pei] of 1885.[13]

1922: the Hilbert operator is launched

We turn to the proposal made by Hilbert in 1922; not because of any alleged superiority to the other contemporary accounts of logic but because that was the one adopted by Bourbaki; and indeed the conclusion to which we shall come is that Bourbaki backed the wrong horse.

Hilbert's 1922 proposal was based on what is often called the Hilbert ε-operator, by which quantifiers could formally be avoided and predicate logic reduced to propositional.** The apparent simplicity of this proposal commended it to the members of Bourbaki, who made it, though in a different notation, the basis of their Volume One, on the theory of sets, and developed it as the logical basis of their series of books. We review its history now and defer to Section B a discussion of its demerits.

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12 For a portrait of the mutual non-admiration of Peirce and Russell, see the paper [An].

13 A helpful introduction to the early history of the quantifier is the paper [Pu].

** But *something* is being concealed, as propositional logic is decidable whereas predicate logic is not.

τ versus ε:[14]

A·13 In a lecture [Hi1][15] given in September 1922, Hilbert, describing joint work with Bernays, introduced what he called a logical function which he wrote as $\tau(\mathfrak{A})$ or $\tau_a(\mathfrak{A}(a))$, which associates to each one-place predicate $\mathfrak{A}(a)$ an object $\tau(\mathfrak{A})$. He gave its intended meaning in his Axiom 11, (which he credits to Bernays)

$$\mathfrak{A}(\tau\mathfrak{A}) \implies \mathfrak{A}(a)$$

and illustrated its use by saying that if $\mathfrak{A}$ were the predicate "is bribable", then $\tau\mathfrak{A}$ would be understood to denote a man of such unassailable uprightness that were *he* bribable then must all mankind be too.

Hilbert then defines the quantifiers in terms of his operator.

Using his operator Hilbert went on to sketch a proof of the consistency of a weak version of arithmetic with a single function symbol ϕ defined by recursion equations not involving the symbol τ. His idea was, roughly, a priority argument: *start by assigning* 0 *as the value to all τ-terms; redefine whenever a contradiction is reached; end by showing that you cannot have* $0 \neq 0$.

Hilbert plainly intended his operator to be the lynchpin of the new proof theory that he and his collaborator Bernays had set themselves to develop; he apologises for lacking the space in which to give all the details, but is evidently confident that their new theory will be able to dispel all the recent doubts about the certainty of mathematics.

A·14 In his inaugural dissertation, [Ack1][16], Ackermann reworked Hilbert's proof with greater care, found he needed to restrict the system yet further for the proof to work, and worked with the dual operator, which supplies a witness to an existential statement rather than a counter-example to a universal one; he named that operator ε, not τ. Thus his corresponding axiom reads

$$\mathfrak{A}(a) \implies \mathfrak{A}(\varepsilon_a\mathfrak{A}(a)).$$

This change of letter and operator was thenceforth adopted, except by Bourbaki who followed the change of operator without changing the letter.

In [vN],[17] von Neumann[11] criticised Ackermann's paper and gave a

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[14] Much of this subsection has been gleaned from van Heijenoort's anthology [vHe]. For vastly improved detail, see [Zac2].

[15] manuscript received by *Mathematische Annalen* on 29.ix.1922 and published the following year.

[16] manuscript received 30.iii.1924 and published that year.

[17] manuscript received 29.vii.1925 and published in 1927.

[11] Johann von Neumann, (1903-1957); Göttingen 1926/7 then Berlin; Hamburg 1929/30, thereafter in Princeton.

consistency proof for first order number theory with induction for quantifier free formulæ. Hilbert in 1927 gave a lecture [Hi3], outlining Ackermann's paper and the method of "assign values then change your mind".

1928: Hilbert at Bologna

In his address [Hi4] on 3·ix·1928, to the International Congress of Mathematicians at Bologna,♣ Hilbert reiterated his belief in the consistency, completeness and decidability of mathematics. He remarks that as mathematics is needed as the foundation of all the sciences, it is incumbent on mathematicians to secure its foundations. He hints at Skolem and Fraenkel having completed the axiomatisation of Zermelo; he mentions the ε-axiom, in Ackermann's notation, and the work of Ackermann and von Neumann's work on ε, which he seems to think has established the consistency of arithmetic; and he discusses four problems.

The first is to extend the proof of consistency of his ε-axioms to a wider class of formulæ; the second is to establish the consistency of a global, extensional, form of choice as expressed through his symbol; the third is to establish the completeness of axiom systems for arithmetic and for analysis; and the fourth is to establish the completeness of predicate logic: is everything that is always true a theorem ?

In closing he remarks that we need mathematics to be absolutely true, otherwise *Okkultismus* might result; and he repeats his belief that *in der Mathematik gibt es kein Ignorabimus.*

♣ with Henri Cartan present, but not Gödel, who spent that summer in Brno reading *Principia Mathematica.*

1928: the war of the Frogs and the Mice

Brouwer gave two lectures in Vienna early in 1928 which according to an entry in Carnap's diary stimulated the young Gödel. Though Hilbert had helped Brouwer in his early career, their relationship had soured; personal tensions mounted in 1928, when Hilbert campaigned for and Brouwer against German participation in the Bologna Congress[18]; and matters came to a head in October 1928.[19] Even though Einstein chaffed Hilbert with the phrase *Froschmaüsekrieg*, in the hope of restoring calm, the outcome was the expulsion of Brouwer, after thirteen years' service, from the Editorial Board of the *Mathematische Annalen*.

Brouwer retaliated by launching the journal *Compositio Mathematica*.

1929: the completeness theorem

The fourth of Hilbert's Bologna problems was solved affirmatively the following year by Gödel in his doctoral dissertation.[20] It would seem that Skolem had earlier come close to a proof, and that to some extent his modesty has obscured the chronicle:♠ Gödel in 1964 wrote that Skolem in his 1922 paper [Sk] proved (but did not clearly state) the result that if a formula is not a theorem its negation is satisfiable. Syntactic completeness might be finitistic: one could imagine an algorithm which given a non-theorem finds a proof of its negation. But semantic completeness is not: as the models in which formulæ are to be tested are countable but perhaps not finite, an infinite sequence of admittedly finitistic steps is needed to build one. Gödel thought that Skolem had the steps but not the sequence. Goldfarb [Gol] writes:

> *"Gödel's doctoral dissertation and its shorter published version . . . is a fitting conclusion to the logic of the twenties. [. . .] Although Gödel [worked] independently, the mathematics is not new: it was substantially present in the work of both Skolem and Herbrand. What is new is the absolute clarity Gödel brings to the discussion."*

For a more recent study that also conveys very clearly the impact of Gödel's dissertation, see [Ke].

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[18] [Seg2], pp 352, 354.

[19] [vDa1; vDa2, Volume II]

[20] All cited papers and correspondence of Gödel will be found in the Collected Works [Gö].

♠ as has Bernays' modesty, as shown by Zach [Za1], the history of propositional logic.

1930/31: the incompleteness theorems

The third of Hilbert's Bologna problems was solved negatively by the two incompleteness theorems of Gödel. It is tempting to speculate that Hilbert was initially misled by his success in giving a formal treatment of Euclidean plane geometry into thinking that the corresponding foundational problem posed by arithmetic would prove to be similar, so that the consistency and completeness of mathematics could be established by finitistic means. Goldfarb [Gol] highlights that misconception:

> *" By the end of the decade the Hilbert school was quite certain that they had in all essentials a [consistency] proof for full number theory. Gödel's Second Incompleteness Theorem came as a terrible shock."*

1931/34: Hilbert's delayed response to the incompleteness theorems

None perhaps was more shocked than Hilbert. A recent paper of Sieg [Si4] documents the strange contrast between the admirably enthusiastic and generous response of von Neumann to Gödel's results and the rather less admirable state of denial that was Hilbert's initial response.

Hilbert and Gödel were at two different meetings in Königsberg in East Prussia in September 1930. On September 7th, at a round-table discussion, moderated by Hans Hahn and attended by von Neumann and Carnap among others, which discussion formed part of the second *Tagung für Erkenntnislehre der exakten Wissenschaften*, Gödel announced his first incompleteness theorem. Gödel and von Neumann discussed this result immediately after the session, and then by mid-November each of them had independently found the second incompleteness theorem.

On September 8th, Hilbert gave his famous lecture *Naturerkennen und Logik* to a meeting of the *Gesellschaft Deutscher Naturforscher und Ärzte.*

Gödel left Königsberg on September 9th, without, it seems, having discussed his new result with Hilbert.[21]

I follow Sieg in thinking that it is implausible that von Neumann should not have spoken to Hilbert about it, for given that Hilbert had said at Bologna that von Neumann and Ackermann had a proof of the consistency of arithmetic, then undoubtedly von Neumann, once its impossibility was clear to him, would immediately have disabused him of this idea.

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[21] In a letter cited in footnote 4 of [Daw], Gödel states that he neither met nor ever corresponded with Hilbert.

The strange thing is that Hilbert published two papers [Hi5] and [Hi6], on foundational themes after Gödel's announcement without explicitly mentioning Gödel's work. Both were published in 1931; [Hi5] is the text of a lecture given to the *Philosophischen Gesellschaft* of Hamburg in December 1930 and was received by the editorial board of *Mathematische Annalen* on December 21st, 1930; whereas [Hi6] was presented to the Göttingen *Gesellschaft der Wissenschaften* on July 17th, 1931.

In [Hi5] from page 492 on, Hilbert gets defensive, suggesting an awareness of Gödel's results, but writes defiantly on page 494:

> *"Ich glaube, das, was ich wollte and versprach, durch die Beweistheorie vollständig erreicht zu haben: Die mathematische Grundlagenfrage als solche ist dadurch, wie ich glaube, endgültig aus der Welt geschafft."* [i]

In [Hi6], on page 122, he writes:

> *"Nunmehr behaupte ich, daß "widerspruchsfrei" mit "richtig" identisch ist."* [ii]

When Bernays in an interview given on August 17th, 1977, three weeks before his death, cited in [Si4], was asked *"Wie hat Hilbert reagiert, als er von Gödels Beweis der Unmöglichkeit, einen Widerspruchsfreiheitsbeweis für die Zahlentheorie im Rahmen der Zahlentheorie selbst zu führen, erfuhr?"* [iii], he replied *"Ja, ja, er war ziemlich ärgerlich darüber ... Aber er hat nicht bloss negativ reagiert, sondern er hat ja dann eben auch Erweiterungen vorgenommen, z.B. schon im Hamburger Vortrag von 1930"* [iv].

Hilbert's programme after Gödel

It took some years for Gödel's 1931 paper to be generally taken on board[22]: although von Neumann and Herbrand grasped the point quickly, Zermelo did not. One might ask, "After this check, what is left of Hilbert's programme ?"

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[i] "I believe that through proof theory I have completely achieved what I wanted and promised: foundational questions about mathematics are, so I believe, finally expelled from the world."

[ii] "Further, I assert that "consistent" is identical with "true".

[iii] "How did Hilbert react when he realised that Gödel's proof showed that a consistency proof for number theory could not be given within number theory ?"

[iv] "Yes, yes, he was fairly cross about it .... but his reactions weren't only negative: already in his lecture in Hamburg in 1930, he developed extensions of it."

22 See [Daw].

The first reaction of many, including Herbrand,[23] was to think that the incompleteness theorem showed the impossibility of Hilbert's programme. The view taken by Bernays and Gödel was that suggested by Hilbert in his foreword to [HiB1]: there might be finitist arguments not formalisable in Peano arithmetic.

Goldfarb writes that Herbrand's papers (1929, 1930) made an important contribution to Hilbert's programme which led to the Hilbert–Bernays ε theorems, even though one of Herbrand's arguments is fallacious; further, Herbrand's work was largely the foundation for Gentzen's Hauptsatz, which substantiated Hilbert's hope that finitism would go further.[24] Ackermann in [Ack2] showed that Gentzen's 1936 proof [G2] of the consistency of arithmetic by an induction up to the ordinal ε_0 could be presented in terms of the Hilbert-Bernays-Ackermann operator.[25]

Sieg in [Si4] describes not only the discomfiture of Hilbert but on the positive side the advent of the young Gentzen and the further growth of proof theory in a new direction beyond Hilbert's original conception.

Thus Hilbert's programme [Si1] was not wasted but developed into proof theory [Si3].

1934, 1939: publication in two volumes of the treatise of Hilbert and Bernays

Both these volumes were, in fact, written entirely by Bernays, by 1934 expelled from Göttingen by the newly-elected Nazi Government of Germany, and residing once more in Zürich.

A·15 The first volume [HiB1], of 1934, of the treatise of Hilbert and Bernays, which may be regarded as an expanded version of [HiA], presents a treatment of first order logic without the operator, and includes a mature account of the theory of recursion.

Hilbert in his foreword maintains that the view that the results of Gödel entail the impossibility of Hilbert's programme of proof theory is erroneous: they merely make necessary a more precise account of finitism. Bernays in his explains that it had become necessary to divide the projected book mentioned in the foreword to [HiA] into two parts, partly as a result of the works of Herbrand and of Gödel. Both forewords are dated March 1934, with no exact day given.

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23 Sieg in [Si2] examines the brief exchange of letters in 1931 between Herbrand and Gödel.

24 Black's review [Bl1] of both [HiB2] and a 1938 survey paper [G3] of Gentzen explains this point very clearly.

25 For a later study of Hilbert's operator, see Leisenring [L], and for further historical perspective, see [DrKa].

A·16 The foreword of Paul Bernays to [HiB2] is dated February 1939. This second volume gives a detailed development of the epsilon calculus and proof of the two epsilon theorems; then proofs of Gödel's two incompleteness theorems. The very brief foreword of Hilbert to that second volume is dated March 1939 and makes no mention of Gödel.

In §2, 4, f), starting on page 121, Bernays presents an example due to von Neumann which pinpoints the error in the earlier alleged consistency proof for arithmetic.

1935: the naissance of Bourbaki

It was at this delicate period in the history of mathematical logic, when the subject was realigning itself following Gödel's discoveries, that the Bourbaki group was formed. There are many accounts in print of the group's inception, character and achievements,[26] so it is not necessary to repeat that saga here. Our concern is with the group's chosen treatment of logic.

Hilbert's account of logic had received a considerable set-back. He had based his strategy on the belief that all problems could be solved within a single framework. There were substantial texts expounding his ideas; but the hope of a single proof of the consistency and completeness of mathematics, in my view the only justification for basing an encyclopædic account of mathematics on Hilbert's operator, had been dashed.

Constance Reid in Chapter XXI of her life [Re1] of Hilbert discusses the pernicious anæmia diagnosed in him in late 1925. A diet of raw liver seems to have saved him from the worst consequences, but the disease might well have sapped his strength, and more and more he relied on his younger colleagues to carry out his research programmes. When the incompleteness theorems appeared, he did respond to them in time, but, it would seem, only with reluctance.

Undeterred, and unfortunately, Bourbaki, as we shall see, adopted Hilbert's pre-Gödelian stance. In the next section we shall examine Bourbaki's account as finally presented; and in section H explore the soul-searching, revealed in the recently available archives of Bourbaki, among members of the Bourbaki group that led to that final chosen position.

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26 such as [Bea1], [Bea2], [Cor1], [Cor2], [Cor3], [Mas1], [Mas2], [Bor], [Cho], [PlS], and [Sen].

B: ... *which is adopted by Bourbaki* ...

BOURBAKI'S SYNTAX, which we shall now outline, was followed by Godement, whose treatment of logic in his *Cours d'Algèbre* we examine in the next section; we follow their notation in our discussion. Citations such as E I.34 are from Bourbaki's text *Théorie des ensembles* [Bou54].[27]

There were position papers on the foundations of mathematics, published in 1939 and 1943, by members of the Bourbaki group; and on December 31st, 1948, in Columbus, Ohio, Nicolas Bourbaki, by invitation, addressed the eleventh meeting of the Association for Symbolic Logic. That address, [Bou49], chaired by Saunders Mac Lane, delivered by André Weil and published the following year, presents the system that is discussed in [M10] and there called Bou49. The book on logic and set theory that Bourbaki had, after an initial reluctance[28], by then decided to include as Livre I of their projected series, was published by chapters, in 1954, '56 and '57. We denote by Bou54 the system of set theory developed in [Bou54].

Some differences between the two: in 1949, Bourbaki made no mention of the Hilbert operator, and claimed to be able to base "all existing mathematics" on an axiomatic system broadly similar to that of Zermelo 1908, a claim modestly reduced to "modern analysis" in [Di1]. They take ordered pair as a primitive, and appear to believe that the existence of unordered pairs will then follow, a belief refuted in [M10]. They present their underlying system of logic by introducing the notion of a true formula and of the synonymy of two formulæ; they, in effect, state that all propositional tautologies are to be axioms; and they give axioms for quantifier logic, their treatment differing both from [HiA] and from Gödel's completeness paper.

By 1954 on the other hand, they had enhanced their axiom scheme of union to imply a form of the axiom scheme of replacement, they had refined their treatment of propositional logic,‖ giving the same four axioms as those on page 22 of [HiA], which itself follows closely the treatment in *Principia Mathematica* [WR], where these four axioms are given, besides a fifth shown in 1918 by Bernays [Ber1] to be redundant:

$$p \vee p \mathbin{.} \supset \mathbin{.} p, \qquad q \mathbin{.} \supset \mathbin{.} p \vee q, \qquad p \vee q \mathbin{.} \supset \mathbin{.} q \vee p, \qquad q \supset r \mathbin{.} \supset : p \vee q \mathbin{.} \supset \mathbin{.} p \vee r$$

[27] We shall occasionally refer to Bourbaki's in-house journal *La Tribu*, but defer comment on these and other revealing archives, now on-line, till Section H, as our immediate concern is with Bourbaki's books as published.

[28] The first formal gathering of Bourbaki, at Besse-en-Chandesse in 1935, resolved to give no axioms for set theory.

‖ possibly encouraged by the proof in [Hu] that the propositional logics of [WR] and [HiB1] coïncide.

and, most significantly, they based their predicate logic no longer on quantifiers but on the Hilbert operator,◇ a puzzling change as Hilbert himself, in his text with Ackermann and in the first volume of his text with Bernays, presented a development of predicate logic that is operator-free. According to the archives of Bourbaki, the Hilbert operation was missing from Draft 4 of Chapter I, but is found in Draft 5, presumably by Chevalley, of July 1950, and kept in Draft 6, by Dixmier, of March 1951.

Bourbaki's formal language admitted a potentially infinite supply of letters. In 1954 they kept ordered pair as a primitive, written Ɔ, but in later editions followed Kuratowski and defined $(x, y) = \{\{x\}, \{x, y\}\}$, where the unordered pair $\{x, y\}$ is the set whose sole members are x and y, and the singleton $\{x\}$ is $\{x, x\}$.

Bourbaki's syntax

B·1 Bourbaki use Ackermann's dual operator but write it for typographical reasons as τ rather than ε.

Bourbaki use the word *assemblage*, or, in their English translation, *assembly*, to mean a finite sequence of signs or letters, the signs being τ, $\square$, $\vee$, $\neg$, $=$, $\in$ and, in their first edition, Ɔ. The substitution of the assembly A for each occurrence of the letter x in the assembly B is denoted by $(A|x)B$.

Bourbaki use the word *relation* to mean what Anglophones would call a well-formed formula.

B·2 The rules of formation for τ-terms are these:

let R be an assembly and x a letter; then the assembly $\tau_x(R)$ is obtained in three steps:

(B·2·0) form τR, of length one more than that of R;

(B·2·1) link that first occurrence of τ to all occurrences of x in R

(B·2·2) replace all those occurrences of x by an occurrence of $\square$.

In the result x does not occur. The point of that is that there are no bound variables; as variables become bound (by an occurrence of τ,) they are replaced by $\square$, and those occurrences of $\square$ are linked to the occurrence of τ that binds them.

The intended meaning is that $\tau_x(R)$ is some x of which R is true.

Certain assemblies are *terms* and certain are *relations*. These two classes are defined by a simultaneous recursion, presented in Godement [Gd] in nine clauses, thus:

◇ They would not have desired the heavily type-theoretic treatment of predicate logic in *Principia Mathematica*.

T1: every letter is a term

T2: if A and B are terms, the assembly $\supset AB$, in practice written (A, B), is a term.

T3: if A and T are terms and x a letter, then $(A|x)T$ is a term.

T4: if R is a relation, and x a letter, then $\tau_x(R)$ is a term.

R1: If R and S are relations, the assembly $\vee RS$ is a relation; in practice it will be written $(R \vee S)$.

R2: $\neg R$ is a relation if R is.

R3: if R is a relation, x a letter, and A a term, then the assembly $(A|x)R$ is a relation.

R4: If A and B are terms, $=AB$ is a relation, in practice written $A = B$.

R5: If A and B are terms, the assembly $\in AB$ is a relation, in practice written $A \in B$.

B·3 Remark Clauses T3 and R3 are, as pointed out to me by Solovay, redundant — if omitted, they can be established as theorems — and were added to Bourbaki's original definition by Godement, presumably for pedagogical reasons.

B·4 Remark Note that every term begins with a letter, $\supset$ or τ; every relation begins with $=$, $\in$, $\vee$, or $\neg$. Hence no term is a relation.

Quantifiers are introduced as follows:

B·5 Definition $(\exists x)R$ is $(\tau_x(R) \mid x)R$;

B·6 Definition $(\forall x)R$ is $\neg(\exists x)\neg R$.

Thus in this formalism quantifiers are not primitive. Informally, the idea is to choose at the outset, for any formula $\Phi(x)$ a witness, some a such that $\Phi(a)$; call it $\tau_x\Phi$. If there is no such witness, let $\tau_x\Phi$ be anything you like, say the empty set.

B·7 We pause to consider some consequences of basing a formal system on the operator developed by Hilbert, Bernays and Ackermann, commonly called the Hilbert operator and used by Bourbaki.

The length of τ-expansions.

Notice first that if we translate $\exists x\Phi$ as $\Phi(\tau_x\Phi)$, then in writing out that latter formula, we must replace every free occurrence of x in Φ by the string $\tau_x\Phi$, so that formulæ with many quantifiers become, when expanded, inordinately long. In [M8], it is shown that Bourbaki's definition of the number one, when expanded to their primitive notation, requires 4523659424929 symbols, together with 1179618517981 disambiguatory links.

To conceptualise the formalism becomes even more hopeless in later editions of Bourbaki, where the ordered pair $(x,\ y)$ is introduced by Kuratowski's definition, not as a primitive, and the term for 1 takes an impressive

2409875496393137472149767527877436912979508338752092897 symbols,[29] with 871880233733949069946182804910912227472430953034182177 links.

Strangely, the number 1 takes much longer to define than the concept of the Cartesian product of two sets: for example in that formalism of Bourbaki's later editions, the term $X \times Y$ proves to be roughly of length 3.185×10^{18} with 1.151×10^{18} links, and 6.982×10^{14} occurrences each of X and Y; whereas the term defining 1 has over 2×10^{54} symbols with nearly 9×10^{53} links.

Every null term is equal to a proper term

Let us, following Leisenring's 1969 study [L], call a term $\tau_x(R)$ *null* if there is no x with the property R; and let us call it *proper* otherwise: we ignore the problem that there may be some terms of the status of which we know nothing. Is every term T, proper or otherwise, included in the range of variables ? It is, and to see that, start from the

B·8 Proposition $\forall a \exists b\, b = a$.

Now Criterion C30 on page E I.34 states that $(\forall x)R \implies (T \mid x)R$ is a theorem whenever T is a term, x a letter and R a relation. Hence we have, substituting T for a,

B·9 Corollary $\exists b\, b = T$.

Now let β be the term $\tau_b b = T$. β is a proper term, for we have proved there is such a b; and thus we have

B·10 Corollary $\beta = T$.

Any two null terms are equal

The principle S7, on page E I.38 of the *Théorie des Ensembles*, says that

> *si R et S sont des relations de $\mathcal{T}$ et x une lettre, la relation*
>
> $$((\forall x)(R \Longleftrightarrow S)) \implies (\tau_x(R) = \tau_x(S))$$
>
> *est un axiome.*

They add, on page E I.39, *"le lecteur notera que la présence dans S7 du quantificateur $\forall x$ est essentielle."*

B·11 Remark A few lines lower, in small type, is the following remark:

[29] according to a program written by Solovay in Allegro Common Lisp.

> *Par abus de langage, lorsqu'on a démontré une relation de la forme $T = U$ dans une théorie $\mathcal{T}$, on dit souvent que les termes T et U sont "les mêmes" ou sont "identiques". De même, lorsque $T \neq U$ est vraie dans $\mathcal{T}$, on dit que T et U sont "distincts" au lieu de dire que T est différent de U.*

I wish that Bourbaki had not said that; I wish that they had reserved "identical" for the stricter meaning, and been happy with the phrase "$\mathcal{T}$-equivalent" for the looser meaning; because in a passage on page E II.4, which I quote below, they actually mean that two relations are identical, the very same formula, and not merely that they are provably equivalent.

B·12 We note a consequence of the axiom that if R and S are equivalent relations and x is a letter then the relation $\tau_x(R) = \tau_x(S)$ is true.

Set theorists often wish to form the class of all sets with some property: $\{x \mid \Phi(x)\}$; and a question whenever such a class is formed is whether it is a set. How does Bourbaki handle that ?

Bourbaki has a notation for set formation; but it is introduced in an ambiguous way. I quote from page E II.4:

> *Trés fréquemment, dans la suite, on disposera d'un théorème de la forme $Coll_x R$* [defined on page E II.3 as $(\exists y)(\forall x)((x \in y) \iff R)$, where y is a variable distinct from x and not occurring in R]. *On introduira, alors, pour représenter le terme $\tau_y(\forall x)((x \in y) \iff R)$ ... un symbole fonctionnel; dans ce qui suit, nous utilisons le symbole $\{x \mid R\}$; le terme correspondant ne contient pas x. C'est de ce terme qu'il s'agira quand on parlera de "l'ensemble des x tels que R". Par définition (I, p. 32) la relation $(\forall x)((x \in \{x \mid R\}) \iff R)$ est* **identique** *à $Coll_x R$; par suite la relation R est* **équivalente** *à $x \in \{x \mid R\}$.*

I understand that to mean that the symbol $\{x \mid R\}$ is to be introduced in all cases as an abbreviation for the term $\tau_y(\forall x)((x \in y) \iff R)$, whether or not the relation R is collectivising in x, for in that last sentence, "identique" means what it says, the relation $(\forall x)((x \in \{x \mid R\} \iff R)$ and the relation $\mathrm{Coll}_x(R)$ are actually the same relation; which is why I regret the remark cited above encouraging an *abus de langage*: if the proposed laxity for terms were extended to relations, chaos would result.

On the other hand, the *par suite* remark will hold only for collectivised R: take R to be $x = x$; the class of such x is not a set; as we saw above, every term will be some set, and therefore some x will not be in the term $\tau_y(\forall x)((x \in y) \iff R)$.

B·13 Thus there is an acute difference between the normal use, in ZF and many other set theories, of the class-forming operator $\{\,|\,\}$ with the Church conversion schema $x \in \{x \mid R\} \iff R$ holding for all classes whether sets or not, and the Bourbaki treatment whereby, magically, conversion holds for a class if and only if that class is a set.

B·14 Now let $R\{x\}$ and $S\{x\}$ be two formulæ such that R and S are, provably in Bou54, not collectivising in x; for example $R\{x\}$ might be $x \notin x$ and $S\{x\}$ might be "x is a von Neumann ordinal"and we could use the Russell paradox in the one case and the Burali-Forti paradox in the other to establish the non-set-hood of these classes.

Write C_R for $(\forall x)((x \in y) \iff R)$ and C_S for $(\forall x)((x \in y) \iff S)$. Then Bou54 proves that for all y, C_R is false, and proves for all y that C_S is false; so it proves that $C_R \iff C_S$. Hence, by axiom S7, page E I.38, $\tau_y C_R = \tau_y C_S$: hence our notation is highly misleading in that all classes which are not sets have become "equal".

B·15 Example As illustration, let T be a term which is certainly null, namely the universe. In Bourbaki's notation the following is a theorem:

B·16 Theorem $\exists b\, b = \{x \mid x = x\}$.

Any ZF-iste reading that assertion would interpret it as false. The following is also a theorem of Bou54:

B·17 Theorem $\exists a\, a = \{x \mid x \notin a\}$

and indeed the b of Theorem B·16 and the a of Theorem B·17 are equal.

Perverted interpretation of quantifiers

The step from an uninterpreted formal language to its possible interpretations is one of great epistemological importance. So, phenomenologically, the τ-operator perverts the natural order of mental acts: to interpret $\exists$ you look for witnesses and must first check whether a witness exists before you can pick an interpretation for $\tau_x(R)$; and then you **define** $(\exists x)R$ to mean that the witness witnesses that $R(\tau_x(R))$: a strange way to justify what Bourbaki, if pressed[30], would claim to be a meaningless text.

Discussion

B·18 Hilbert developed his operator in the belief that it would lead to consistency proofs for systems of arithmetic and of set theory; at the time he believed in the completeness of mathematics. In a complete system, such

[30] See the remarks below in D·11 and the anti-ontologist jibe in issue 15 of *La Tribu* quoted in H·8.

as the theory of a particular model, it works happily as a formal version of a Skolem function; but it starts to behave strangely in incomplete systems; and work in logic has shown that the phenomenon of incompleteness is not something marginal but pervades mathematics.[31]

B·19 A second count against Hilbert's operator is its blurring of subtleties concerning the various forms of the Axiom of Choice. If you then immerse yourself in a formalism based on it, how can you discuss models where AC is false ? Indeed, the use of the τ-operator in this fashion seems to render impossible discussion of non-AC models, and as Gandy remarked in his review [G1] of *Théorie des Ensembles*, it makes it hard to tell when the Axiom of Choice is being used or not.

B·20 The strange way in which null terms behave makes the τ-calculus peculiarly unsuitable for use in set theory, where the idea of a proper class is important; and, of course, proper classes are important to the way category theorists think about functors; so that my objections to Bourbaki's logic apply both to a set-theory and to a category-theory based view of mathematics.

I am, I admit, being unfair to Hilbert in that I am writing eighty-nine years later, and benefitting from the enormous development of logic and set theory that has taken place in that time. But a modern account of foundational questions must allow for that development, and Bourbaki shut their eyes to it.

B·21 The idea of an expanding mathematical universe is essential to contemporary set theory. Consider the construction by Gödel of his inner model of AC and GCH. We have an iteration along the ordinals; at each successor stage we look at the class of those things definable over the previous stage; so the Bourbaki trick that a proper class "equals" some pre-assigned set is not the right mind-set for this construction. If you have immersed yourself in Bourbaki, Gödel, or set-theoretic recursion in general, will make no sense to you.

B·22 The "intuitive interpretation" where you choose once and for all witnesses for every true existential statement: implicit in that is the idea that you will choose one universe once and for all, which is precisely the notion that, according to Goldfarb, was current at the start of the nineteen-twenties. But if you are imbued with that idea, how are you to understand arguments, common in contemporary set theory ever since Cohen's announcement [Coh1], which involve passing from one universe to a larger one obtained by a forcing extension ? If you have immersed yourself in

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[31] The writings of Harvey Friedman give many examples; the paper [M5] describes four from analysis.

Bourbaki, Cohen will make no sense to you.

B·23 Comment So it might reasonably be said that Bourbaki's *Théorie des Ensembles* provides a foundation for many parts of mathematics, but not for set theory. That in turn provokes the thought that a mathematician who takes the Bourbachiste shilling *ipso facto* switches off that portion of his mathematical intelligence that can respond to the insights of set theory.

Bourbaki's remarks on progress in logic

Their historical remarks at the end of their set theory volume are well worth reading;□ it is only as their narrative comes up to modern times that one senses a pressure to adopt their view to the exclusion of all else. We quote various remarks that show that Bourbaki have a lively sense of logic as a developing subject.

They remark that philosophers don't have an up-to-date idea of mathematics and that logic is concerned with many things outside mathematics; but the neglect by philosophers of mathematics helps block progress in formal logic. They acknowledge that mathematicians with a good grasp of philosophy are equally rare.

On E IV.37, a footnote mentions the legendary event when at a lecture at Princeton in the presence of Gödel, the speaker said that there had been no progress in logic since Aristotle;⋆ I sense a hint that the speaker might have been a philosopher. The speaker—and I would love to know who it was—was in good company: Bourbaki mention Kant's dictum that there was no need for new ideas in logic.

On page IV.42, they mention the harm done to Peano by Poincaré's often unjust jibes. The seven enthusiastic collaborators of Peano in the creation of his *Formulario Mathematico* of 1895 are listed.

B·24 They mention that Leibniz had the idea of Gödel numbering; that Frege had good ideas but bad notation; that Peano's notation was good; that Russell and Whitehead created a formalised language that "happily combines the precision of Frege with the convenience of Peano"; and that among subsequent modifications "the most interesting is certainly the introduction by Hilbert of" his symbol; they note that there was a shift to the dual; and that the symbol (apparently) bypasses the axiom of choice.

B·25 The paradoxes are discussed and the "Five letters" [Had] are mentioned, as is, on page 70, without naming him, Hermann Weyl's consequent resolve, quoted below in D·10, to avoid areas with paradoxes.

□ According to *La Tribu*, Weil was to consult Rosser when preparing the *Note Historique.*

⋆ Might Rosser have been the source of this information ?

B·26 They trace the history that culminated in the treatise of Hilbert and Bernays, with references to Poincaré, Ackermann, Herbrand and von Neumann, but, strangely, in their bibliography list Hilbert–Ackermann but not Hilbert–Bernays. Kleene's 1952 book [Kl] is listed; as (in the 1970 edition) are Cohen's PNAS announcements [Coh1].

Bourbaki's account of the incompleteness theorem.

B·27 Their formulation of the incompleteness theorem is correct, if in their definition on page E I.21, a *theory* is to be a deductive system with finitely many explicit axioms and finitely many axiom schemes, (such as Zermelo's Aussonderungsprinzip), the substitution instances of which will be called the implicit axioms of the theory. Hence a theory, according to their definition, will be recursively axiomatisable. They put much emphasis on the role of substitution; they have to consider the result of substituting terms for variables in the explicit axioms, but need not do so for the implicit axioms, which by definition will form a class of wffs closed under the relevant substitutions.

B·28 Bourbaki define a formula to be *false* if its negation is provable. On page E IV 75, in footnote 1, they say that of any mathematical proposition it will eventually be known whether it is true, false or undecidable; so they are aware of the distinction between falsehood and unprovability. But their love of identifying truth with provability lands them in a tangle on Page E IV 74, in footnote 3: they say, incorrectly, that the proposition $S\{\gamma\}$ affirms its own falsehood, but it actually affirms its own unprovability.

B·29 COMMENT We shall see that this idea that systems are complete, with its corollary that "unprovable" is the same as "refutable", leads to repeated errors of logic in the works of members of the Bourbaki school.

C: ... and by Godement, though with expressions of distrust.

ROGER GODEMENT is listed by Cartier in an interview [Sen] in the *Mathematical Intelligencer* as a member of the second generation of Bourbaki, in the same group as Dixmier, Eilenberg, Koszul, Samuel, Schwartz, and Serre. In 1963 his *Cours d'Algèbre* was published by Hermann of Paris. An English translation, copyrighted by Hermann in 1968, was published by Kershaw of London in 1969, and runs to some 600 pages, of which the first hundred are devoted to an account of logic and set theory.

I used the last 500 pages of this text for many years during the time that I was teaching algebra to Cambridge undergraduates, and found it excellent. When, later, I read the first part, on Set Theory, consisting of §§0-5, I found, unhappily, that the account offered, although containing many tart remarks to delight the reader, is flawed in various ways: one finds errors of metamathematics, mis-statements of the results of Gödel and Cohen, and an accumulation of negative messages about logic and set theory.

In this chapter I shall present my findings by combing quickly through his account and commenting as I go. I follow Godement in using the sign § to indicate chapter and N^o to indicate section within a chapter.

In the main he follows Bourbaki's *Théorie des Ensembles.* I have rearranged this material and simplified it in one or two places. Quotations, from the English 1968 translation of his book, are given in *slanted type*; the pagination follows that edition.

Godement has, pleasingly, in later French editions, moderated some of the provocative statements on which I comment, and I shall in such places draw attention to those revisions.

Godement's formal system

Godement states that the opening chapter is "an introduction to mathematical logic", and then adds, somewhat alarmingly, that "in it we have tried to give a rough idea of the way mathematicians conceive of the objects they work with." That opening chapter begins on page 20, where he writes:

> *In mathematics there are three fundamental processes: construction of mathematical objects, the formation of relations between these objects, and the proof that certain of these relations are true, or, as usually said, are theorems.*
>
> *Examples of mathematical objects are numbers, functions, geometrical figures, and countless other things which mathematicians handle; strictly speaking these objects do not exist in Nature*

but are abstract models of physical objects, which may be complicated or simple, visible or not. Relations are assertions (true or not) which may be made about these objects, and which correspond to hypothetical properties of natural objects of which the mathematical objects are models. The true relations, as far as the mathematician is concerned, are those which may be logically deduced from a small number of axioms laid down once and for all. These axioms translate into mathematical language the most "self-evident" properties of the concrete objects under consideration; and the sequence of syllogisms by which one passes from the axioms (or, in practice, from theorems previously established) to a given theorem constitutes a proof of the latter.

C·1 COMMENT Notice the use of the phrase **once and for all**.

He continues:

Explanations of this sort, which may perhaps appear admirably clear to some beginners, have long since ceased to satisfy mathematicians: not only because mathematicians have small patience with vague phrases, but also and especially because mathematics itself has forced [mathematicians] to consider carefully the foundations of their science and to replace generalities by formulas whose meaning should be altogether free from ambiguity, and such that it should be possible to decide in a quasi-mechanical fashion whether they are true or not; and whether they make sense or not.

C·2 COMMENT Notice the residual faith in the completeness and decidability of mathematics implicit in that last sentence.

After some further discussion he begins his presentation of the formal system Bou54. Broadly he uses the syntax summarised in my Section B, but where Bourbaki speaks of terms, he speaks of mathematical objects; otherwise he follows Bourbaki's development, with, as in Bourbaki's first edition, a primitive sign, Ɔ, for the ordered pair of two objects. He immediately states that he will write (A, B) rather than ƆAB.

He departs from Bourbaki by seeking to soften the austerity of the formalism, which leads him to make mistakes of logic, for he blurs the distinction between a uninterpreted formal language and its interpretations; and by a strange reluctance to state the axioms of set theory.

C·3 To introduce the axioms of logic, he writes, at the bottom of page 21:

Once the list of fundamental signs has been fixed, and the list of criteria of formation of mathematical objects and relations, it remains to state the axioms. Some will be purely logical, others of a strictly mathematical nature.

On page 25, he says that

> **true** *relations or* **theorems** … *are those which can be obtained by repeated application of the two rules:*
> *(TR 1): Every relation obtained by applying an axiom is true*
> *(TR 2): if R and S are relations, if the relation $(R \implies S)$ is true and if the relation R is true, then the relation S is true.*

and on page 26, he says that

> *A relation is said to be* **false** *if its negation is true.*

and then immediately in Remark 1 that:

> *what characterizes true relations is that they can be proved.*

C·4 Comment This equation of truth and provability is the standard Bourbachiste position, found in the papers of Cartan and of Dieudonné of 1939-43 cited in *The Ignorance of Bourbaki* [M3], and found again in Dieudonné's last book [Di3, 1987, 1992]. But Godement then, in *Remark 2* cautions the reader with the symbol for a *tournant dangereux*:

> *There is a natural tendency to think that a relation which is "not true" must necessarily be "false". … Unfortunately there is every reason to believe that in principle this is not so.*

We shall return to the rest of *Remark 2* later.

C·5 Remark Thus words like "true" and "satisfies", as for example in the sentence

> *The mathematical object A is said to satisfy the relation R if the relation $(A|x)R$ is true,*

are being defined in terms of the provability relation $\vdash_{\mathsf{Bou54}}$ that Godement is developing. Note, too, his comment in *Remark 4* of §0, on page 28:

> *[from (TL 2)] ... the relation R* **ou** *(* **non** *R) is true. It does not follow that at least one of the relations R,* **non** *R is true: this is precisely the question of whether there exist undecidable relations !*

C·6 Of (TL 7), which says that if $R(x)$ is true so is $(A|x)R$, Godement comments at the top of page 31 that

> *[its] purpose … is precisely to justify this interpretation of letters as "undetermined objects";*

C·7 Comment We were told on page 21 that assemblies are built up from fundamental signs and letters, which suggests that a letter is a symbol, but at the top of page 30 we read

> *Let R be a relation, A a mathematical object, and x a letter (i.e. a "totally indeterminate" mathematical object),*

so by page 30 a letter is not a symbol: Godement is slipping away from treating his system as an uninterpreted calculus, and moving towards an informal Platonism.

C·8 Quantifiers are introduced on page 31 exactly as in Bourbaki, and need little comment beyond our remarks in Section B. He remarks on page 37 that

> *we shall now be able, by using the Hilbert operation, to introduce them as simple abbreviations*

— a phrase which in the light of the calculations of [M8] may strike the reader as a bit rich; and he ends the chapter with this pleasing remark:

> *Like the God of the philosophers, the Hilbert operation is incomprehensible and invisible; but it governs everything, and its visible manifestations are everywhere.*

Godement's set-theoretic axioms

Early in the next chapter, §1, he says

> *... if a and b are mathematical objects (or* **sets***— the two terms are synonymous) ...*

We shall see that that phrase causes trouble. A footnote on page 41 describes the less formal style he now wishes to adopt.

He introduces the axioms of equality in Theorem 1 on pages 41/42:

> *... intuitively this relation, when it is true, means that the concrete objects which a and b are thought of as representing are "identical". We do not enter into a philosophical discussion of the meaning of "identity" . . .*
>
> *THEOREM 1: a) The relation $x = x$ is true for all x.*
>
> *b) The relations $x = y$ and $y = x$ are equivalent, for all x and all y.*
>
> *c) For all x, y, z, the relations $x = y$ and $y = z$ imply the relation $x = z$.*
>
> *d) Let u, v be objects such that $u = v$ and let $R\{x\}$ be a relation containing a letter x. Then the relations $R\{u\}$ and $R\{v\}$ . . . are equivalent.*

He comments that part d) is an axiom, while parts a), b) and c) can be deduced from

> *a single much more complicated axiom, (which the beginner should not attempt to understand) namely that if R and S are equivalent relations and x is a letter then the relation $\tau_x(R) = \tau_x(S)$ is true.*

C·9 COMMENT In Part d), formal system and informal interpretation are confused: otherwise "*such that* $u = v$" has no meaning. His definitions, taken *au pied de la lettre*, would imply that he means that if $\vdash_{\mathsf{Bou54}} [u = v]$ then $\vdash_{\mathsf{Bou54}} [R\{u\} \iff R\{v\}]$, which is weaker than what I suspect he intended, namely that $\vdash_{\mathsf{Bou54}} [u = v \implies (R\{u\} \iff R\{v\})]$—a point with consequences for the notion of *cardinal number*.

C·10 COMMENT It may be for pedagogical reasons that he introduces the axioms one by one, but it is regrettable that there is no signal to the reader when the presentation is complete.

Nothing is said about the origins of the axioms: they are presented as oracular pronouncements.

So far as I can tell, searching the first hundred pages and assuming that no further axioms will be introduced once he starts on the algebra, his set-theoretic axioms are these:

First, the axiom of **extensionality**, which appears on page 42:

> ... *In fact there is only one axiom governing the use of* $\in$, *namely:*
> *THEOREM 2: Let* A *and* B *be two sets. Then we have* $A = B$ *if and only if the relations* $x \in A$ *and* $x \in B$ *are equivalent.*

I take the preamble to Theorem 2 to mean that he considers it to be an axiom: thus we have had one set-theoretic axiom so far, that of extensionality, but stated as a theorem.

C·11 COMMENT Note the further slipping between languages: "*the relations are equivalent*" means that a certain formula is provable in the system Bou54 we are building up. That is a clear assertion, and is said to hold iff $A = B$; but $A = B$ is an (uninterpreted) relation. Does he mean "$A = B$ is true iff the relations are equivalent", or does he mean "it is provable in Bou54 that $\big(A = B \iff (x \in A \iff x \in B)\big)$" ? That he is aware of the difference is evident from part (2) of Remark 7 on page 34.

C·12 COMMENT Theorem 2 as stated is false. Let x and y be distinct letters. Looking ahead to pages 46–7, where Godement adds the axiom of pairing, let A be $\{x\}$ and let B be $\{x, y\}$. I believe that A and B are mathematical objects; indeed a definition using τ can be given, though Godement does not do so: I assume that formally he would put $\{x\} =_{\mathrm{df}} \tau_y((\forall z)((z \in y) \iff z = x))$ and $\{x, y\} =_{\mathrm{df}} \tau_z((\forall w)((w \in z) \iff w = x \textbf{ ou } w = y))$. On page 41 he says that the terms *mathematical object* and *set* are synonymous. Very well, A and B are sets, and the following is provable in Bou54: $x \in A \iff x \in B$, so that the relations $x \in A$ and $x \in B$ are equivalent. If Theorem 2 were provable, we could infer that $A = B$ and thus that $x = y$; so we would have proved that any two sets are equal !

Of course, what is lacking is a requirement that the letter x has no occurrence in A or in B. That slip is strange in that in the statement of Theorem 4 on page 44 (a version of the scheme of separation) he is careful to say that the relation R is to contain a variable x: though there the theorem would still be true if it did not. On the other hand the further statement of theorem 4, that *for every set X there exists a unique subset A of X, ...* shows further confusion of language; "*for every set X*" might mean "for every mathematical object", that is "for every term ", but "*there exists a unique subset*" certainly is using "there exists" mathematically; A is a variable here not a term, and will be the subject of various assertions.

C·13 Of his version of the scheme of **separation** given in Theorem 4 of §1, on page 44, Godement says in Remark 4 on that page that

> *Mathematically, Theorem 4 cannot be proved without using axioms which are far less self-evident, and the beginner is therefore advised to assume Theorem 4 as an axiom*

so, so far, we have had extensionality and separation.

C·14 Then Godement, wishing to comment on the necessity of giving a scheme of separation rather than of comprehension, writes on page 44:

> *Remark 5: In spite of the dictates of common sense, it is not true that for every relation $R\{x\}$ there exists a set (in the precise sense of §0) whose elements are all the objects x for which $R\{x\}$ is true.*

C·15 COMMENT When I turn to §0, I find that the word "set" occurs only thrice, namely on page 20 in N^{o}1, in the phrases *the development of the "theory of sets"*, *Set theory was created by Cantor*, and *set theory had given rise to genuine internal contradictions*; I can find it nowhere else in §0, although there is a lot of talk about **objects**. The word **sets** appears, in bold face, early in §1, suggesting that that is its definition, namely that a set is the same as a mathematical object. So, as I can find no precise sense of "set" in §0, whereas "mathematical object" is given a very precise sense in that section, namely that it is a term in a certain carefully specified formal language, and as I am told that a set is the same as a mathematical object, so be it: I shall take 'set' to mean 'term in Godement's formal language'.

Remark 5 continues:

> *Suppose that there exists a set A such that the relations $x \in A$ and $x \notin x$ are equivalent.*

He hopes to get a contradiction, but there is nothing wrong with that, as it stands: let A be

$$\tau_y((\forall z)((z \in y) \Longleftrightarrow (z = x \;\&\; z \notin z))).$$

Then A is a mathematical object, therefore a set, and the relation $x \in A \iff x \notin x$ is true.

C·16 Remark Again, what is missing is the requirement that the mathematical object A have no occurrence of the letter x; then his remark that the supposition of the existence of such an A leads to contradiction would become correct.

C·17 We shall refer below to his Remark 6, on page 45, in which he says that apparently obvious assertions cease to be so simple when it is a question of effectively *proving* them.

The empty set is discussed in N^{o}4: if there is a set then the empty set exists, by the scheme of separation: no axiom so far asserts the unconditional existence of any set; but the τ-formalism guarantees that the existence of something is provable.

Godement goes on in N^{o}5 to discuss sets of one and two elements, and then says *In the same way we can define sets of three, four ... elements. The sets so obtained are called finite sets, and all other sets are called infinite sets. These two notions will be considered afresh in §5.*

C·18 Comment Note that that is not a formal definition of "finite", the use of dots constituting an appeal to the reader's intuitive notion of finiteness.

C·19 In §1, N^{o}5, Remark 8, on page 47, he states that the existence of the **pair set** of two objects is an axiom, and goes on to say that the existence of **infinite sets** is also an axiom, and acknowledges that we have yet to define the natural numbers, which will be done in §5; so far we have had axioms of extensionality, separation, pairing and infinity, but we await a definition of *finite*.

In N^{o}6 the **set of subsets** of a given set is introduced thus:

> *Let X be a set. Then there exists — this again is one of the axioms of mathematics — one and only one set, denoted by*
>
> $$\mathcal{P}(X)$$
>
> *with the following property: the elements of $\mathcal{P}(X)$ are the subsets of X,*

so hitherto we have had extensionality, separation, pairing, infinity, and power set; we await a definition of *finite*.

C·20 In §2 he discusses ordered pairs and Cartesian products, no new axioms being introduced till §3. In Remark 1 of §2 on page 50, he writes:

> *The [Kuratowski method of defining] ordered pair is totally devoid of interest. . . . The one and only question of mathematical importance is to know the conditions under which two ordered pairs are equal.*

C·21 COMMENT To an algebraist, that might be true. But to a set-theorist interested in doing abstract recursion theory, it is very natural to ask whether a given set is closed under pairing. For that reason, an economical definition of ordered pair is desirable, such as is furnished by Kuratowski's definition: otherwise one might find that the class of hereditarily finite sets is not closed under pairing, or even that no countable transitive set is. Bourbaki in their later editions have indeed adopted the Kuratowski ordered pair.

In §2 N^{o}2 he declares that

using the methods of §0, cartesian products can be proved to exist.

C·22 COMMENT I deny that that can be proved from the axioms he has stated so far, given that he has refused to define ordered pair — hence we do not know where the values of the unpairing functions (projections) lie — and he has not stated a scheme of replacement.

Write $\langle x,\, y\rangle_K$ for the Kuratowski ordered pair $\{\{x\},\, \{x,\, y\}\}$, and $u \times_K v$ for $\{\langle x,\, y\rangle_K \mid x \in u \;\&\; y \in v\}$, the correspondingly defined Cartesian product.

Let "F defines a possible pairing function", where F is a three-place Bourbaki relation, abbreviate the conjunction of these statements:

(C·22·0) $\forall x \forall y \exists$ exactly one z with $F(x, y, z)$

(C·22·1) $\forall z \forall u \forall v \forall x \forall y \big(F(u, v, z) \;\&\; F(x, y, z) \implies [u = x \,\&\, v = y]\big)$.

Let "$X \times_F Y \in V$" denote the formula $\exists W \forall w (w \in W \iff \exists x \exists y (x \in X \,\&\, y \in Y \,\&\, F(x, y, w))$.

Consider the following argument, which I present in ZF-style set theory.

C·23 LEMMA *Suppose $x \mapsto G(x)$ is a one-place function with domain V. Define $F(x, y, z) \iff_{\mathrm{df}} z = \langle G(x),\, \langle x,\, y\rangle_K\rangle_K$. Then F defines a possible pairing function.*

Proof : Write $(x,\, y)_F$ for $\langle G(x),\, \langle x,\, y\rangle_K\rangle_K$. We have only to check that the crucial property $(x,\, y)_F = (z,\, w)_F \implies x = z \;\&\; y = w$ is provable. But that is immediate from the properties of the Kuratowski ordered pair. ⊣ (C·23)

C·24 LEMMA *Now let A be a set, and let $B = \{\varnothing\}$; let G and F be as above; then if $A \times_F B \in V$, then the image $G``A$ of the set of points in A under G is a set.*

Proof : If $A \times_F B \in V$,

$$A \times_F B = \{(a,\, b)_F \mid a \in A, b \in B\} = \{\langle G(a),\, \langle a,\, b\rangle_K\rangle_K \mid a \in A, b \in B\}.$$

$G``A = \{G(a) \mid a \in A\} \subseteq \bigcup\bigcup(A \times_F B)$, so the lemma will follow from two applications of the axiom of union, which will be become available to us on a second reading of §3 N^{o}2, and an application of the scheme of separation, which we have been advised to take as an axiom. ⊣ (C·24)

Now let GT be the system comprising the axioms of extensionality, pairing and power set, together with the scheme of separation: since $\vdash_{\mathsf{GT}}$ $u \times_K v \subseteq \mathcal{P}(\mathcal{P}(\bigcup\{u, v\}))$, the set-hood of $u \times_K v$ when u and v are sets can be proved in GT plus the axiom that $u \cup v \in V$, a weak form of the axiom of union.

C·25 METATHEOREM *The following systems are equivalent over* GT*:*

(C·25·0) *Bourbaki's scheme S8 of selection and union;*

(C·25·1) *Godement's scheme of union, discussed below;*

(C·25·2) *the axiom of union plus the scheme of replacement;*

(C·25·3) *the axiom of union plus the scheme that for each formula F with three free variables, the sentence expressing "if F defines a possible pairing function then for each A and B, $A \times_F B$ is a set" is an axiom.*

Since GT is a subsystem of Z, in which many instances of replacement fail, Godement's claim to prove the existence of cartesian products, no matter what definition of ordered pair is used, must also fail.

C·26 In Remark 4 on page 56, the existence of the set of natural numbers is assumed (and referred to the existence of infinite sets, stated to be an axiom, but not formulated: so far there has been no proper definition of *finite*).

C·27 The **Axiom of Choice** sneaks in *via* the τ-operator, about which Godement has said, on page 37, §0 N^{o}9:

> *It is also used nowadays in place of the Axiom of Choice. (§2 Remark 7).*

Turning to §2 N^{o}8, on page 63, we find in Remark 8, not 7, a demonstration of the use of τ to get AC:

> *.. for want of anything better we can define a function h by $h(y) = \tau_x(f(x) = y)$ ...*

C·28 In §3 he turns to unions and intersections. In Remark 1 on page 70 it is mentioned that we need an axiom of union to form $X \cup Y$, with forward reference to N^{o}2. Of N^{o}2 it is said that it may be omitted at a first reading. It is stated that the existence of the **union** of an arbitrary family is an axiom of mathematics. But ZF-istes must beware ! for Godement's axiom of union is really a scheme which is much stronger than the simple axiom of union, and therefore I shall speak of it as Godement's scheme of union.

With that point in mind, we have now had the axioms of extensionality, pairing, infinity, and power set, and the schemes of separation and union; we continue to await a definition of *finite*.

C·29 That his system is equivalent to Bourbaki's follows from an examination of Bourbaki's *schéma de sélection et réunion* found as S8 of the *Théorie des Ensembles*, on page E II.4:

> *Soient R une relation, x et y des lettres distinctes, X et Y des lettres distinctes de x et y et ne figurant pas dans R. La relation*
>
> $$(\forall y)(\exists X)(\forall x)(R \implies (x \in X)) \implies (\forall Y)\mathrm{Coll}_x((\exists y)((y \in Y) \textit{ et } R))$$
>
> *est un axiome.*

Bourbaki is aware of the power of S8, drawing attention to the difference between the union of a family of subsets of a given set and the union of a family of sets where no containing set is known. Thus the ZF-ists' axiom of union together with schemes of separation and replacement is equivalent to Godement's scheme of union and to Bourbaki's scheme of selection and union.

C·30 §4, on equivalence relations, calls for little comment. Example 4 on page 78 speaks of the set of rational integers, though we still await a definition of $\mathbb{N}$.

C·31 In §5, on *Finite sets and integers* the concept of finiteness will be at last defined. Kronecker's witticism is here attributed to Dedekind. Godement remarks that *the integers with which we are concerned here are mathematical objects, not concrete integers,* which underlines the need for a formal definition of *finite*.

In N^{o}1, he introduces the notion of equipotence; his Theorem 1 is a version of AC: any two sets are comparable. (Bernstein's proof is given in Exercise 5).

C·32 In §5 N^{o}2, the cardinal of X is elegantly defined as $\tau_Y(Eq(X, Y))$, emphasizing the reliance on the identity of τ-selected witnesses to equivalent propositions: my readers should bear C·9 in mind.

On page 90, discussing equipotence, he emphasizes that the "ordinary" numbers are metaphysical ideas derived from concrete experience, whereas "Mathematical" numbers are objects defined by following the procedures of §0.

His treatment of cardinals follows Bourbaki. Thus 0 is defined on page 90 to be the cardinal of the empty set, therefore some object equipollent to the empty set; therefore (as remarked by Bourbaki but not by Godement) the empty set itself.

1 is the cardinal of the singleton of the empty set, so is some object with exactly one element and therefore not equal to 0. The paper [M8] shows how this definition gets out of hand.

2 is the cardinal of the von Neumann ordinal 2, so is some object with exactly two elements. The calculations of [M8] would presumably yield even more monstrously long assemblies for this and other finite cardinals. There is a forward reference to §5 N^{o}4. Finally he assumes without proof:

Theorem 2: any set of cardinal numbers has a sup and an inf.

C·33 COMMENT That too involves an appeal to replacement. Without it, curious things happen. Suppose we define $Card(n)$ to be the set $\{\aleph_k \mid k < n\}$, where we take $\aleph_k$ to be an initial ordinal: a reasonable definition, by Bourbachiste standards, as it is indeed a set of cardinality n. But then in a set theory without some version of the axiom of replacement, (for example, in Zermelo set theory) the class of finite cardinals as we have just defined them need not be a set.

C·34 COMMENT There will be other unheralded uses of replacement: for example, the construction on page 97 of the set $\bigcup_{n\in\mathbf{N}} X_n$, and in the footnote on the same page, the proof that the class of cardinals less than a given cardinal is a set.

His exercise 3 page 108 asks the reader to prove that the countable union of countable sets is countable: another covert use of AC !.

C·35 In §5 N^{o}3, he defines the sum of a family of cardinals. That enables him, in N^{o}4 on page 95, to follow Dedekind's approach and define a cardinal x to be *finite* if $x \neq x + 1$; a *natural number* is then defined to be a finite cardinal; and a set is finite if its cardinal is finite.

C·36 In N^{o}5 Godement derives the existence of the set of natural numbers from the existence of infinite sets, which he has previously stated to be one of the axioms of mathematics. Thus his axiom of infinity would state that there is a cardinal x which equals $x + 1$.

C·37 REMARK His definition of *finite* relies on a mild form of the Axiom of Choice to be correct; a set is Dedekind-infinite iff there is an injection of $\mathbb{N}$ into it; and if ZF is consistent, then there are models of the ZF axioms without AC in which there are Dedekind-finite sets not equipotent to any finite ordinal.

C·38 That appears to be all the axioms given by Godement. I find no mention of an axiom of foundation; but, as we have seen, the scheme of replacement is embedded in his scheme of union.

C·39 COMMENT Mathematicians, wrote Godement, have small patience with vague phrases; why then should they tolerate the purposeless imprecision of the Bourbaki–Godement treatment of finite cardinals and AC ?

Misunderstandings of work of logicians

When Godement comes to the work of Gödel and other logicians, he makes more serious errors. For example, on page 26, §0, N^{o}4, Remark 2, after defining the notion of an *undecidable* relation, he wrote:

> *At the present time no example is known of a relation which can be proved to be undecidable (so that the reader is unlikely to meet one in practice ...) But on the other hand the logicians (especially K. Gödel) showed thirty years ago that there is no hope of eventually finding a "reasonable proof" of the fact that every relation is either true or false; and their arguments make it extremely probable that undecidable relations exist. Roughly speaking the usual axioms of mathematics are not sufficiently restrictive to prevent the manifestation of logical ambiguities.*

C·40 COMMENT Notice the use of the word **fact**.

C·41 COMMENT The French original ... *du fait que toute relation est soit vraie soit fausse ...* of those words was written in 1963, thirty three years after the incompleteness theorems were announced. What is one to make of these statements ? If he believes that theorems are deduced from a small number of axioms laid down once and for all, and if that means that the set of axioms is recursive, then if his chosen system is consistent, undecidable relations are certainly known; and given his definition of "true" as provable and "false" as refutable, it is simply not the case that every relation is either true or false. Why is he so reluctant to allow Gödel's discoveries to be established rather than be merely "extremely probable" ?

The last sentence quoted above is perfectly correct, more so than perhaps Godement realised. Incompleteness pervades mathematics: the phenomenon may be found in almost any branch of mathematics and is not something confined to artificial and contrived assertions on the very margin of our science.

C·42 In §0 N^{o}4, on page 26, he writes

> *Remark 3: ... contradictory relations are both true and false. The efforts of the logicians to establish a priori that no such relations exist have not so far met with success.*

COMMENT That last statement wholly misrepresents the import of the Incompleteness Theorems.

C·43 We have seen that he follows Bourbaki in using τ to get the Axiom of Choice. His comment,

> *The possibility of constructing a [choice set] (which is obvious if one uses the Hilbert operation) is known as the Axiom of Choice.*

> *Until recent times it was regarded with suspicion by some mathematicians, but the work of Kurt Gödel (1940) has established that the Axiom of Choice is not in contradiction to the other axioms (which of course does not in any way prove that the latter are non-contradictory)*

is well-phrased, though it might leave the reader wondering what the problem was that Gödel solved.

C·44 The first French edition of Godement's *Cours d'Algèbre* was written, presumably, just before the announcement of Cohen's discoveries. In Remark 9 of §5 N^{o}7, on page 98 of the 1969 English translation, he mentions Cohen's "magnificent result" that CH is undecidable, and acknowledges that his earlier assertion that the reader would not encounter an undecidable relation should now be amended; but he makes no mention of Gödel's consistency proof for CH.

In his later French editions, such as the impressions of 1973 and 1980, he does mention Gödel's relative consistency proof for the continuum hypothesis, and also rewrites the other three Remarks that I have just quoted, so that his readers are now told *something* about the existence of undecidable statements in normal mathematics.

Unease in the presence of logic

Besides the above mis-statements by Godement concerning the work of logicians, we find repeatedly an undertow of unhappiness about logic, which, sadly, has not been corrected in his later French editions.

He writes on page 22, in §0 N^{o}2,

> *It has been calculated that if one were to write down in formalized language a mathematical object so (apparently) simple as the number 1, the result would be an assembly of several tens of thousands of signs.*

C·45 COMMENT This remark goes back to Bourbaki, and it is shown in [M8] that the estimate given is too small by a factor of perhaps a hundred million. But even if their estimate were correct, **what is the point of all those symbols ?** Why not follow Zermelo and von Neumann and define 0 as $\varnothing$ and 1 as $\{0\}$?

Poincaré mocked Couturat for taking perhaps twenty symbols to define the number 1, in an attempt to reduce that arithmetical concept to one of logic; now Bourbaki is taking 4 European billions (= American trillions) of symbols to do the same thing: one million thousand-page books of densely packed symbols. Suppose an error occurred somewhere in those pages: would anyone notice ? would it matter ? That is not where the mathematics resides.

C·46 In short, the chosen formalism is ridiculous, and Godement knows that it is ridiculous, for he makes the excellent remark:

> *A mathematician who attempted to manipulate such assemblies of signs might be compared to a mountaineer who, in order to choose his footholds, first examined the rock face with an electron microscope.*

Did it ever occur to him to wonder whether other formalisms might be possible ? I fear that where logic is concerned, Godement's state of mind is that imputed[32] by Strachan-Davidson to the schoolboy who "believes in his heart that no nonsense is too enormous to be a possible translation of a classical author."

Let us list the other symptoms:

On page 25, there is a footnote:

> *it is very difficult, in practice, to use the sign $\Longrightarrow$ correctly.*[v]

On page 31, another footnote:

> *it is very difficult to use the signs ∃ and ∀ correctly in practice, and it is therefore preferable to write "there exists" and "for all", as has always been done.*[vi]

What will he do, I asked myself, with the set-forming operator ? The answer astonished me: he does not use it. I have been right through the book searching, and I cannot find it at all. He introduces signs for singletons and unordered pairs; but every time he wants to introduce a set, for example a coset in a group, he writes out in words "let F be the set of ....".

In Remark 5 of §1 N^{o}3, on page 44, Godement says

> *these examples show that the use of the word "set" in mathematics is subject to limitations which are not indicated by intuition.*[vii]

In Remark 6 he says

> *... apparently obvious assertions cease to be so simple when it is a question of effectively proving them. The Greeks were already*

---

32 according to Sir Donald Francis Tovey [T, page 10].

v *Il est du reste fort difficile, dans la pratique, d'utiliser *correctement* le signe $\Longrightarrow$.*

vi *Il est fort difficile d'utiliser *correctement* les signes ∃ and ∀ dans la pratique courante; il est donc préférable de se borner à écrire "il existe" et "pour tout" comme on l'a toujours fait.*

vii *Ces exemples montrent que l'usage du mot "ensemble" est soumis en Mathématiques à des limitations que l'intuition n'enseigne pas.*

aware of this.[viii]

and on page 98, in commenting on Example 1 of §5 N^{o}5, he says:

> *it is precisely one of Cantor's greatest achievements that he disqualified the use of "common sense" in mathematics.*[ix]

C·47 COMMENT The cumulative effect of all these comments is this: Godement tells the reader that a simple concept such as the number 1 can take thousands of signs to write out formally, that it is very difficult to use connectives correctly, and that it is very difficult to use quantifiers correctly. Coupled to this comprehensive group of negative messages about logic are some equally discouraging statements about set theory: that the concept of 'set' is counter-intuitive, that apparently obvious assertions are hard to prove, that common sense has been disqualified from set theory; further, he avoids the usual notation for forming sets and he evinces a remarkable reluctance even to state the axioms of set theory.

Can I be blamed for suspecting that Godement distrusts formalised reasoning ? I know he says, in §0 N^{o}1, on page 22, that

formalized mathematics exists only in the imagination of mathematicians[x]

but I feel he would rather even that did not happen. He brings to mind a remark of Padoa:

> *Logic is not in a good state: philosophers speak of it without using it, and mathematicians use it without speaking of it, and even without desiring to hear it spoken of.*

In sum, his message is that logic and set theory are a morass of confusion: but what has happened is that Bourbaki, whom he follows, have chosen a weird formalisation, they have noticed that in their chosen system proof is very awkward, and they have concluded that the whole thing is the fault of the logicians.

Nowhere, in Bourbaki or in Godement, is there any suggestion that other formalisations are possible. Godement says "mathematicians are impatient of vague statements", he explains that formality is a good thing, and then like a sharper forcing a card, offers you a choice of exactly one formalisation, and, at that, one that is cumbersome and destructive of intuition.

[viii] *Ceci montre que des assertions en apparence évidentes cessent d'être simples lorsqu'on veut effectivement les démontrer, c'est ce que les Grecs avaient déjà remarqué.*

[ix] *C'est précisement l'une des plus grandes réussites de Cantor que d'avoir pu disqualifier d'emploi du "bon sens" en Mathématiques.*

[x] *Elles n'existent bien entendu que dans l'imagination des mathématiciens.*

C·48 If their chosen system, Bou54, is what the Bourbachistes think logic and set theory are like, it is no wonder that they and their disciples are against those subjects and shy away from them. But on reading through Godement one last time, I was left with the impression that he is not so much a disciple of Bourbaki as a victim: loyalty to the group has obliged him to follow the party presentation of logic and set theory, and his intelligence has rebelled against it. I would love to teach him.

D: *It is this distrust, intensified to a phobia by the vehemence of Dieudonné's writings ...*

JEAN DIEUDONNÉ is acknowledged by many witnesses to have had a central, even a dominant, rôle in the successful functioning of the Bourbaki group: Armand Borel in his essay [Bor] mentions shouting matches, generally led by Dieudonné with his stentorian voice, and writes

> *"There were two reasons for the productivity of the group: the unflinching commitment of the members, and the superhuman efficiency of Dieudonné."*

Pierre Cartier in his interview [Sen] with the *Mathematical Intelligencer* describes Dieudonné as "the scribe of Bourbaki", and also makes, among numerous thoughtful points, these significant assertions:

Bourbaki never seriously considered logic.
Dieudonné himself was very vocal against logic.

D·0 That last disclosure is endorsed by a passage in Quine's autobiography [Q, page 433]:

> *"[In 1978] a Logic Colloquium was afoot in the École Normale Supérieure. [...] Dieudonné was there, a harsh reminder of the smug and uninformed disdain of mathematical logic that once prevailed in the rank and vile, one is tempted to say, of the mathematical fraternity. His ever hostile interventions were directed at no detail of the discussion, which he scorned, but against the enterprise as such. At length one of the Frenchmen asked why he had come. He replied 'J'étais invité.' "* [33]

There is admittedly a tradition of hostility to logic in France: one can look back to the suppression of Port Royal in the 17th century, and yet earlier to Abélard who lamented that his logic had made him odious to the world; in the twentieth century, Poincaré[34] quipped that "la logique n'est plus stérile, elle engendre des paradoxes"; Alexandre Koyré in "Epiménide le menteur"[35] wrote, less charitably: "la logique symbolique forme une discipline hybride, aussi ennuyeuse que stérile"; and papers of Tarski on the Axiom of Choice submitted to the *Comptes Rendus* in the late 1930s

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[33] An eye-witness has suggested that Quine might have been over-reacting to Dieudonné's characteristic behaviour.

[34] See Chapter II of [M-K] for some consequences for France of Poincaré's perceived position on logic.

[35] reviewed by Max Black in [Bl2]

were rejected by Lebesgue as absurd and then by Hadamard on the grounds that AC is a trivially obvious fact.

But it might be said that Bourbaki have continued that tradition, perhaps taking their cue from Poincaré without remarking that he was against logicism rather than against logic. We have seen the undertow in Godement's treatise; and if we look for further evidence of the antipathy of the Bourbachistes to logic, we find that the finger points at Dieudonné. So whether or not the Bourbaki bias was due solely to one extremely energetic man, it seems desirable to examine Dieudonné's position.

But that is easier said than done, for just as Leibniz has been spotted (notably by Couturat, following Vacca) saying one thing to the Queen of Sweden and saying another to his private diary, Dieudonné seems to have one opinion when he is thumping the tub on behalf of Bourbaki and another when he is musing as a private individual. Though his energy is evident, the coherence of his position is not.

Dieudonné's earlier writings on foundational questions were touched on in [M3]; here we look at his essay entitled *La Philosophie des mathématiques de Bourbaki* which is to be found on pages 27-39 of Tome I [Di2] of an anthology of his papers in two volumes published by Hermann of Paris in 1981 with the title *Choix d'Œuvres de Jean Dieudonné de l'Institut.*

The essay contains many good debating points and delightful jibes at various figures such as Russell and (to my surprise) Poincaré, but the title is strange, since it suggests that the essay will present or discuss the philosophy of mathematics of Bourbaki, yet the author is at pains to explain that Bourbaki have only ever made two statements about the philosophy of mathematics, and that the opinions expressed are his own.

I could find no reference, in that anthology, to any previous appearance of that essay, which on internal evidence seems to be the text of a talk given at, and towards the end of, a gathering of philosophers held not before 1977, since he cites his *Panorama* of mathematics, which was published in that year.

D·1 The first message of the paper is to warn his audience that philosophers of mathematics are unaware of the scope of modern mathematics, and whilst they believe they are discussing today's mathematics, in fact they are discussing that of the day before yesterday.

D·2 Comment That chimes with my experience of joint seminars on this theme: the philosophers know no mathematics and the mathematicians no philosophy, so that the interaction is weak. My readers should be similarly warned that Dieudonné is not discussing logic and set theory as understood in the first decade of the twenty-first century, but the logic and set theory of the first quarter of the twentieth.

D·3 So now Dieudonné will tell the philosophers what mathematicians are up to, and his second message is that 95% of mathematicians agree with Bourbaki and the rest are crazy, and arrogant with it.

He has two ways of saying “almost all” mathematicians: one is to say that 95% of mathematicians do this or think that; and the other is to say that the quasi-totality of mathematicians do that and say this. I am not sure whether the quasi-totality means at least 99.9%.

D·4 He considers that whereas at the beginning of the twentieth century foundational questions commanded the attention of many of the greatest mathematicians of the day, from 1925 onwards that has ceased to be the case, and that logic and set theory have become marginal disciplines for the quasi-totality of mathematicians:

> *“Au début du vingtième siècle .. les plus grands mathématiciens se passionaient pour les questions des “fondements” des mathématiques; aujourd’hui le divorce est presque total entre “logiciens” et “mathématiciens”. .. Il ne faut pas cesser de redire que, pour la quasi totalité des mathématiciens d’aujourd’hui, la logique et la théorie des ensembles sont devenues des disciplines* **marginales***: elles se seraient définitivement arrêtées après 1925 qu’ils ne s’en apercevraient même pas.*
>
> *... je ne parle pas d’opinions mais de* **faits***. Les travaux de Gödel, Cohen, Tarski, J. Robinson et Matijasevich n’exercent aucune influence.”*

Dieudonné makes both his position, and his ignorance of developments in logic, very clear. His choice of the date 1925 is puzzling: does he allude to the completion of Ackermann’s thesis ?

D·5 He pauses to swipe at the study of large cardinals.

> *“Les spéculations sur les “grands” cardinaux ou ordinaux laissent froids 95% entre eux, car ils n’en rencontrent jamais.”*

Let me pause too, to remark that the paper [M5] gives examples to show that there are straightforward problems of ordinary mathematics in which large cardinal properties prove to be embedded, even if ordinary mathematicians are not aware of the fact.

D·6 He warns that many philosophers unconsciously identify two parts of mathematics, logic-and-set-theory on the one hand, and the rest on the other,* which, he believes, are in fact strongly separated in the practice of mathematics: paragraph I 3 of the essay reads, in part:

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* I take that as an admission that logic-and-set-theory are indeed part of mathematics.

> *le divorce est presque total entre les mathématiciens s'occupant de Logique ou de Théorie des ensembles (que j'appellerai pour abréger "logiciens") et les autres (que j'appellerai simplement "mathématiciens", pour ne pas toujours dire "mathématiciens ne s'occupant pas de logique ni de théorie des ensembles.")*

D·7 Dieudonné then says he will give special meanings to the words "logicians" and "mathematicians"; I shall indicate those by capital letters; thus he defines Logicians to be mathematicians concerned with logic or set theory; and Mathematicians to be mathematicians-concerned-with-neither-logic-nor-set-theory.

Presumably from that point on in his essay, he intends those words to be used in that exclusive sense, and supposes that every mathematician is either a Mathematician or a Logician, but not both.

But how exclusive is it ? A mathematician who works in several areas, one of which is logic, need not have logic on the brain the whole time. Littlewood, for example, once wrote a paper on cardinal arithmetic, but no one has ever accused him of being a logician. Everyone would call Shelah a logician; when he solved Kuroš' problem by proving that there is an uncountable group with no uncountable subgroup, he harnessed small cancellation theory to ideas from combinatorial set theory. When he gave primitive recursive bounds for van der Waerden's theorem, was he doing number theory or was he doing recursion theory ?

How, for example, would Dieudonné have categorized Louveau ?

D·8 **How, indeed, would he categorize himself ?** On pages 354–356 in the same volume [Di2] of Selected Papers we find a short note called "Bounded sets in (F)-spaces", first published in the Proceedings of the American Mathematical Society **6** (1955) 729–731.

He asks two questions about locally convex metric spaces, and **using the Continuum Hypothesis** – ho, ho, ho – gives a counterexample to each.**

He writes "It would be interesting to give negative answers without the continuum hypothesis". Without looking at the specific questions asked, we might remark his implied belief that a positive answer without the continuum hypothesis would not be interesting. Indeed it might place the question beyond mathematics, for it would then not have been answered within the scope of ZF.

Suppose his question had been equivalent to CH: would that be of

** He uses a lemma, assuming the Continuum Hypothesis, the conclusion of which is easily equivalent to the assertion that $\mathfrak{d}$, the dominating number, equals $\aleph_1$.

interest ? Or is it that CH to him is only a prop for a pre-proof which, he hopes, will eventually discard that prop ?

In his essay he declares that for the quasi-totality of today's Mathematicians logic and set theory have become marginal disciplines. — How did he view his own use of the continuum hypothesis ? He thought his paper worth including in a selection made 26 years after the paper was originally published.

D·9 We have heard Dieudonné say that Gödel, Cohen, Tarski, Julia Robinson and Matijasevic, profound though their work is, have exerted no influence (positive or negative) on the solution of the immense majority of problems which interest Mathematicians.

Is that true ? What one finds on surveying the mathematical scene is a slippery characteristic: a problem, thought to be Mathematical, which proves to have a solution using ideas from Logic, is liable to be declared by those formerly interested in it to have revealed itself to be unimportant. One might cite here the response of the late J. Frank Adams to the information that Shelah had solved Whitehead's problem concerning free Abelian groups: "Whitehead would only have been interested in the countable case".♣

There are many problems stated by Mathematicians to be of interest and which are later proved, usually by Logicians, to have a Logical component. The continuum hypothesis was included by Hilbert — a Mathematician or a Logician in Dieudonné's eyes ? — in his famous list of 1900, and so was the search for an algorithm for the solution of Diophantine equations. What is this strange interest that is extinguished should the problem prove not to have an answer of the kind simple Mathematicians seem to expect ?

D·10 Dieudonné claims to be speaking of facts; indeed he might almost be claiming, in Sir Herbert Butterfield's immortal phrase, that the 'facts' are being allowed to 'speak for themselves'. But the facts are not that clear. On pages 4 and 5 of their 1958 book on the *Foundations of Set Theory*, (and on page 4 of the revised edition [Fr-bH-L]), Fraenkel and Bar-Hillel write that "Nevertheless, even today the psychological effect of the antinomies on many mathematicians should not be underestimated. In 1946, almost half a century after the despairing gestures of Dedekind and Frege, one of the outstanding scholars of our times made the following confession", and they then quote these words of Hermann Weyl[36]:

♣ Have any of my readers seen anything in Whitehead's writings to support Adams' contention ?

36 in *Mathematics and Logic*, a brief survey serving as preface to a review of "The philosophy of Bertrand Russell", *Am. Math. Monthly* **53** (1946) 2–13.

> *"We are less certain than ever about the ultimate foundations of (logic and) mathematics. Like everybody and everything in the world today, we have our "crisis". We have had it for nearly fifty years. Outwardly it does not seem to hamper our daily work, and yet I for one confess that it has had a considerable practical influence on my mathematical life: it directed my interests to fields I considered relatively "safe", and has been a constant drain on the enthusiasm and determination with which I pursued my research work."*

Weyl's statement rebuts Dieudonné's suggestion that foundational work has had no influence: Weyl, for one, was influenced to move away from areas where the paradoxes had manifested themselves.

So I am not convinced by Dieudonné's attempt to link logic and set theory and separate them from the rest of mathematics; each mathematician sets his own boundaries; there are among my acquaintanceship mathematicians with whom I have fruitful exchanges when I am in set-theorist mode, but whose eyes glaze over should I ever say anything to them when I am in logician mode.

D·11 In the section on *les conceptions de Bourbaki*, he says that Bourbaki have invented nothing but have restricted themselves to making explicit the practice of those mathematicians called "formalists", who, as we shall see, (or rather, as Dieudonné will tell us) form the quasi-totality of mathematicians of today.

II.7 Bourbaki has only two things to say: *all are free to think what they will about the nature of mathematical entities or about the truth of the theorems they use, so long as their proofs can be transcribed into the common language*

D·12 ASIDE — the common language being that imposed by Bourbaki —

and as for contradictions, Bourbaki *believes in doing nothing till an actual contradiction occurs.*

D·13 COMMENT Really what he is saying is that most mathematicians are content to be ignorant, and Bourbaki wishes to reinforce that ignorance.

> *Formalists are interested in objects ... the interpretations of a system of signs that is subject to a rigorous syntax that is independent of any interpretation.*

D·14 COMMENT I suspect Dieudonné thinks, reasonably enough, that all minds are different and that the common ground is to be subscription to a particular syntax.

In the syntax are the rules of classical logic and axioms of ZF.

D·15 Comment That statement is false, for Bourbaki omit the axiom of foundation, which is not derivable from the axioms that they give: the issue, for Logicians, here is that of collection versus replacement.

Dieudonné shirks the question of why these particular rules should be adopted; the "common language" appears to be one imposed by fiat, of which no discussion is permitted, and hence the intuition is paralysed.

II.3: ZF *gives only the bare theory of sets, which 95% at least of today's Mathematicians consider to be without interest.*

D·16 Comment I wonder what Dieudonné means by "the bare theory of sets". It might be that Dieudonné is confusing two things: the theory of sets and the theory of **Set**, the category of sets, of which the objects are indeed sets without any structure, about which, taken in isolation, there would be little to say. But post-Zermelo set theory studies the membership relation $\in$, and that relation, when the axioms of ZF are assumed, provides so rich a universe that large amounts of mathematics can be expressed within it.

II.4: *the axioms of a structure are disguised definitions.*

D·17 Comment One feels that for Dieudonné a structure is a type rather than a token.

D·18 Bourbaki evidently believe they have provided a formalism that is adequate for the quasi-totality of Mathematicians. They also seem very keen that no-one should examine the formalism that they provide. They have not explained why their chosen axiomatisation—somewhere between ZFC without foundation and ZF with global choice—is to be the standard. Indeed, given that Bourbaki, as Corry [Cor1, 2] has shown, ignore their own foundations, why should others be expected to use them ?

II.6 *The quasi totality of Mathematicians are naïf pre-Cantorians.*

D·19 Comment Is that desirable ? Grothendieck in his re-structuring of algebraic geometry needed some set theory, but he only knew the cruder parts of set theory as presented in Bourbaki's book: how might his work have advanced if he had had the subtler parts of set theory at his fingertips ?

D·20 Comment As for the "pragmatic" suggestion that no one should think about possible contradictions: in one way it is sensible; in another it is idiotic. Dieudonné has used CH: was Gödel wasting his time in seeking and finding a consistency proof for it ? I am not saying that people *should* go out of their way to think about possible contradictions; but, as Saccheri found, looking for a consistency proof is much the same as looking for an inconsistency proof; and sometimes one finds a "proof" of a contradiction which turns out to be a proof of something quite different.

D·21 In the résumé Dieudonné says that Bourbaki's system is implicit in the work of the quasi-totality of mathematicians; and they are all happy to be naïve about philosophical questions. Is that *really* so desirable ?

Dieudonné in effect is saying in a patronising manner,

> **"NO NEED FOR ANY OF YOU YOUNG CHAPS TO WORRY YOUR HEADS ABOUT FOUNDATIONS, WE'VE DONE IT ALL FOR YOU."**

That is a dangerous position, for it is an attempt to gag the future. Look how Gordan criticised Hilbert for his refusal to present a certain proof within the then accepted rules; or turn to the book [Ad] of J. Frank Adams. On page 293, in section 14, "A category of fractions", he writes:

> *"(Added later.) I owe to A.K.Bousfield the remark that the procedure below involves very serious set-theoretical difficulties. Therefore it will be best to interpret this section not as a set of theorems, but as a programme, that is, as a guide to what one might wish to prove."*

and on page 295, presumably discussing the same difficulty, he writes:

> *"(Added later: unfortunately there is no reason why the result should be a small category.)"*

In other words, he has essayed a localisation construction, and it founders on his inadequate grasp of set theory. Later Bousfield [Bous1,2] found a rigorous, set-theoretically correct replacement for Adams' ill-founded argument. and very recently Fiedorowicz [AF] has shown how to correct Adams' original lectures.

D·22 The point is enriched by a paper of Casacuberta, Scevenels and Smith [CaScS] entitled "Implications of large-cardinal principles in homotopical localization." They find that the question "Is every homotopy idempotent functor equivalent to localization with respect to some single map ?", which was motivated by Bousfield's discovery, cannot be answered in ZFC, for they find that their localisation principle actually implies the existence of fairly large cardinals, and follows from the existence of the even stronger hypothesis known as Vopěnka's principle; thus their localisation principle inescapably involves those same large cardinals that Dieudonné was so confident♠ the quasi-totality of mathematicians would never encounter.

More recently [BCM, BCMR], Bagaria, Casacuberta, Rosický and the present author have obtained similar results from a weaker hypothesis than Vopěnka's principle, namely the existence of a supercompact cardinal. Casacuberta and Rosický think in terms of categories, whereas Bagaria and I are set theorists, and we have had to keep translating from one perspective

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♠ as was Mac Lane.

to the other, each perspective contributing to the outcome; a set-theoretical argument having to be re-worded so that it might have meaning in categories where the objects do not have members; an example from category theory having to be explained to set theorists. The experience has only reinforced my belief in the necessity of a pluralist account of the foundations of mathematics.

D·23 To look briefly at the other non-mathematical essays in [Di2]: in his piece on *L'évolution de la pensée mathématique dans la Grèce ancienne*, Dieudonné argues against Launcelot Hogben's view that mathematics is utilitarian. Thus he attacks logic using the same weapons that others use to attack mathematics, attacks he would wish to repulse.

In his essay, *Liberté et Science Moderne,* he writes: "*Il faut, pour pouvoir faire des découvertes en science, avoir l'audace de contredire les idées reçues*" — a bit rich, that, but not as rich as his final paragraph on the philosophy of Bourbaki:

> *"La plus charitable hypothèse est de penser que cela n'est dû qu'à l'ignorance, ou au refus de s'informer, ou à l'incompréhension; sinon, il faudrait concluire qu'il s'agit d'illuminés aveuglés par leur fanatisme, et que la "crise" qu'ils croient voir dans les mathématiques d'aujourd'hui ne se trouve que dans leur cerveau."*

In *L'abstraction et l'intuition mathématique* he talks sense: he says there is more than one intuition—precisely the grounds on which I argue for a pluralist view of the foundations of mathematics—and that there are transfers between intuitions; here and in *Liberté* he is arguing in favour of those conditions that favour creativity; precisely those conditions which, if claimed by logicians, he so strongly rejects.

D·24 It should perhaps be remarked that in the first volume *Foundations of modern Analysis* [Di1] of his treatise, Dieudonné, though referring the reader to [Bou54] for a formal axiomatisation, lists in his Chapter I axioms of a system that is, one ambiguity aside, essentially Bou49 and not Bou54: the axioms he mentions are extensionality, singleton, power set, the scheme of separation, ordered pairs, cartesian products, and the axiom of choice. He discusses families of subsets of a given set; the ambiguity is that when, later, he enunciates the principle that the union of a countable family of countable sets is countable he does not state that the elements of the family are required to be subsets of a given set, though he might have intended that restriction, since he offers a proof. But the proof reads slightly oddly, since it uses, as it must, the axiom of choice, but does not mention it. On the other hand Dieudonné quite unnecessarily states as an additional axiom the easy consequence of the axiom of choice, that each infinite set has a denumerably infinite subset.

D·25 In his last book, [Di3], Dieudonné makes the same mistake that he made in his position papers of fifty years previously: he went to his grave believing that truth and provability are identical.

E: ... and fostered by the errors and obscurities of a well-known undergraduate textbook,

TURN NOW TO A TEXTBOOK that has had a wide following in France: *Tome 1, Algèbre,* of the *Cours de mathématiques* by Jacqueline Lelong-Ferrand and Jean-Marie Arnaudiès, anciens élèves de l'École normale supérieure. This book is the first of a four-volume treatise of mathematics, which is described by Alain Pajor as a classic which students often consider to be difficult and use like a dictionary. Its first edition was in 1978; I work from the third edition, of 1995. Ominously, among the works cited in its *Bibliographie* are these two:

N. Bourbaki, *Théorie des ensembles,* published by Hermann in 1957;
R. Godement, *Cours d'algèbre,* Hermann, edition of 1966;

and the influence of those two works reveals itself in the choice and the bias of the presentation of logic and set theory in Chapter I of this textbook, which chapter, though, displays a further degeneration of clarity, correctness and coherence as compared with the above two sources. Indeed I propose to criticise their Chapter I rather severely. In doing so I aim to show, first, that what the authors have to say on foundational themes is often lamentably vague and, worse, in places actually false; and, secondly, that, some of their Delphic pronouncements being only interpretable in the light of their sources, their account must be seen as deriving from those of Hilbert, Bourbaki and Godement, and thus as continuing the descent into incoherence; from this perspective the banning of logic from the CAPES might be a reasonable consequence of the belief that seems to have taken hold, that the subject is too messy to inflict on the young and on their teachers.

My typographical conventions in this section: passages in *slanted type*, if in French, are taken direct from their text; if in English, are my translation of a passage from the text. Passages in roman type are my commentary on their text, with **bold face** used to emphasize certain of my comments. I use *italic type* to highlight certain phrases, most often reproducing a highlight in their text. A centred section heading **[L-F,A] I.2** marks the beginning of my main criticism of their Chapter I, section 2. A marginal reference [LFA] p2,-6 will be to page two of their book, line 6 from the bottom.

The first four divisions of [L-F, A] Chapter I are labelled

1. *Notions sur la formalisation*
2. *Règles de logique formelle*
3. *Quantificateurs*
4. *Opérations sur les ensembles*

and contain respectively

1. remarks on the need for formalisation, and some comments on syntax
2. remarks on and some axioms for propositional logic
3. remarks on the meaning of ∃ and on the Axiom of Choice
4. introduction of certain set-theoretic axioms.

Each of those four requires criticism; in the rest of the chapter, as the authors get closer to areas of mathematics with which they are familiar, the need for comment diminishes but a few remarks will still be necessary.

The order in which topics are treated is sometimes unexpected; there is an early mention, on page 2, of the need for an axiom asserting the existence of the set of natural numbers; on the other hand a definition of "finite" is not given till page 32, though at some language level the concept is being used on many previous pages, such as 10, 11, 12, 17, 20, 21.

The bulk of the discussion of the Axiom of Choice takes place before most of the axioms of set theory have been presented; this ordering of topics might be the result of the implicit reliance on the epsilon operator and its consequent representation of the Axiom of Choice as a principle of logic rather than of set theory.

[L-F,A] I.1: Syntax

The authors begin by commenting briefly that certain paradoxes can be avoided by constructing formalised languages, which are less expressive than natural languages, and then developing formal logics. Thus the theory of sets may be built up by the axiomatic method:

[LFA] p 1		
[LFA] p 1	*On se donne un petit nombre de signes logiques, et un petit nombre de règles permettant, à l'aide de ces signes, et des lettres des divers alphabets, d'écrire des "mots permis". (Le plus souvent, les mots permis s'appellent des assemblages.)*	*One gives oneself a small number of logical signs, and a small number of rules permitting with the aid of these signs and letters of various alphabets, the writing of "permitted words". (The permitted words are most often called assemblies.)*

E·1 Comment Although they do not set out the rules of formation of languages, I suspect they intend only formulæ with quantified set variables to be considered.

[LFA] p 1 *On se donne un moyen de distinguer deux sortes de mots permis: les uns, appelés* termes*, seront les répresentants abstraits des objets sur lesquels on fera des raisonnements; les autres, appelés* relations *représenteront les assertions que l'on peut faire sur ces objets. Puis on se donne des règles régissant l'usage des relations, permettant de construire de nouvelles relations à partir de relations données, etc. ces règles sont les règles de logique formelle.*

Among the permitted words or assemblies there are terms, (which are abstract representations of the objects about which one will reason) and there are relations which represent the assertions that one can make about these objects. Then one gives oneself some rules governing the usage of relations, permitting the construction of new from old; these are the rules of formal logic.

E·2 COMMENT We see that whereas Bourbaki held to a strictly formalist line that their system was an uninterpreted calculus, the present authors allow a difference between terms and the objects that are their interpretation. However they give no rules for deciding which assemblies are terms.

[LFA] p 1 *Cela étant fait, la notion de verité mathématique est "relativisée" de la manière suivante: on pose un petit nombre de relations, appelées* axiomes*, comme vraies* a priori*. Puis on définit la notion de* démonstration*.*

The notion of mathematical truth is "relativised" in the following manner: a small number of relations, called axioms*, are supposed true* a priori*. Then one defines the concept of a* formal proof*.*

E·3 COMMENT As with defining the notion of "term", the authors leave the notion of a formal proof all too vague. It is not clear what are the axioms nor what are the rules of inference.

Two most revealing remarks are these:

[LFA] p 2 *une relation est alors dite* vraie *si elle peut être insérée dans une démonstration.*

A relation is called true *if it may be inserted in a proof.*

[LFA] p 3 *une relation est* vraie *si on peut l'insérer dans un texte démonstratif.*

A relation is true *if one may insert it in a demonstrative text.*

E·4 COMMENT This equation of truth and provability is, of course, standard for horses from the Bourbachiste stable. But it is far from satisfactory.

There has been a suggestion that any relation might be taken to be an axiom, and therefore might be used in a proof. For example, both the Continuum Hypothesis, CH, and its negation have interesting consequences. So either of them might be used in a proof, and therefore, in this odd meaning of “true”, both are true.

Thus the notion of “true” is imprecise; mutually contradictory systems certainly exist.

On page 5, they speak of *axiomes momentanés*, thus acknowledging that one might wish to postulate hypotheses for an argument, to be discharged once the argument is complete.

[LFA] p2, 6 *Lorsqu’on dispose d’un langage formalisé cohérent pour fonder une théorie (la théorie des ensembles, ou toute autre théorie mathématique), le développement de cette théorie consiste à en trouver les relations vraies, auxquelles on donne le nom de théorèmes, propositions, lemmes, scholies etc.*

Once one has a formalised language available for basing a theory (such as the theory of sets, or any other mathematical theory) the development of that theory consists of finding true relations which one calls theorems, propositions, lemmata, scholia, &c.

[LFA] p2,-6 *Signalons qu’on peut construire toutes les mathématiques connues à ce jour à l’aide des axiomes et du langage formalisé de la théorie des ensembles; l’axiome fondamental ’etant l’existence d’au moins un ensemble de nature mathématique: celui des nombres entiers.*

Let us note that one can construct all the mathematics known today with the aid of the axioms and formalised language of the theory of sets; the fundamental axiom (for mathematicians, at least) is the assertion of the existence of at least one set of a mathematical kind, namely the set of whole numbers.

[LFA] p 3 *Mais en pratique, il est impossible d’écrire toutes les mathématiques en langage formalisé: pour le moindre théorème facile, il faudrait des livres entiers. On est donc amené à utiliser des abréviations et du langage courant; et on se contente d’écrire des textes “dont on est sûr” qu’on pourrait les formaliser.*

But in practice it is impossible to write mathematics entirely in a formal language; for even the easiest theorem it would need whole books. So one contents oneself with writing texts of which one may be sure that they could be formalised.

E·5 Comment The claim made in the second of those three paragraphs echoes that at the end of Bourbaki’s address [Bou49] and is shown in [M10] to be erroneous. We shall also see that, curiously, the authors do not give the existence of **N**, the set of natural numbers, as an axiom, but state that its existence can be derived from a less precisely phrased assertion of the existence of an infinite set.

E·6 COMMENT The third paragraph, of course, echoes the remarks of Bourbaki and Godement about the length of expansions of the formulæ of their chosen language.

[L-F,A] I.2: propositional logic

[LFA] p 3 *Étant donnée une relation A, on définit son contraire, notée non A; (non A) est aussi appelée la négation de A. Par définition, A est fausse si (non A) est vraie.*

Given a relation A, one defines its contrary, written (not A); (not A) is also called the negation of A. By definition, A is false if not-A is true.

E·7 COMMENT What does "false" mean ? Perhaps that is a definition. But how to know if not-A can be used in a proof ?

[LFA] p 3 *S'il existe A telle que A et (non A) soient vraies, la théorie est dite* contradictoire *et on démontre qu'alors toute relation de la théorie est vraie. Bien que cela n'ait pas été démontré, on pense généralement que la théorie des ensembles n'est pas contradictoire, de sorte que pour toute relation A de cette théorie, l'une au plus des relations A et (non A) est vraie.*

If for some A, both A and not-A are true the theory is called contradictory, and then every relation of the theory may be proved to be true. It is generally supposed that the theory of sets is not contradictory, although that has not been demonstrated, so that for each relation of this theory at most one of the relations A and (not A) is true.

E·8 COMMENT As often for writers of the Bourbachiste persuasion, Gödel is a non-person and no mention is made of the reason why the consistency of set theory has not been demonstrated.

They go on to mention the possibility that there are some statements that are neither provable nor refutable; and seek to marginalise this shocking phenomenon in the standard Bourbachiste way:

[LFA] p 3 *Il ne faut pas croire que l'une des deux relations A et (non A) soit forcément vraie: il pourrait existait en effet des relations contraires A et (non A) telles qu'aucune des deux ne puisse être insérée dans une texte démonstratif; A est alors dite indécidable. En pratique nous ne rencontrerons pas de relation indécidable. !*

It should not be thought that necessarily one of the relations A and (not A) is true: there might exist in fact contrary relations A and (not A) such that neither of them can be inserted in a demonstrative text; A is then said to be undecidable. In practice we do not meet them.

E·9 COMMENT That was Godement's original hope and also Dieudonné's, except of course when he found he could use the Continuum Hypothesis to construct a counterexample to something. But whereas Godement by 1969 was acknowledging the existence of undecidable statements of ordinary mathematics, these followers of his were still denying that existence in 1995.

[LFA] p 3 *Étant données deux relations A et B on définit la disjonction de A et B, notée: (A ou B). Si l'une au moins des relations A, B est vraie, (A ou B) est vraie.*

Given two relations A and B, one defines their disjunction, written A-or-B. If at least one of A or B is true then (A-or-B) is true.

E·10 COMMENT That seems much too vague; how might one say that A-or-B is the strongest such statement ? Indeed below, they remark that (A or (not A)) is always true, whereas above they have admitted that for certain A, neither A nor (not A) is true.

[LFA] p 3 *La relation ((non A) ou B) s'appelle l'implication de B par A, et se note*

$$A \implies B.$$

Si A et ($A \implies B$) sont vraies, B est vraie; si B est vraie, ($A \implies B$) est vraie pour toute relation A.

The relation (not-A or B) is called the implication of B by A, and is written

$$A \implies B.$$

If A and $A \implies B$ are true, so is B. If B is true, $A \implies B$ is true for every A.

E·11 COMMENT Of those two comments, the first is *Modus Ponens*, the rule of inference given in Bourbaki and used in *Principia Mathematica.* The second is a derived rule, given as C9 in Bourbaki.

[LFA] p 3 *De plus, nous admettrons les relations ci-dessous, qui fournissent des règles de raisonnement:*

1) *A* $\Longrightarrow$ *A. Cette relation est très interessante, car elle exprime que (A ou (non A)) est toujours vraie, (même si A est undécidable !)*
2) *(A ou A)* $\Longrightarrow$ *A.*
3) *A* $\Longrightarrow$ *(A ou B).*
4) *(A ou B)* $\Longrightarrow$ *(B ou A)*
5) *(A* $\Longrightarrow$ *B)* $\Longrightarrow$ *[(C ou A)* $\Longrightarrow$ *C ou B].*
6) *(A* $\Longrightarrow$ *B)* $\Longrightarrow$ *[(B* $\Rightarrow$ *C)* $\Longrightarrow$ *(A* $\Rightarrow$ *C)].*
7) *(A* $\Longrightarrow$ *(non(nonA)).*
8) *[A* $\Longrightarrow$ *B]* $\Longrightarrow$ *[(non B)* $\Longrightarrow$ *(non A)]*

Parmi les règles ci-dessus, les règles numéros 1), 2), 3), 4), 5) sont des axiomes *dans la plupart des logiques formelles usuelles.*

Further, we shall admit the following relations, which supply the rules of reasoning:

1) *A* $\Longrightarrow$ *A. This relation is very interesting, for it says that (A or (not A)) is always true (even if A is undecidable !)*
2) *(A or A)* $\Longrightarrow$ *A.*
3) *A* $\Longrightarrow$ *(A or B).*
4) *(A or B)* $\Longrightarrow$ *(B or A)*
5) *(A* $\Longrightarrow$ *B)* $\Longrightarrow$ *[(C or A)* $\Longrightarrow$ *C or B].*
6) *(A* $\Longrightarrow$ *B)* $\Longrightarrow$ *[(B* $\Rightarrow$ *C)* $\Longrightarrow$ *(A* $\Rightarrow$ *C)].*
7) *(A* $\Longrightarrow$ *(not(notA)).*
8) *[A* $\Longrightarrow$ *B]* $\Longrightarrow$ *[(not B)* $\Longrightarrow$ *(not A)]*

Of the above rules, numbers 1) to 5) are axioms in most customary logical systems.

E·12 Comment Of those axioms, 2) to 5) are exactly S1, S2, S3, S4 of Bourbaki and essentially *1.2, *1.3, *1.4 and *1.6 of *Principia Mathematica* and AL1, AL2, AL3 and AL4 of Godement. The others are consequences of them, 1), 6), 7), 8) being respectively C8, C6, C11, C12 of Bourbaki and TL2, TL1, and essentially TL3, and TL4 of Godement.

[LFA] p 4 *Si A et B sont de relations, on définit la* conjonction *de A et B, notée (A et B): c'est la relation:*

$$\textit{non}\ [(\textit{non}\ A)\ \textit{ou}\ (\textit{non}\ B)].$$

On dit que A et B sont équivalentes *si* $(A \Longrightarrow B)$ *et* $(B \Longrightarrow A)$ *sont vraies, on écrit alors* $(A \Longleftrightarrow B)$.

If A and B are relations, their conjunction, in symbols (A et B), is defined as the relation

$$\textit{not}\ [(\textit{not}\ A)\ \textit{or}\ (\textit{not}\ B)].$$

One says that A and B are equivalent *if the two implications are true. One then writes* $A \Longleftrightarrow B$.

E·13 COMMENT As far as I can tell, that implies that $(A \iff B) \iff C$ is meaningless. According to the text, the sequence of symbols "$A \iff B$" expresses the conjunction of the two statements

"$A \implies B$" *can be inserted in a proof*

and

"$B \implies A$" *can be inserted in a proof*

and is thus not a formula but a statement about two formulæ and a theory. But if Φ is a statement about a system and C is a formula of the system, neither $C \implies \Phi$ nor $\Phi \implies C$ can be inserted in a proof since neither is a formula of the system.

[L-F,A] I.3: predicate logic and the Axiom of Choice

[LFA] p 6

Nous écrirons

(1) $\exists x, \quad A(x)$

pour exprimer la relation "la relation $A(x)$ est vraie pour au moins un objet x". Cette définition n'est qu'intuitive, nous ne ferons que décrire les règles d'usage du symbole $\exists$, appelé quantificateur existentiel.

We write

(1) $\exists x, \; A(x)$

to express the relation "the relation $A(x)$ is true for at least one object x". This definition is only intuitive, we only make it to describe the rules of use of the symbol $\exists$, which is called the existential quantifier.

E·14 COMMENT Here we have a confusion of language levels. Relations have been defined as certain assemblies, that is certain strings of formal symbols. The statement within quotation marks is not a relation; it is a (French) phrase about relations and objects. So the authors have achieved precisely that confusion of language levels (as exploited by the paradoxes) that the introduction of formalised languages was intended to avoid.

[LFA] p 6	*Nous écrirons* $$\forall x, A(x)$$ *pour exprimer la relation* (2) *non*$(\exists x,$ *non*$A(x))$ *Cette relation signifie que la propriété $A(x)$ est vraie de tous les objets x.*	*We write* $$\forall x,\ A(x)$$ *to express the relation* (2) *not-*$(\exists x,$ *not-*$A(x))$ *This relation signifies that the relation $A(x)$ is true for all objects x.*

E·15 COMMENT We verge on a problem of ω-incompleteness here. In a theory of arithmetic it could easily be the case that each $A(\mathfrak{n})$ is provable but that $\forall n A(n)$ is not; if objects are the same as terms and truth is provability, then "the relation $A(x)$ is true for all objects x" says that each $A(\mathfrak{n})$ is provable, but that does not mean that $\forall n A(n)$ is provable.

E·16 REMARK The authors state that the formulæ (1) and (2) and the rules of logic permit the mechanical use of quantifiers. But what are those rules ? They are not stated, though some examples are given.

page 7: the problem of choice

We come now to the six paragraphs that Lelong-Ferrand and Arnaudiès devote to commenting on the nature of the Axiom of Choice. Here I use the sign ¶ to mark the start of each discussion of one of the six.

¶1 The first mentions that there is a problem concerning the meaning of an existential statement: some great mathematicians such as Émile Borel have not believed non-constructive proofs of an existential statement.

[LFA] p 7	*Intuitivement, le problème se présente comme il suit: peut-on démontrer, dans une théorie donnée, un théorème de la forme:* $(\exists x,\ A(x))$ *sans construire, par un procédé descriptif, un objet x pour lequel la relation $A(x)$ est effectivement vraie ?*	*Intuitively the problem is this: can one prove, in a given theory, a theorem of the form* $(\exists x, A(x))$ *without constructing, by a descriptive procedure, an object x for which the relation $A(x)$ is actually true ?*

E·17 COMMENT Those with a taste for constructive proofs usually eschew the Axiom of Choice.

¶2 The second, dreadfully confused, and actually wrong (rather than stylistically undesirable or misleading) paragraph alleges that the axiom says that when an existential formula is true one can always formally construct a witness:

[LFA] p 7 *On a vite reconnu la nécessité d'introduire, en théorie des ensembles, un axiome appellé axiome du choix:* grosso modo, *cet axiome dit que lorsqu'une relation du type $\exists x$, $A(x)$, est vraie, on peut toujours construire formellement un objet x pour lequel $A(x)$ est vraie.*	*The necessity of introducing an axiom called axiom of choice into the theory of sets was quickly recognised; roughly, the axiom says that when a relation of the type $\exists x, A(x)$ is true, one can always formally construct an object x for which $A(x)$ is true.*

E·18 Comment **That is false:** the Axiom of Choice implies, for example, that there is a well-ordering of the continuum, but that is perfectly compatible with there being no definable such.

One wonders if this mistake is related to one noted by Alonzo Church in his 1948 review [Chu] of "L'énumeration transfinie. Livre I. La notion de rang", by Arnaud Denjoy [De]:

> *"in an otherwise excellent work, the treatment (pp 5, 110-116) of the Axiom of Choice and of Zermelo's theorem that every class can be well-ordered, is without value, because the author mistakenly identifies the Axiom of Choice with the proposition that every non-empty class has a unit subclass."*

¶3 The third paragraph says that this axiom has many equivalent formulations, of which the best known are Zermelo's axiom concerning well-ordered sets, and the theorem of Zorn.

E·19 Comment True, in that the Axiom of Choice, as usually understood (but not as presented by our authors) is indeed equivalent, as proved by Zermelo, to the proposition that every set can be well-ordered, and as proved by Zorn, to the proposition known in Anglophone countries as Zorn's Lemma.

¶4 The fourth paragraph is highly revealing:

[LFA] p 7 *Dans la mathématique formelle usuelle, l'axiome de choix est introduit dès le départ, à l'aide d'un signe logique. Dans la théorie des ensembles ainsi construite, le symbole $\exists x,\ A(x)$ n'est qu'une abréviation pour exprimer, en quelque sorte, que l'objet théorique qu'il est possible de construire et qui vérifie $A(x)$, vérifie effectivement cette relation.*	*In mathematics as usually formalised, the Axiom of Choice is introduced at the start by the aid of a logical sign. In this presentation of the theory of sets, the symbol $\exists x, A(x)$ is only an abbreviation for expressing in some manner that the theoretical object that it is possible to construct and which satisfies $A(x)$ does indeed satisfy this relation.*

E·20 COMMENT I have no idea how to interpret their remarks; but I can say where they came from, namely the use of the Hilbert ε-operator, the one called τ by Bourbaki.

¶5 The fifth paragraph remarks correctly that set theories without the Axiom of Choice have been studied, and that one can therefore classify results according to their dependence or otherwise on that axiom.

¶6 The sixth and last paragraph on page 7 lists some example of existence statements that require the Axiom of Choice for their proof; their first example is erroneous, though the others are correct. The statement that the Axiom of Choice is needed to prove the existence of an arbitrary product is inaccurate; AC is needed to prove that the product is non-empty if the factors are, not that the product exists.

Fortunately, at the top of page 21, they state correctly that:

Si, pour tout $i \in I, A_i \neq \varnothing$, $$\text{alors} \prod_{i\in I} A_i \neq \varnothing.$$ *Cette propriété est un axiome équivalent à l'axiome du choix.*	*If, for each $i \in I, A_i \neq \varnothing$,* $$\text{then} \prod_{i\in I} A_i \neq \varnothing.$$ *This property is an axiom equivalent to the Axiom of Choice.*

[L-F,A] I.4: Operations on sets

The authors now discuss various operations on sets, and mention various justificatory axioms. But they make conflicting statements: at the top of page 8 they say that a set is a term equipped with a relation $\in$. Lower down, they say that it is hard to tell which terms are sets.

[LFA] p8, 1	*Nous admettons la notion d'ensemble. Un ensemble est donc un terme, muni d'une relation:* $\in$. *— La relation :* $a \in E$ *se lit "a appartient à E"* ⋮	*We accept the notion of a set. A set is then a term, equipped with a relation:* $\in$. *— The relation :* $a \in E$ *is read "a belongs to E"* ⋮
[LFA] p8,-11	*On n'a aucun moyen effectif de reconnaître si un terme donné est un ensemble; aussi la théorie des ensembles procède par construction, à partir de termes dont on admet qu'ils sont des ensembles (par exemple,* **N***).*	*We have no effective means of telling whether a given term is a set; so the theory of sets proceeds by construction, starting from those terms that are acknowledged to be sets, (for example* **N***).*

E·21 Comment They are, I suspect, in the condition of the patient in Laing's double-bind model: they have been given contradictory statements by people they regard as authorities. On the one hand, Zermelo held that sets are those classes which are small enough to be members of some class, whereas proper classes are those classes which are too big to be a member of any class. So if one has a class $\{x \mid R\}$, to say that it is a set is to say that $\exists y\, y = \{x \mid R\}$; and there are certain classes, such as the Russell class $\{x \mid x \notin x\}$ of which the set-hood is refutable. On the other hand, we saw in Section B that Bourbaki's use of the symbol $\{x \mid R\}$ is not the same as Zermelo's. With Bourbaki, each term is, by syntactical trickery, provably equal to some set; thus

$$\vdash_{\mathsf{Bou54}} \exists y\, y = \{x \mid x \notin x\} \quad \text{whereas} \quad \vdash_{\mathsf{ZF}} \neg\exists y\, y = \{x \mid x \notin x\}$$

So at the top of page 8, the authors are with Bourbaki, but lower down they are with Zermelo.

The translation into Bourbaki's dialect of set theory of the assertion in Zermelo's dialect that the class $\{x \mid R\}$ is a set, is the formula $Coll_x(R)$. Reassuringly,

$$\vdash_{\mathsf{Bou54}} \neg Coll_x(x \notin x)$$

E·22 Comment In fact the authors are cautious with the use they make of the $\{\,\cdot \mid \ldots\}$ notation. They introduce at the top of page 9 the notation $\{x \mid x \in E \text{ et } A(x)\}$, for use only when E is a set and $A(x)$ is a relation, to mean the set of those elements of the set E which have the property A;

and on page 8, they use $\{a, b, c, \ldots, l, m\}$ as a notation for (presumably) a finite set, though they do not explain their use of three dots to the reader.

E·23 COMMENT The use of the word "relation", as used at the top of page 8, is abnormal: for example, a total ordering is a set together with a relation that might hold between two members of that set, but here the relation $\in$ is one that holds between a member of the set and the set itself. The same letter $\in$ is used for the relation associated to any set. Why, I wonder ?

Then in the third line of text on page 8, "relation" is used in the sense introduced on page 1, to mean a permitted word that is interpretable as an assertion.

On page 1 it is stated that a term is a permitted word that is an abstract representation of one of the objects about which we wish to reason; I do not see how a word, a finite string of symbols, can be *muni d'une relation:* $\in$; though I can believe that the entity abstractly represented by that word might be.

Discussion of equality

At the top of page 8, $a = b$ is informally introduced: no axioms for equality have hitherto been given, though the sign $=$ occurs in examples on page 5.

The authors say that $a = b$ expresses that a and b are the same object, and that intuitively (page 8 again) a set E is the collection of objects that are members of E.

On page 8: unordered pairs, triplets, etc are stated to exist, though no axiomatic justification is given or claimed for that. Three dots are used, to suggest a finite but arbitrarily long sequence; the word "finite" is not used here.

On page 8: what set theorists know as the axiom of extensionality, that two sets with the same members are equal, is formulated but not stated to be an axiom.

E·24 COMMENT On page 9, Theorem I.4.1 is stated and "proved", that the empty set exists and is unique. To prove its uniqueness some form of extensionality would normally be required. To prove its existence, the existence of some set must be asserted; and so far no axiom says that, so I suppose that the authors are tacitly relying on the fact that it is a theorem of Hilbertian logic with equality that something exists.

A list of axioms of set theory

Now the authors proceed to state various axioms of set theory, but there is nothing to indicate when their listing of axioms has reached its end.

Axiom 1 On page 8, near the bottom, the separation scheme is given.

Axiom 2 On page 9, line −7, the power set axiom is given.

Axiom 3 At the top of page 10, the existence of the ordered pair, *le couple*, (a, b), of two terms a and b, is

stated to be an axiom; presumably the principle stated at the bottom of page 9,

$$((a', b') = (a, b)) \Longleftrightarrow ((a = a')\ \&\ (b = b')),$$

should be included in this axiom.

Axiom 4 Page 10, line 6, cartesian products: the existence of $E \times F$ is an axiom

E·25 Aside Cartesian product is then stated to exist for **any finite number** of terms, though nothing is said about associativity.

E·26 Remark The existence of the intersection of two sets is, correctly, derived from the scheme of separation; the existence of the union of two sets is said to follow from an (unstated) axiom. The principal construction of [M10] shows that the statement, that if x and y are sets then so is $x \cup y$, does not follow from the axioms that the authors actually formulate.

E·27 Remark The axioms are stated to imply the existence of finite sets; whence, the authors say, one can define the integers and develop some number theory.

Axiom 5 At the top of page 12, it is said that the existence of the set $\mathbf{N}$ of whole numbers requires a new

axiom, called the axiom of infinity, which states that "there is a set which is not finite".

E·28 Remark The authors state that once one has $\mathbf{N}$, one can construct all the sets used in usual mathematics.

No definition is given at this point of "finite", nor is *entier* defined; nor is any derivation offered from the stated axiom of infinity that the set of whole numbers exists,

No further axioms are listed: Thus their system is essentially Bou49, though presented with less precision.

The remaining divisions of [L-F,A] Chapter I

E·29 On page 17, the authors begin their discussion of indexed families of sets, but their families are always taken to be always subsets of some given set. Thus given two sets which are subsets of the same set X say, their union, being a subclass of the set X, can be proved to be a set by separation; but, as mentioned in Remark E·26, their system fails to prove that the union of two arbitrary sets is a set. Bou49 is thus weaker than the system Bou54 used in their two cited sources [Bou54] and Godement [Gd], who all admit indexed families of sets which are not necessarily subsets of a set given in advance, thus obtaining a system that includes a form of the axiom of replacement; whereas Bou49 is essentially the system of Zermelo without foundation, without pairing, but with ordered pairs, cartesian products and a global form of choice.

E·30 No definition of *finite* is given before page 32: as with Godement the definition is very late in arrival, but is used earlier. Some of those earlier uses are really of finiteness in the metalanguage; but no such distinction is made by the authors.

N is mentioned on page 8 as an object that will be admitted to be a set. **N** is used on page 20, when sequences are introduced, and again on page 28, to provide an example of an ordered set; it is emphasized that that ordering is a well-ordering. Three pages later, our hopes of a definition of **N** are dashed:

[LFA] p 31	*Nous supposons connues toutes les définitions et propriétés élémentaires relatives aux nombres entiers naturels. Tout au long de l'ouvrage, l'ensemble des nombres entiers sera désigné par* **N**.	*We suppose known all the definitions and elementary properties relating to the natural numbers. Throughout the work, the set of (non-negative) integers will be denoted by* **N**.

E·31 Finally, on page 32, we reach the long-awaited definition: a set is said to be *finite* if it is in bijection with an initial segment of the natural numbers.

E·32 Comment **That is circular**. The axiom of infinity was formulated as "There is a set which is not finite", but without a definition of *finite* having been given. We were told that one can derive the existence of **N** from the existence of a set which is not finite, again without a definition of *finite*. Now we are told that a set is finite if it is in bijection with an initial segment of the natural numbers.

If one tries to interpret those statements in a way that removes the circularity, one arrives at the statement that there is a linearly ordered set which is not in bijection with any proper initial segment of itself. But that is insufficient: the set $\{0, 1, 2, 3, 4\}$ has a linear ordering under which it is not in bijection with any proper initial segment of itself; but no one would say that that set was infinite.

E·33 REMARK Godement avoids this trap: he uses Dedekind's definition of *finite* and then defines **N** as the set of finite cardinals. But our authors, on page 36, speak only of infinite cardinals.

On page 32, it is stated that if A is not finite there is an injection of **N** into A: a covert use of AC.

Discussing the continuum hypothesis, Lelong-Ferrand and Arnaudiès write:

[LFA] p 37 *Depuis 1966 (travaux de l'américain Cohen) on doit considérer que cette proposition est indécidable. Mais l'influence de cette hypothèse sur les mathématiques est restée à peu près nulle.*

Since the work of the American Cohen in 1966, this proposition must be regarded as undecidable. But the influence of this hypothesis on mathematics has remained negligible.

E·34 COMMENT That last remark is an echo of the pronouncements of Dieudonné and of Godement on the foundations of mathematics. It is objectionable because they are using "true" relative to some set of axioms, which they might change at will, but pretend that "true" has some absolute meaning.

E·35 COMMENT 1966 is the date of Cohen's book [Coh2]: the news of his discoveries, which only reached me as a Cambridge undergraduate in 1964, first broke in late 1962; two formal announcements followed [Coh1]. Precise dating will be found in [Ka], which accurately portrays the atmosphere of excitement created by Cohen's break-through.

E·36 REMARK On page 38, there is an unsignalled use of AC in the proof of Corollary 2, about the countability of the union of a countable family.

E·37 REMARK The model for their axioms in which unordered pairing fails, presented in [M10], refutes the contention at the end of [Bou49] that the system presented, Bou49, suffices for all the mathematics "of the present day"—even in 1949 one would have wished to prove that for each c and d, the set $\{c, d\}$ exists—and, since $\{c, d\} = \{c\} \cup \{d\}$, confirms the misgivings of Rosser,[12] who suggested in his review [Ro1] that the existence of $a \cup b$ for two arbitrary sets a, b, would not be provable in Bou49.

E·38 HISTORICAL NOTE Entries in *La Tribu* show that Bourbaki consulted Rosser more than once in the early 1950's: it might be that members of Bourbaki contacted him following the appearance of [Ro1], but there may

[12] J. Barkley Rosser, 1907–1989; Ph. D. Princeton 1934; at Cornell, 1936–1963; at Madison from 1963.

have been earlier contact, as a paper by Rosser on the eliminability of ι-terms is invoked, but without exact citation, in [Bou49]. Bourbaki and Rosser might also be linked through the young Halmos, who was in Chicago from 1946; he had been von Neumann's assistant at Princeton for a spell, and, some years later, was instrumental, with Rosser, in bringing about the famed 1957 Logic Summer Institute at Cornell. Professor Derus writes that he found in a University of Chicago bookstore in 1991 a copy of [Łu] bearing Rosser's stamp on the title page, and the inscription: "To Barkley Rosser, a /professional logician, / from an amateur, / Paul R. Halmos / July 1958."

E·39 COMMENT Jacqueline Lelong-Ferrand and Jean-Marie Arnaudiès are both *archicubes*, that is, former pupils of the École Normale Supérieure in the Rue d'Ulm in Paris.‖ Madame Ferrand entered that school in 1936 and in 1939 was ranked first equal with Roger Apéry in the *agrégation masculine*; Dieudonné was a member of the jury and later wrote that "Only two of the papers impressed me with their sense of analysis and precocious maturity very rare among candidates for the agrégation. Those two were Roger Apéry and Jacqueline Ferrand." M. Arnaudiès entered the École Normale Supérieure in the Rue d'Ulm in 1960. I am told that no logic was taught there in the early sixties, nor was any logic taught at the Rue D'Ulm when Madame Ferrand was there in the 1930's, partly because of the tragically premature death of Jacques Herbrand in a mountaineering accident. The main subjects then being taught were differential geometry, mathematical physics and probability, the protagonists being Elie Cartan, Louis de Broglie and Georges Darmois. Where, then, did our authors imbibe their particular view of logic ? Who taught them ? It would seem that they both originally learned the system of Bourbaki's 1949 address, but that at some point awareness of the shortcomings of that system had filtered through, leading them to hint that certain unstated axioms are needed to supplement those stated.

E·40 HISTORICAL NOTE Indeed it appears that there was no teaching of mathematical logic at the Rue d'Ulm until 1989, since when it has been maintained, at fourth-year level, by a series of mainly three-year contracts: Jacques Stern 1989-1995; Jean-Louis Krivine 1995-1998; Alain Louveau

‖ Originally the École de la Rue d'Ulm admitted only men, and the École de Sèvres only women. Between the two world wars, the Rue d'Ulm admitted women and men, and a female student could try the entrance examination for either. Then there was a period in which the Rue d'Ulm returned to admitting only men. The two Écoles were then merged and from 1986 have formed a single École Normale Supérieure.

1998-2001; Elisabeth Bouscaren 2001-2004; Patrick Dehornoy 2004-2007; François Loeser 2007-2010.

At higher levels, the renaissance began in the 1950s with some courses by Jean Ville and Pierre Samuel, and talks in the seminar of algebra and number theory run by Paul Dubreil. While in Europe for his sabbatical year, 1955/56, from Berkeley, Tarski gave five lectures at the Institut Henri Poincaré at the invitation of J.-L.Destouches, who started a logic seminar there, in connection with the Mathematical Physics that he taught at the Faculté des Sciences while teaching Modern Algebra at the École Centrale; his assistants were Daniel Lacombe, Jean Porte and Roland Fraïssé.⋆ The development of French logic was further helped by the two years, 1960/62 that Georg Kreisel spent in Paris, invited by Henri Cartan to lecture at the Sorbonne. Tarski returned in 1962 to lecture at Clermont-Ferrand. Roger Martin taught logic in the philosophical faculty at the Sorbonne from 1964, and at Paris-V from 1969 till his death ten years later.

⋆ Information from French colleagues, who further write: *Lacombe—le premier logicien à avoir une poste dans une Faculté des Sciences—est le premier spécialiste français de la théorie des fonctions récursives et oriente rapidement des étudiants vers l'emploi des ordinateurs et le langage Lisp pour tester des hypothèses. Porte, formé par Jean Ville et chargé de l'exploitation d'un ordinateur de l'institut Blaise-Pascal, enseigne à la fois la statistique et la théorie des systèmes formels. Fraïssé, formé par l'ancien Bourbachiste René de Possel, enseigne le calcul des formules logiques et la théorie des relations.*

F: ... that has, I suggest, led to the exclusion of logic from the CAPES examination.

In the French educational system, there are collèges for pupils aged 12–15 and lycées for pupils aged 16–18. If you wish to teach in a collège you must have done successfully a third-year university course, called a licence, and obtained the *Certificat d'Aptitude au Professorat de l'Enseignement du Second Degré*, commonly called the CAPES, for which in 2007, there were 5388 candidates in mathematics, of whom 952 passed; a success rate of about 18 %. To teach in a lycée you must have done successfully a fourth-year university course, called a maîtrise and pass another examination called the agrégation. The written part of each of these examinations is on a syllabus specified by a national committee; and these syllabi serve as paradigms for the content of university licence and maîtrise courses, since universities seeking to attract students for these courses naturally wish to provide teaching on topics for the examinations CAPES and agrégation, success in which is the aim of perhaps the bulk of those students.

Thus it comes about that the syllabi for CAPES and agrégation have a profound influence on the whole educational system, and naturally a uniformising influence.

Come with me now to examine the syllabus for the CAPES, which for a given year is announced in April or May of the preceding year in a special number of the *Bulletin Officiel*; in many years details for some subjects are not given explicitly, but merely stated to be the same as in a previous year. A complete statement in the *Bulletin Officiel* of the programme for mathematics was published on 24 May 2001 (for the session of 2002) in Special Number 8, pages 112–124, to which some minor modifications to the section on algebra and geometry were published on 20 May 2004 in Special Number 5, pages 57–58.

F·0 In the *Bulletin* of 2001, the programme is divided into four sections; three of the sections are further divided into chapters, as follows:

1- Notions sur la logique et les ensembles

I. Généralités sur le langage et le raisonnement mathématiques. Éléments de logique.
II. Ensembles, relations, applications.
III. Rudiments de cardinalité.

2- Algèbre et géometrie

I. Nombres et structures
II. Polynômes et fractions rationelles
III. Algèbre linéaire

IV. Espaces euclidiens, espaces hermitiens
V. Géometrie affine et euclidienne

3-Analyse et géometrie differentielle

I. Suites et fonctions
II. Fonctions d'une variable réelle: calcul différentiel et intégral
III. Séries
IV. Équations differentielles
V. Notions sur les fonctions de plusieurs variables réelles
VI. Notions de géometrie différentielle

4-Probabilités et statistiques

Section 4 is not divided into chapters. In all sections there is a further subdivision into paragraphs, which contain lists of topics. In the fourth section, one topic is "Parallèle entre le vocabulaire probabiliste et le vocabulaire ensembliste à propos des opérations sur les événements."

In sections 2, 3 and 4, the title of the section is immediately followed by the title of the first subdivision; but at that point in section 1, on page 112, there is inserted the minatory sentence:

Tout exposé de logique formelle est exclu.

That sentence is also to be found in the *Bulletin Officiel* Special Number 3, of 29 April 1999, (the earliest year accessible to me), on page 97; and was left unaltered for ten years by subsequent *Bulletins Officiels* .[37,38]

F·1 Thus there was, officially, a ban on formal logic in each of the sessions 2000-2009, and the ban, though now muted, continues. The objection seems to have been to the actual process of formalisation, for the topics listed in detail in paragraph I of section 1 form an entirely reasonable and coherent group, though their ordering might be challenged, as discussion of the distinction between free and bound variables is placed before discussion of the propositional calculus; and of course the subsection on probability will perforce contain much set theory and Boolean logic. Is it too far-fetched

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[37] Special Numbers 4, 18 May 2000, page 72; 13, 30 May 2002, page 41; 3, 22 May 2003, page 87; 5, 19 May 2005, page 123; 3, 27 April 2006, page 138; 3, le 17 May 2007, page 122; and 4, 29 May 2008, page 123.

[38] In the annual reports of the Jury of the CAPES from 2003 onwards the minatory sentence is, interestingly, replaced by the more delicate disclaimer that *Aucun exposé de logique formelle n'est envisagé*; which replacement has now been made in the very text of the CAPES mathematical syllabus itself, as witness its recent complete statement in Special Number 6, 25 June 2009, of the *Bulletin Officiel*. But, that possible softening and typographical improvements aside, the 2009 syllabus hardly differs from that of 2001.

to suggest that the origins of this nervousness about formalisation, and by extension, about logic, is the Bourbachiste confusion over quantification ?

F·2 I hear (though may have difficulty in verifying, given the secrecy of much French decision-making) that the ban on logic was imposed by a CAPES committee comprised largely of disciples of Bourbaki.[◇] I am not privy to their secrets, and of the educational background of *Inspecteurs Généraux de Mathématiques* know only that some were themselves *archicubes*, and therefore can only guess that the committee's action is rooted in the distrust of logic evinced by Godement, Dieudonné and their colleagues, as documented in Sections C, D and E, and not dispelled by other potential educational influences.

We have seen that in the nineteen-tens and twenties, there was a widespread effort to develop predicate logic. Hilbert made a proposal which was developed by his disciples and cast in concrete by Bourbaki; so that Bourbaki's foundational ideas are rooted in logic as understood by followers of Hilbert in the nineteen-twenties. Hilbert's proposal is not the only possible treatment of predicate logic, but must be one of the clumsiest as measured by the lengths of formulæ generated. We have seen in Section B various idiosyncrasies of the τ operator and its unsuitability in a formalism for set theory, and the inadequacy for functorial ideas of the concomitant treatment of classes.

Thus it would seem that the spiritual ancestor of all the oddities that I dislike in Bourbaki's treatment of logic and set theory is that 1922 paper of Hilbert. The story seems to be that a great man became interested in an alternative but non-optimal approach to a problem; then, rather like newly-hatched ducklings, the early Bourbachistes followed him in adopting that approach; and when, later, they came to positions of influence and power, caused others to do likewise, even though the problem itself had by then been shown to be insoluble.

Bourbaki, in short, by uncritically taking the Hilbert–Bernays–Ackermann operator as their cornerstone, created a nightmare. By working with a formalism that is the product of the enthusiasms of a pre-Gödelian age, they arrived at a negative view of logic and thus created in the minds of their readers a barrier against understanding the aims and enthusiasms of post-Gödelian foundational studies. But what they ought to have been hostile to is not logic but their own (or rather Hilbert's 1922) twisted version of it.

[◇] Is it coïncidence that the other area of mathematics subject to considerable reservations in the CAPES syllabus is probability and statistics, whereas one learns from the embarrassed reminiscence of Laurent Schwartz quoted by Maurice Mashaal [PlS, p.76] that probabilists were subjected to numerous public insults from followers of Bourbaki ?

F·3 For despite their fixation on the Hilbert–Bernays–Ackermann operator, Bourbaki did at least notice that logic based on it is seriously flawed, though they might not consciously have identified the nature and cause of the flaw. Rather, they appear to have reasoned that "Hilbert was a great man; his treatment of logic is messy; therefore logic is a mess." Section C shows how a member of Bourbaki betrays in his remarks about logic and set theory numerous apprehensions and misapprehensions, and Sections D and E identify further coarsenings of the situation.

One would expect the degeneration that I have described to have been the subject of comment by concerned mathematicians: but not a peep; it appears that people are frightened to speak out. The Bourbachiste œuvre constitutes a remarkable achievement, and the members of Bourbaki are individually so distinguished, each in his own sphere of competence, that one has to be extremely careful in criticising them; nevertheless I contend that they have infected mathematicians across many generations with their stunted conception and phobia of logic, and that this regrettable result is the consequence of their inadequacy as logicians coupled to their eminence as mathematicians.

F·4 So the CAPES committee may well, in the short term, have made a realistic decision: distinguished and widely-read textbooks of algebra such as the two we have examined present accounts of logic that are repellent; so it is hardly to be expected that in teacher-training colleges, known in France as IUFMs, logic will be well-taught; so one can see why it might have been thought desirable to bar it from the examination.

F·5 But that policy in the long term will, I submit, be intellectually crippling. These syllabi govern to some extent the subjects that can be taught at University level in France; as most of the 3rd and 4th year students are aiming to teach. **I imagine** that there will be universities in France where no logic is taught, it not being thought necessary, not being in the CAPES, just as senior figures in British universities have been heard to say that "we don't need logicians";[♡] therefore the next generation of schoolteachers will be giving their pupils a view of mathematics without formal logic in

[♡] To give this matter some international perspective: in 1984/5, when Cambridge was considering increasing the amount of teaching offered there of logic, including set theory, model theory and recursion theory, statistics from thirteen American universites—Berkely, Boulder, CalTech, Chicago, Cornell, Harvard, Madison, M.I.T., Penn State, Princeton, Stanford, UCLA, and Yale—were produced concerning the number of logic lectures offered to undergraduates and to graduates at these places. Taking as a unit the Cambridge standard lecture length of 50 minutes, it was found that for undergraduate teaching the mean and variance among those thirteen were 93.23 and 32.06 units respectively; at the time it. Indeed that particular piece of "dumbing down" has already happened : mid-career French mathematicians tell me that as fourteen-year-olds they were fascinated to be introduced to truth tables and formal reasoning, but that today introductory logic is no longer taught in French schools.

That in turn will lead to mathematics itself not being taught in many schools, as, I am told, is already the case in the educational systems of certain countries. So it would be much better to replace all that warped account with a sensible and correct account of logic and set theory.

F·6 REMARK What we have seen is an example of the "trickle-down" process in learning. A bad decision at research level leads in turn to bad teaching at university level, to bad preparation of school teachers, and to bad teaching at school level; thus the scorn for logic displayed by Dieudonné to Quine in a Parisian seminar became, some twenty years later, an entrenched global policy of the French educational system; with the result that French schoolchildren today are described as being *angoissés* by mathematics.

F·7 REMARK Beyond the CAPES is a higher examination called the Agrégation, success in which guarantees a teaching appointment in a *lycée*. Until recently, the syllabus for the Agrégation had not a word about logic, and was arranged under these headings:

I-Algèbre linéaire.
II-Groupes et géometrie.
III-Anneaux, corps, polynômes et fractions rationelles
IV-Formes bilinéaires et quadratiques sur un espace vectoriel
V-Géometrie affine, projective et euclidienne
VI-Analyse à une variable réelle
VII-Analyse à une variable complexe
VIII-Calcul différentiel
IX-Calcul intégral et probabilités
X-Analyse fonctionelle
XI-Géometrie différentielle

But fortunately the need of computer science departments for logic courses of a particular kind has led to a revision of this syllabus. So the rigid stance portrayed above is, encouragingly, beginning to be modified; but how did it come about in the first place ?

the offering in Cambridge was 16 units, and, it seems, in Paris 0. For graduates, the American mean and variance were 179.31 and 67.19 units; in Cambridge the amount varied annually, but averaged perhaps 36. What was available in Paris ?

G: *Centralist rigidity sustains the confusion and consequently flawed teaching; ...*

WHEN AN EDUCATIONAL SYSTEM is prescriptive, what is to be taught will be laid down at the centre, thus negating the personal and individual nature of teaching. The undesirability of that is excellently expressed in the following passage from Feyerabend, *Against Method* [Fe]:

p45: "Any method that encourages uniformity is in the last resort a method of deception. It enforces an unenlightened conformism and speaks of truth; it leads to a deterioration of intellectual capabilities, and speaks of deep insight; it destroys the most precious gift of the young — their tremendous power of imagination — and speaks of education. Variety of opinion is necessary for objective knowledge."

The legacy of Napoleon: the foundation of the modern French university system

G·0 French *dirigisme*—the taste for strong orders from the centre—goes back at least to Richelieu; in 1789 as the monarchy tottered a prescient nobleman remarked that "If the king will not have an army, the army will have a king," and within ten years a young general from Corsica had risen to supreme political power.

G·1 Since coming to work in France I have found that in order to understand the way French academics behave, one should imagine that one is in the army; and it has seemed to me that there are no universities in France, only units in a university system. The reason emerged in 2008: the University from which is descended the contemporary French educational system was founded by decree two centuries ago, when the Emperor Napoléon I caused the Corps Législatif to pass the decree/law of May 10 1806, visible at

`http://www.inrp.fr/she/universite_imperiale_bicentenaire_loi.htm`

which enacted that:

Art.1er. Il sera formé, sous le nom d'Université impériale, un corps chargé exclusivement de l'enseignement et de l'éducation publique dans tout l'Empire.

Art 3. L'organisation du corps enseignant sera présentée, en forme de loi, au Corps législatif, à la session de 1810.

But the Emperor could not wait so long and hurried things forward by a long and detailed decree of March 17, 1808, visible at

`http://www.inrp.fr/she/universite_imperiale_bicentenaire_decret.htm` which enacted that:

Art 1er. L'enseignement public, dans tout l'empire, est confié exclusivement à l'Université.

Art 2. Aucune école, aucun établissement quelconque d'instruction ne peut être formé hors de l'Université impériale, et sans l'autorisation de son chef.

Art 3. Nul ne peut ouvrir d'école, ni enseigner publiquement, sans être membre de l'Université impériale, et gradué d'une de ses facultés. Néanmoins, l'instruction dans les séminaires dépend des archevêques et des évêques, chacun dans son diocèse.

Art 4. L'université impériale sera composée d'autant d'académies qu'il y a de cours d'appel.

This document makes fascinating reading: of the 144 articles, I draw attention to

Article 5, which shows the comprehensive character of the concept: all educational establishments, down to "Dames' Schools", are to come under the single umbrella;

Article 29, which fixes the rank of the various fonctionnaires: the university professors are at level 10; above them are the Grand Master, the Chancellor, the Treasurer, and assorted councillors, inspectors, rectors and deans;

Article 38, which states that the base of the teaching at all levels is to be fourfold: the precepts of the Catholic religion; fidelity to the emperor and his dynasty; obedience to the statutes, of which the aim is to create citizens that are attached to their religion, their prince, their country and their family; conformity to the dispositions of the Edict of 1682 concerning the four propositions contained in the declaration of the clergy of that year;□

Article 101, which proposes something like an Oxbridge college in stating that teachers of junior rank will be constrained to be celibate and to live in community; more senior professors may be married, but if they are single they are encouraged to "live in" and benefit from communal life;

Article 102, which provided that no woman is to be lodged or received in any lycée or collège; and

Articles 33, 128, 129, 130, which concern the robes to be worn by members of the various Faculties.

A professor is required, by clause 8 of Article 31, to be a doctor of his Faculty. To become a doctor in, for example, the Faculty of mathematical

□ the purpose of which was to limit the authority of the Pope in France and in effect to create a Church of France somewhat similar to the Church of England.

sciences and physics, one must, by Article 24, submit two theses, in those subjects which one intends to teach. Mention of such theses aside, the Imperial decree, though frequently speaking of *le corps enseignant*, the teaching body, says not a word about research; but what might Napoleon, a soldier of genius but not an academic, be expected to know about that ?

Politics and mathematics

G·2 Despite the ceaseless tug-of-war between the pro- and anti-clerical parties over the educational system in France in the 19th century and the various perturbations in the 20th, the centralist conception of Napoleon remains. In our present discussion we see French *dirigisme* at work in the CAPES. I wonder how similar is this imposition of a regressive policy by an uncomprehending bureaucracy to the situation of logic in Eastern Europe under Stalin, described in my essay *Logic and Terror* [M2], when, for example, in Poland the great Andrzej Mostowski dared not call himself a logician till the late nineteen-sixties.

G·3 When in 1871 the Third Republic began and Jules Ferry took control of the educational system, his concern was to ensure that only republican ideas would be taught. Try as I might, I can see no relationship between republican values and mathematics, but the French can; and, a hundred and forty years later, a section on "republican values" was included in the syllabus recently proposed in Réunion for the second year Master course in mathematics, which syllabus also mentions developments "since 1789". Is this an echo of the course on *arithmétique républicaine* taught in Rouen in 1794 by Caius-Gracchus Prud'homme ?

G·4 A reader of *The Ignorance of Bourbaki*, the holder of a (C4) chair of pure mathematics at a leading German University, told me that as a young man he had been reduced to a state of intellectual paralysis by reading Bourbaki and that he had had to retire from mathematics for six months before making a fresh start. Fortunately, it is not necessary to worship at the Bourbachiste shrine in order to do serious mathematics.

G·5 That it might ever have been thought so necessary can be divined from fleeting remarks about intellectual terrorism by Miles Reid in his book [Re2]. I quote from the remarks on pages 114–117 of the 1994 reprint.

> *"Rigorous foundations for algebraic geometry were laid in the 1920s and 1930s by van der Waerden, Zariski and Weil. (van der Waerden's contribution is often suppressed, apparently because a number of mathematicians of the immediate post-war period, including some of the leading algebraic geometers, considered him a Nazi collaborator.)"*

> *"By around 1950, Weil's system of foundations was accepted as the norm, to the extent that traditional geometers (such as Hodge and Pedoe) felt compelled to base their books on it, much to the detriment, I believe, of their readability."*

> *"From around 1955 to 1970, algebraic geometry was dominated by Paris mathematicians, first Serre then more especially Grothendieck."*

> *"On the other hand, the Grothendieck personality cult had serious side effects: many people who had devoted a large part of their lives to mastering Weil foundations suffered rejection and humiliation. ... The study of category theory for its own sake (surely one of the most sterile of all intellectual pursuits) also dates from this time."*

> *"I understand that some of the mathematicians now involved in administering French research money are individuals who suffered during this period of intellectual terrorism, and that applications for CNRS research projects are in consequence regularly dressed up to minimise their connection with algebraic geometry."*

G·6 Let us set against Reid's remarks a comment [Bor] of Armand Borel:

> *"Of course there were some grumblings against Bourbaki's influence. We had witnessed progress in, and a unification of, a big chunk of mathematics, chiefly through rather sophisticated (at the time) essentially algebraic methods. The most successful lecturers in Paris were Cartan and Serre, who had a considerable following. The mathematical climate was not favourable to mathematicians with a different temperament, a different approach. This was indeed unfortunate, but could hardly be held against Bourbaki members, who did not force anyone to carry on research in their way."*

G·7 COMMENT I wonder if there is an element of complacency in that last statement of Borel. Suppose it were the case that over a certain period in numerous universities, in France, in Spain, in England, or elsewhere, the Bourbachistes seized power and pursued a policy of denying jobs to non-Bourbachistes. How would one obtain evidence of that ? The poor non-Bourbachistes, being excluded from employment which would permit them to research would be likely to move away from universities and find jobs in industry or elsewhere, and indeed to lose touch with research mathematics. So they would be excluded from any figures that might be produced. People would be saying that the Bourbachiste view is the standard one; what would not be said is the subtext, that that state of affairs has come about because the opposition has been suppressed. There would thus be a political component to what has been called mathematical practice.

An explicit example: François Apéry writes in [Ap1,2] of his father Roger Apéry, who at the age of 61 proved the irrationality of $\zeta(3)$, that he declined Dieudonné's invitation to join Bourbaki, and that the dominance of Bourbaki meant marginalisation for an anti-Bourbakiste; despite, one might add, Apéry *père* having at the age of 23 impressed Dieudonné by his performance in the *agrégation masculine*.[39]

G·8 That prudent would-be critics of Bourbaki should conceal their identity is suggested by the circumstance that in the special issue [PlS] of *Pour la Science*, published in 2000, dedicated to a study of Bourbaki, on page 78, where I am named and briefly quoted, and described, to my delight, as having *pourfendu l'ignorance bourbachique*, a *spécialiste parisien* is quoted at greater length but **on condition of anonymity.**

G·9 To that Parisian critic's remarks, with which I am in complete agreement, I would add that it is not only in France that Bourbaki is regarded as an unchallengeable authority on logic. Their stifling influence is to be found elsewhere. Let me give a comparatively mild example. A mathematical logician has confided in me that he obtained tenure at his University, in a European country other than France, by pretending that despite retaining an eccentric interest in logic, in reality he subscribed to his Department's view that "real men don't do logic". He believes, and I with him, that had he revealed the depth of his commitment to logic he would not have been given tenure, for the reason that the quasi-totality of his decision-making colleagues were imbued with Bourbaki's negative attitude. I could wish that now that he has landed safely in the Realm of the Blessed, he would speak up for logic, but it appears that the habit of caution is too deeply ingrained. Still, it is not for me to "out" him.

G·10 Professor Segal in his *Zentralblatt* review [Seg1] of my essay [M3] writes that I am unhappy with the neglect of logic by mathematicians. No, it is not the neglect — surely all are free to be as ignorant as they choose — to which I object but the imposition, by the high-placed ignorant, of their ignorance on their subordinates, their interference with the teaching of logic to those who wish to learn it, and their denial, through the mechanism mendaciously called "peer review"[♡], of research funds for work in this area.

G·11 COMMENT In 1851, at the Albany meeting of the American Association for the Advancement of Science, Alexander Dallas Bache, in his address as out-going president, spoke of *that modified charlatanism which*

[39] See [Ag] for further references and discussion, and [Ch] for a public protest by Chevalley and others against the neo-Bourbachiste cosily repressive attitude lampooned by Molière: *Nul n'aura de l'esprit hors nous et nos amis.*

[♡] "clique review" would be more accurate.

makes merit in one subject an excuse for asking authority in others, or in all. In 1974, in his Nobel Memorial Prize address *The pretence of knowledge*,[40] Friedrich August von Hayek describes the "scientistic" attitude as *decidedly unscientific in the true sense of the word, since it involves a mechanical and uncritical application of habits of thought to fields different from those in which they have been formed.*

Somewhere between those two is what, in a nutshell, I fear has happened with Bourbaki and logic.[41]

[40] in English in [vHay1], in German translation in [vHay2].

[41] The point is reïnforced by the intriguing lecture [Du] given by Till Düppe at Siena in October 2007, entitled *Gerard Debreu from Nicolas Bourbaki to Adam Smith*, exploring the harmful consequences for economics of Bourbaki's influence on the psychological relationship of mathematical economists to their subject.

H: The recovery will start when mathematicians adopt a post-Gödelian treatment of logic.

THE MANY DEVELOPMENTS IN LOGIC and foundational studies since the 1920's—it would be invidious to name names, but let me mention the incompleteness theorems, the relative consistency proofs for the Axiom of Choice, the evolution of category theory, in set theory the discovery and development of forcing, the emergence of large cardinal properties, the fine structure and core model programmes, and the work on infinitary games, to say nothing of the many advances in model theory, proof theory and recursion theory—have created a foundational arena so different from that envisaged in the 1920's as to demand a new and positive approach to the teaching of logic in schools and universities.

Concerning that teaching, I would say that *of course* there are things in logic which are not yet understood, as in any living subject that tackles difficult problems, but there are ways of presenting logic and set theory which are far better than the Bourbachiste method. My message to the adventurous youth of today must be this: if you want to know what has happened in logic in the past century, do not go to Bourbaki, for they cannot tell you.

H·0 There is a widely held view, vehemently urged by Dieudonné and spread by the less enlightened of Bourbaki's disciples, that mathematicians need trouble themselves no longer about foundational questions. But there are many examples of classical conjectures being proved both consistent and independent: one might mention Souslin's hypothesis about a possible characterization of the real line as an ordered set, and Whitehead's conjecture concerning free Abelian groups; one might also mention the use of set theory in elucidating the structure of weakly distributive Boolean algebras and in the study of the Lebesgue measurability of an arbitrary set of reals. And if one looks only for positive contributions of ideas from logic to other branches of mathematics, one finds that they too are legion. Immediately to mind, again without naming names, come the use of model theory in the proof of the near-truth of Artin's conjecture and in the proof of the Lang–Mordell conjecture for fields of arbitrary characteristic; the use of Ramsey theory in the study of the subspace structure of Banach spaces and in the positive solution of Kurosh' problem concerning the existence of uncountable groups with only countable subgroups; the use of priority arguments from recursion theory in the construction of topological manifolds; and the use of proof theory in the study of sums of squares.

So I think mathematicians would be unwise to tell themselves that they will never encounter foundational problems nor have a use for foundational ideas.

Bourbaki and French nationalism

H·1 In my essay *The Ignorance of Bourbaki* I wondered whether the attitudes of Bourbaki might stem from the influence of Hilbert or from some nationalist or chauvinist feeling, and Professor Segal, in his review [Seg1], suggested that I was thereby contradicting myself.

Perhaps I should state that I see a distinction between nationalism and chauvinism. Consider, for example, Janiszewski, who at the end of the First World War called for a small poor country to make its mark in foundational studies: I see him as a Polish nationalist but not a chauvinist. It is one thing to say "Good things are going on elsewhere in the world: let us try to do as well or better." It is another to say "Everything that is worth knowing is known by us; let us ignore the activities of others".

H·2 Now Cartier's interview [Sen] makes it clear that Hilbert and German philosophy were held up as models by Weil and others. He says

> *"The general philosophy is as developed by Kant. Bourbaki is the brainchild of German philosophy. Bourbaki was founded to develop and propagate German philosophical views in science. All these people ... were proponents of German philosophy."*

H·3 So I really do not see that there is a contradiction between wishing to strengthen French mathematics and saying that the Germans do it better. One might say that the Bourbachistes were nationalist but not chauvinist. They considered, indeed, that the French policy of putting scientists in trenches in World War I, when the Germans, wisely, protected their scientists, had retarded French mathematics by one full generation. Further evidence comes from *Claude Chevalley described by his daughter*, [Cho, pages 36–39], where she says that the Bourbaki movement was started essentially because rigour was lacking among French mathematicians by comparison with the Germans, that is, the Hilbertians.

The chimera of completeness

H·4 Thus we come back to Hilbert. We began this essay with a translated excerpt of a letter from him to Frege. I suggest that Hilbert never shook off the illusion that a complete recursive axiomatisation of the whole of mathematics awaited discovery. It underlies his championing of the epsilon operator, the use of which seems to rest on a belief in the completeness of the system under discussion.

It reappeared in his quarrel [vDa1] with Brouwer: from the sources quoted in [Ke] it is evident that Brouwer had accepted some perhaps intuitive notion of the incompleteness of mathematics, and indeed Brouwer's lectures in Vienna may have influenced the young Gödel to probe further. Such a perception was anathema to Hilbert, whose battle-cry was *Wir müssen wissen, wir werden wissen.*

H·5 Bourbaki also were bedevilled by this mistake. Though Bourbaki in their *Note Historique* give a correct sketch of a proof of the incompleteness theorems, their residual identification of truth with provability produces problems. In a complete system, truth and provability are indeed identical; but they are not for recursively axiomatisable consistent systems extending arithmetic. Bourbaki, indeed, are incoherent in their use of "true". They have the idea that it is relative to a certain system; but they also at times wish to say that "true" means "known to be provable at the time of writing".

H·6 A long-held belief, by the time it is shown to be false, may be too deeply embedded to be given up. We saw Hilbert, in his preface to [HiB1], doggedly maintaining that his programme would survive Gödel's unwelcome discovery of incompleteness. Corry, in [Cor3] and [Cor5], shows *inter alia* that Hilbert discarded the view that mathematics is a formal game with marks on paper. Hilbert himself developed his ideas about logic over twenty years or more, as the subject itself developed through the work of many people, leaving us with the problem of explaining Bourbaki's strangely rigid, and indeed oppressive, attitude to logic.

H·7 Part of that oppressiveness may stem from the fact that Bourbaki was not a person but a group of people, so that the mind of Bourbaki is not an entity of the same kind as the mind of Hilbert, and would be subject to discontinuities in its development stemming from tensions between individual members of the group. The archives of Bourbaki are, at least in part, available on-line,[42] at

`http://mathdoc.emath.fr/archives-bourbaki/feuilleter.php`

They make poignant reading. The numerous drafts, by different hands, of the various sections of the book on set theory make manifest the extent of the effort that went into the preparation of the finished work.[43] Among those drafts, my eye is caught by two typescript notes by Chevalley entitled *Ensembles bien ordonnés* and *Le formalisme de Gödel*. These two show that Chevalley was more in tune with mainstream set theory than are

[42] I am greatly indebted to Professor William Messing for this information.

[43] The names pencilled on copies of drafts prior to their distribution are of recipients rather than authors.

Bourbaki's books, and that he had gone so far as to read[44] at least part of Gödel's monograph on the axiom of choice and the generalised continuum hypothesis.* Had Chevalley's voice been louder than Dieudonné's in the shouting matches that, Armand Borel tells us, were a regular feature of Bourbaki meetings, perhaps more of the insights of Gödel would have got through the Bourbaki process, and the volume on logic and set theory would have been far more satisfactory, with fewer of its readers coming to feel that they had, in some sense, been cheated. When Bourbaki decided against the approach to set theory that Chevalley, following his reading of Gödel, would, apparently, have favoured, the result was to create a breach between mathematics as conceived by Bourbaki and set theory as developed by Gödel, and, following Cohen's breakthrough, by Solovay, Jensen and their successors. If only ...

La Tribu

H·8 But even more revealing than the drafts are the issues of *La Tribu*, the Bourbaki in-house journal that contained minutes of the various meetings, including commitments for the future, censures given to various members who had come to meetings ill-prepared, and, here and there, some excellent parodies and jokes. Hitherto in this essay we have treated, as we must, the works of Bourbaki as they were actually published, not as they might have been; but the copies of *La Tribu* enable us to penetrate beneath the surface and find out something of the personal interactions that led to the final choice of treatment.

One cannot know how faithfully the minutes of *La Tribu* recorded the discussions of the meetings; nevertheless it is noteworthy how many points made in earlier sections of this essay against the published logic texts of Bourbaki and their followers are reported to have been the subject of debate at their meetings. We cannot here examine *all* pertinent passages, illuminating though it be to observe the developing perception among members of the group of a need for a book on logic and set theory; to note the adoption in 1950 of the Hilbert operator; and to note the 1951 decision, apparently Dixmier's, to replace the Lesniewski–Tarski system of propositional logic by that of Hilbert–Ackermann, *"arrangé à la sauce Chevalley"*; we shall

[44] as indeed did the young Cartier, who thereby was enabled to have a serious and lengthy conversation with Gödel himself, in German, at Princeton in 1957.

* There are, though, inaccuracies in Chevalley's summary of Gödel's consistency proof: he fails to distinguish between the class of all subsets of a constructible set and the class of all its constructible subsets, and thus appears in places to think that every subset of a constructible set will itself necessarily be constructible.

focus particularly on two further people, Eilenberg and Rosser, their views of logic and their relations with Weil.[45]

H·9 The reader of *La Tribu* will note the interest, expressed repeatedly, in the foundational proposals of Eilenberg, who is usually called Sammy. Eilenberg (1913–1998) had taken a logic course from Tarski in Warsaw in the early 1930's; at his father's urging, he left Poland for the U. S. in 1939, where he was received at Princeton and then appointed to Michigan.

He was recruited into the Bourbaki group at the instigation and urging of Weil as a result of Weil's esteem for his work as a topologist.[46]

From later conversations with Eilenberg, Cartier had the strong impression that although Eilenberg indeed knew more about and took more interest in logic than other Bourbaki members, he was never very interested in its role in foundations or even as mathematical hygiene but viewed it more as one further subject matter to which current abstract structural mathematics could bring deeper methods and concepts.[47]

Eilenberg in conversation with Michael Wright in 1990 said that he thought of foundations as something growing and evolving along with the main body of mathematics, concerned mainly with tracing the relationships within that and as *"something coming into focus as we move from the inside outwards as mathematics grows and we come to see how the various directions of advance are connected"*.

TRIBU 15: Compte-rendu du congrès de Nancy (9 au 13 avril 1948)

`[nbt_017.pdf]`[48]

PRÉSENTS: Chabauty Delsarte Dieudonné Godement Roger Samuel Schwartz Weil

Mornings at this meeting were devoted to Livre I. Its discussion opens with a revelatory pleasantry:

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[45] My discussion of the parts played by Chevalley, Rosser, Eilenberg and Dixmier owes much to the illuminating reminiscences of Pierre Cartier in conversation with Michael Wright in Paris on January 9th 2012.

[46] Cartier gives another example of Weil's influence: he says that the collaboration, which in 1949–50 was already under way, of Eilenberg and Cartan in what became their famous text, was very much imposed—at the beginning—by Weil in the face of initial reluctance from Cartan. Cartan at that point would have preferred to write his own text single handed, although in the course of the collaboration he developed a great appreciation for all that Eilenberg brought to the work.

[47] This attitude clearly marked Eilenberg's student Lawvere.

[48] The numbering of the on-line .pdf files is slightly out compared with the numbering of the issues of *La Tribu*.

p 5 *Malgré le soin constant de chacun de ne pas faire de philosophie ("Philosophy is the systematic misuse of a language especially created for this purpose"), le Congrès fut souvent menacé d'enlisement. Dieudonné rappela à l'ordre les récalcitrants, et "ontologiste" fut l'injure suprême. On nota une éclatante conversion de Schwartz à la Dialectique.*

There follows a lengthy discussion of the difficulties to be encountered at the start of an exposition of logic. Weil is instructed to re-write the Introduction taking account of the discussion.

TRIBU 18: Congrès oecuménique du cocotier (Royaumont, 13 au 25 avril 1949) `[nbt_020.pdf]`

PRÉSENTS: Cartan Chevalley Delsarte Dieudonné Godement Pisot Roger Samuel Schwartz Weil, et le COBAYE Serre (en cour de métamorphose).
ABSENTS: Chabauty, Ehresmann

p 1 *Soucieux de l'avenir, Bourbaki décida d'envisager des situations de repli pour ses membres; la liste suivante a été adopté:* and then on page 2, Eilenberg is in the list, proposed as a *concierge* in a *collège de filles*, although his first attendance at a meeting seems to have been at Royaumont in October 1950.

p 5 Eilenberg engages to make a report on *ses vieux trucs de multicohérence et d'applications dans les cercles.*

p 6 *Dès la première séance de discussion, Chevalley soulève des objections relatives à la notion de texte formalisé; celles-ci ménacent d'empêcher toute publication. Après une nuit de remords,*** *Chevalley revient à des opinions plus conciliantes, et on lui accorde qu'il y a là une sérieuse difficulté qu'on le charge de masquer le moins hypocritiquement possible dans l'introduction générale. ...*

p 7 the system of Gödel is mentioned.

TRIBU 19: Congrès de la Réforme (Paris 2 au 8 octobre 1949) `[nbt_021.pdf]`

PRÉSENTS: Cartan Dieudonné Ehresmann Godement Roger Samuel Serre Schwartz Weil.
COBAYES: Blanchard Malgrange

p 1 Weil proposes that once the contents of a chapter have stabilised, its details should be discussed in committee rather than in plenary session. *La foule applaudit ce projet.* Cartan and Dieudonné ask to be on all the committees. On page 2 the committee for Chapters I and II is constituted as Dieudonné, Cartan and Weil, and for Chapter III as Dieudonné, Cartan and Samuel.

** Was that sleepless night the cause of Chevalley's attending no further meetings till July 1952 ?

TRIBU 20: [Congrès des Comités de Décembre, Paris] 15 Décembre 1949
Compte-rendu des comités de decembre (Paris, 3-5 Déc 1949) [nbt_022.pdf]

p 1 Weil raises objections to the axiom of families of sets (essentially the axiom of replacement); his chief objection is that Bourbaki would have no use for it;♣ but the congress repeats *à l'unanimité* its previous decision to reject any axiom system that forbids unrestricted cardinal arithmetic.

TRIBU 22: Congrès de la revanche du Cocotier (5 au 17 avril 1950) Royaumont [nbt_024.pdf]
PRÉSENTS: Cartan Chabautey Delsarte Dieudonné Godement Mackey (au début) Pisot Roger Samuel Schwartz Serre Weil.

p 2 *Pour satisfaire les désirs inavoués de Chevalley, on basera la logique sur le symbole "yoga" de Hilbert.* Is this the first time that the Hilbert ε symbol was discussed ? Rosser's review [Ro1] was in the issue of the *Journal of Symbolic Logic* dated January 1950. But it is conceivable that he sent it earlier to Bourbaki and opened a dialogue, and perhaps suggested that they use the epsilon symbol. *Et, on eut beau "chasser le Dénombrable", "il revint en trottant".*
p 4 Drop the idea of publishing chapter III before Chaps I and II.
p 5 Chevalley engages to do chapter II, in particular the section on structures.
p 7 Review of state of Livre I: delete the confessions of Chevalley, and add a justification of the ε of Hilbert.

TRIBU 23:[49] Congrès de l'horizon (Royaumont, 8-15 octobre 1950) [awt_002.pdf]
PRÉSENTS: Cartan Dieudonné Dixmier Eilenberg Samuel Serre.
RETARDATAIRES: Godement Schwartz Koszul

p 1 *La présence d'Eilenberg fut le fait marquant du Congrès. .... il sera appelé "Sammy".*

La lecture de la logique souleva une indifférence croissante, qui; après Godement, Schwartz et Serre, commence à gagner Cartan et Dieudonné; il fallut l'expulsion de la relation "x est un ensemble" pour faire quelque peu crier Samuel. Seuls Dixmier et Sammy montrèrent un vif intérêt pour ces questions...

p 2 *Engagements du Congrès:*

DIXMIER:	*redige l'état 6 de la Logique et des premiers § des Ensembles.*
SAMMY:	*explique à Dixmier son système pour les parenthèses.*

♣ This slightly suggests that Rosser's critique [Ro1] of [Bou49] had not yet reached Bourbaki.

49 absent from the Delsarte collection, but in the Weil collection

Pages 11-14 contain minutes of an extensive discussion of a version of Chapter I, État 5 that does not always agree in its numbering of sections with that available on-line.

p11 *On adopte temporairement le symbole ε de Hilbert. ...*

p14 *Pour le chap.II on rejette la proposition Chevalley de remonter les ordinaux avant les structures.*

Cartan voudrait le couple comme relation primitive; d'autres préfèrent l'astuce Gödel (qui donne aussi l'ensemble à deux éléments). ... Montrer que $\{x, y\} = \{y, x\}$.

H·10 On 19 January 1951, Weil wrote to Cartan[50]:
"*Ci-joint une lettre de Barkley Rosser, commentant mes suggestions sur la logique de Bourbaki. Je te demanderai de la transmettre à Nancy, pour la faire tirer, après en avoir pris connaissance. Chevalley m'écrit qu'il se rallie "avec enthousiasme" (sic !!!) à ma proposition d'employer ε pour définir les entiers et les cardinaux. ....*

Je t'envoie deux exemplaires de la lettre de Rosser, un pour Nancy et un pour que tu le transmettes dès maintenant à Dixmier puisque (sauf erreur) c'est celui-ci qui est chargé de la logique."

TRIBU 24:[51] Congrès de Nancy 27 janvier au 3 février 1951 `[nbt_025.pdf]`
PRÉSENTS: Cartan Delsarte Dieudonné Dixmier Godement Koszul Sammy Samuel Serre Schwartz.
COBAYES: Glaesser, Grothendieck, un brésilien

Dixmier engages to finish Draft 6 of logic, for 1 May 1951. On page 3: Dixmier sees contradictions in Weil's suggestions about ε; it is noted that **ceci cadre mal avec les assurances de Rosser.**[♠]

H·11 Who, one wonders, first suggested using the epsilon operator ? Dixmier voiced some reservations; and Cartier recalls Eilenberg saying in the 1960s that though he could see the defects of the Hilbert operator in risking ambiguity of type, that issue did not impinge on the constructions of algebraic topology, so did not justify a fuss. Perhaps Eilenberg felt that since the other Bourbachistes were little interested in logic, and since his relations with Weil were excellent, and helpful to him in his work with Cartan in

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[50] [CW], page 327: tantalisingly, the letter from Rosser sent in duplicate by Weil is nowhere to be found, despite the best efforts of, in Strasbourg, the editor, Michèle Audin, of [CW]; in Paris, Florence Greffe, Conservateur des Archives de l'Académie des Sciences; and in Austin, Carol Mead, Archivist of the Archives of American Mathematics.

[51] erroneously numbered 23 in the Delsarte collection.

♠ Conveyed presumably in the missing letter from Rosser.

Topology, which he saw as his main business, it would have done him very little good—especially with Weil—to try to get the others interested.

TRIBU 25: Congrès oecuménique de Pelvoux-le-Poët (25 juin au 8 juillet 1951) [nbt_026.pdf]

PRÉSENTS: Cartan Delsarte Dieudonné Dixmier Godement Sammy Samuel Schwartz Serre Weil.

ABSENT: Koszul VISITEURS: Hochschild Borel COBAYE: Cartier Mirkic

p 3 The plan of Livre I is confirmed as Introduction; I: description of formal mathematics; II: theory of sets; III: ordered sets, integers; IV: structures.

The commitments made by members are listed on pages 4 and 5: in particular Dixmier engages to make the final version of the logic section two months after Rosser gives his imprimatur.

p 5 Sammy: Rapport sur le rôle des foncteurs au Livre I, chapitre des Structures (janvier 52) Samuel promises to write the Introduction to Livre I, (with Weil) by December 1951; and Weil promises to write, with Rosser, the *Note Historique* for Livre I.

On pages 6-9 there is a detailed report on the set theory book; and the decision is there recorded to "send the list of all our axioms to Rosser: If he finds them "kosher", we will proceed immediately to near-final versions." *WEIL a décanulé le contre exemple de DIXMIER sur l'égalité des ε de deux relations équivalentes (voir "Lamentations")*

On page 7, the ordered pair will be taken as primitive. On page 8, it is decided to speak of schemas rather than implicit axioms. On page 9, it is noted that the opinion of Rosser is awaited, and that Weil will seek instruction from him.[‖]

TRIBU 26: Congrès Croupion (1 au 9 octobre 1951) [nbt_027.pdf]

PRÉSENTS: Cartan Dieudonné Dixmier Godement Samuel Schwartz Serre.

p 3 It is resolved to include the last sentence of the *JSL* article, that—in effect—all can be done in ZC.

p 4 *Rosser trouve kosher notre systême d'axiomes avec l'égalité des τ (remplacera ε pour raisons typographiques) de deux relations équivalents (sous la forme WEIL).*

Cartan wants to change Chapter II; so the meeting, feeling baffled, resolves to pursue discussion by letter and in a congress.

[‖] In the original, *"WEIL se fera tapiriser par ROSSER"*. In Normalien argot, a *Tapir* is a schoolchild whose parents pay a Normalien to give him evening lessons to catch up in Maths or in another discipline. A Cambridge translation would be that Weil would ask Rosser for a supervision on logic.

TRIBU 27: Congrès Croupion des Vosges, (8 au 16 mars 1952). [nbt_028.pdf]
PRÉSENTS: Cartan Dieudonné Dixmier Godement Samuel Schwartz.
INVITÉ: Grothendieck.

The minutes suggest that, much to the relief of certain others present, Grothendieck returned to Nancy in a huff after being told that all empty sets are equal but some are more equal than others.

TRIBU 28: Congrès de la motorisation de l'âne qui trotte (Pelvoux-le-Poët 25-6 au 8-7 1952) [nbt_029.pdf]
PRÉSENTS: Cartan Chevalley Delsarte Dieudonné Dixmier Godement Sammy Samuel Schwartz Serre Weil.
NOBLES VISITEURS ÉTRANGERS: Borel, de Rham, Hochschild

p 2 Livre I: Introduction finished; Chap I adopted; Chapter II: new version of Dieudonné to be examined in October; ditto Chapter III. Chapter IV (Structures): *une nouvelle rédaction sera polie cet automne par un Caucus Americain, puis envoyé au Congrès de Février. Note Historique: Samuel se fera tapiriser par Rosser à Ithaca.* On Logic, *on a décidé de rédiger l'appendice en style "intuitionistisable" (à la Dixmier).* On structures, *on décide d'essayer le système Sammy.*

p 5 Eilenberg engages to draft Chapters 1 and 4 of *Topologie Pédérastique*, and Serre to draft chapters 2 and 3 of the same. Samuel engages to prepare Chapter 4 (structures) and the Note Historique.

On pages 7–9 there is a further report on the book *Théorie des ensembles.* The problems addressed by category theory are starting to reveal themselves.

TRIBU 29: Congrès de l'incarnation de l'âne qui trotte (Celles-sur-plaine, 19-26.10.1952) [nbt_030.pdf]
PRÉSENTS: Cartan Koszul Serre Weil. QUASI-PRÉSENTS : Dixmier Schwartz
QUASI-ABSENT: Delsarte

p 4 *Samuel réclamera Rosser pour qu'il donne rapidement son avis sur les chap.I-II.*

Bourbaki consult Rosser.

H·12 Thus, behind the scenes, Bourbaki seem to have hungered for reassurance about their foundational book. Rosser appears to have left no record of his meetings or correspondence with Weil or Samuel; nor is there any in the Weil archives in Paris.[52] Perhaps the sequence of events began in Chicago,

[52] Cartier suggests that the absence of correspondence in the Weil archive may be due to the fact that most of the exchanges would have taken place through Rosser's coming to Chicago to seek out Weil on his home ground.

whither both Mac Lane and Weil were lured by Stone in 1947; perhaps it was Mac Lane, a founder member, who proposed that Weil be invited to present Bourbaki's foundational ideas to the Association for Symbolic Logic in December 1948; perhaps Rosser's review [Ro1] of [Bou49] led Bourbaki to contact him; perhaps he saw his rôle as that of protecting Bourbaki from error rather than steering them towards any particular account of logic or set theory. He himself was willing to consider widely differing accounts: he published shortly afterwards a book [Ro2] expounding the development of mathematics within Quine's system NF, of which the review [Cu] by Curry is illuminating; some years later he published a book [Ro3] expounding the Boolean-valued models approach to forcing, in something like a ZF context; and, once,[53] in private and perhaps with provocative intent, declared himself a finitist who disbelieved in the existence of infinite sets.

H·13 Rosser's son has written in [RoJr] of his father's character and achievements. The picture of the notoriously abrasive Weil presenting himself at Rosser's door as a humble seeker after truth refuses to come into focus; but Cartier's view is that Rosser was regarded by Bourbaki as arbiter of last resort in logic solely and simply because that is what Weil proclaimed him to be—Cartier recalls Weil describing Rosser as a good personal friend "who happens to know about logic"—and those members, such as Chevalley, Dixmier and Eilenberg, who knew enough of the work of Gödel and Tarski to doubt this assurance, did not feel there was enough at stake to make an issue of it.

H·14 The friendship between Weil and Rosser rested on some non-mathematical tie or common interest—Weil had a very wide range of interests—which brought them together, perhaps from 1947 when Weil came to Chicago, perhaps from the period before Weil's departure from the U.S. for Brazil in 1945. To quote Michael Wright, given Weil's notorious disesteem for logic, which he was scarcely reluctant to voice, it cannot have been founded on admiration for Rosser's professional achievement as a logician. But Weil always spoke warmly of him.

H·15 The three significant changes to Bou49 that yielded the system finally adopted, Bou54, were:

(H·15·0) to follow the Hilbert–Ackermann treatment of propositional logic;

(H·15·1) to treat quantifiers not as primitive signs but as derived from the Hilbert operator;

(H·15·2) to change the set-theoretic axioms from something like ZC to something like ZFC.

[53] according to the testimony of Gerald Sacks.

I can imagine Rosser suggesting (H·15·0) to Bourbaki, but not (H·15·1), unless in mischief; might the latter have been suggested by Mac Lane, the ex-pupil of Bernays, to Weil, the translator of [Hi2] ? If not, perhaps the suggestion came from Chevalley in early 1950, as suggested by Tribu 22, and then was explored by Chevalley in his Draft 5; then adopted provisionally in October 1950 and definitively in Dixmier's Draft 6 in 1951. Let us hope that Rosser's missing letter will re-appear and settle these questions.

H·16 As for (H·15·2), Weil and Dieudonné thought the change unnecessary, despite the criticisms of Skolem [Sk], as would Mac Lane. The day was probably carried by Chevalley, who was interested in Gödel's work on AC and GCH, and Cartan, who was against too fixed and narrow an axiomatic base.

Consider these failings of ZC, documented in the papers cited:

(H·16·0) ZC cannot prove that every set has a rank: see Model 13 in section 7 of [M9, §7];

(H·16·1) ZC cannot prove that every set has a transitive closure: see [M9, §12];

(H·16·2) ZC cannot prove that the class of hereditarily finite sets is a set: see [M6];

(H·16·3) Z cannot conveniently handle Gödel's concept of constructibility: see [M7, §4];

(H·16·4) ZC cannot do Shoenfield-style forcing: see [M11].

(H·16·5) ZC is unable to construct the direct limit discussed in [M7, Example 9.32, p.224];

(H·16·6) ZC cannot prove the determinacy of Borel games: see [Sta].

The reader may sense the problem common to the first five, namely, the absence in Z of explicit forms of replacement, even those supported by the Kripke–Platek system KP. But that amount is there in coded form: it is shown in [M7] that if Z is consistent, so is Z + KP; and in Z+ KP, those first five objections melt away; further, adapting Gödel's proof for ZF to proving the consistency of AC relative to that of Z + KP is straightforward; so the declarations of Weil, Dieudonné and Mac Lane that ZC is plenty for their mathematics merely mean that their mathematics makes very little use of the recursion-theoretic side of mathematics. They missed a lot: had they added to their chosen ZC the axioms of KP, their enhanced theory would be no stronger, consistency-wise, and no nearer the large cardinal axioms they dreaded, but would have given, to them and their followers, conceptual access to the beauty and power of post-Gödelian set theory.

With the last two, the problem is that proving the statement concerned necessarily goes beyond the consistency strength of ZC; very much so in the case of Borel determinacy and other assertions explored by Harvey Friedman; and therefore, if ZC is consistent, such proofs cannot exist even in ZC + KP.

H·17 There is a final class of results which are provable in ZC, or even in MAC, but whose proofs would, conceptually, involve a voyage into the world of ZF. Here are three examples, derived from arguments of Gödel, Solovay, Shelah, Raisonnier, H. Friedman and Martin about projective sets of reals:

if every $\mathbf{\Sigma}^1_3$ set is Lebesgue measurable, then every uncountable $\mathbf{\Pi}^1_1$ set has a perfect subset;

if every uncountable $\mathbf{\Pi}^1_1$ set has a perfect subset, then every $\mathbf{\Sigma}^1_2$ set is Lebesgue measurable;

every Borel game with integer moves is determined provided every Turing-closed such game is.

H·18 COMMENT The pages of *La Tribu* give the impression that Bourbaki finalised plans for a book only when the criticism and energies of the members had reached exhaustion. It is plain that individual members of the group did think, in their different ways, about foundational matters; one feels that a part, at least, of the oppressiveness of Bourbaki comes from the secrecy and anonymity of their activities, with the consequence that no one person would admit to responsibility for the outcome.

We must here leave our scrutiny of *La Tribu* and return to our discussion of the public consequences of Bourbaki's publications; and as we do, we become aware of the change from the sensitivity of individual perceptions to the crudity of collective decisions; much as Rostropovitch declared [Ste, p.249] a brutal entry of the brass in Lutosławski's Cello Concerto to conjure an image of the Central Committee at full strength.

Why use Bourbaki's formalisation ?

H·19 Bourbaki were starting up before the dust had settled from Gödel's discoveries; they wanted to steer clear of the problem of incompleteness; so they made what they thought would be practical decisions; but they could have made better ones. And it is that last message that has not yet reached the public: formalised mathematics need not be the dog's dinner that Bourbaki make of it.

The number 4523659424929 in the title of [M8], when inserted into Google, yields numerous hits, many of which are in Chinese or Japanese, and which, I am told, are contributions to an on-line discussion about the possibility of formalised mathematics on a computer, and that the conclusion being reached is that my calculations in [M8] show that automated theorem proving is an impossibility.

But that conclusion, though reinforced by the grotesque length of terms generated in Bourbaki's later editions, seems premature. The review [Got] of [M8] in *Mathematical Reviews* hints at the existence of simpler formalisations than even that of Bourbaki's first edition; but let us be explicit. Suppose we formalise mathematics with two binary relations $=$ and $\in$, propositional connectives $\neg$, $\&$, individual variables $x, \ldots$, the quantifier $\forall$, and a primitive symbol ꓘ for the class forming operator, with syntax to match, so that ꓘ$x\mathfrak{A}$ is what is commonly written as $\{x \mid \mathfrak{A}\}$; then the empty set, $\varnothing$, can be defined in six symbols, as ꓘ$x\neg x = x$ and its singleton, $\{\varnothing\}$, as ꓘ$x\forall y \neg y \in x$; eight symbols in all, including just one quantifier.

I ask those who would treat mathematics as a formalised text: why use a formalisation that defines the number One not in eight symbols but in 2409875496393137472149767527877436912979508338752092897 ?

Structuralism: a part but not the whole of mathematics

Mathematics and logic move on. After Hilbert and, in effect, after Bourbaki came Gödel; and after Gödel's work of the thirties—his completeness theorem, his incompleteness theorem and his relative consistency proof for AC and GCH$^{\diamondsuit}$—came a further major development in foundational ideas, stemming from the Eilenberg–Mac Lane theory of categories. Though there are differences between Bourbaki and the school of Mac Lane, they are closer to each other than either are to the set-theoretic conception of mathematics, and might conveniently be given the blanket label of *structuralists*.

H·20 Cartier again [Sen]:

> *"Most people agree now that you do need general foundations for mathematics, at least if you believe in the unity of mathematics. I believe now that this unity should be organic, while Bourbaki advocated a structural point of view."*
>
> *"In accordance with Hilbert's views, set theory was thought by Bourbaki to provide that badly needed general framework. If you need some logical foundations, categories are a more flexible tool than set theory. The point is that categories offer both a general philosophical foundation — that is, the encyclopædic or taxonomic part — and a very efficient mathematical tool to be used*

$^{\diamondsuit}$ for which, Bernays [Ber2] suggests, the inspiration may have been Hilbert's attempt in [Hi2] to prove CH by enumerating definitions.

in mathematical situations. That set theory and structures are, by contrast, more rigid can be seen by reading the final chapter in Bourbaki's Set Theory*, with a monstrous endeavour to formulate categories without categories."*

In the second quotation it is plain that what Cartier means by set theory is the very limited contents of the Bourbaki volume of that name; a far cry from what set theorists mean by set theory. On the other hand, in the first one, Cartier may be echoing a point made in [M4], that unity is desirable but not uniformity.

H·21 In mentioning uniformity we touch on very dangerous topics; Solzhenitsyn wrote that Stalin made people second-rate; a comment from Feyerabend [Fe] is here relevant:

p306 "*It is not the* interference *of the state that is objectionable in the Lysenko case, but the* totalitarian *interference that kills the opponent instead of letting him go his own way.*

Even in less threatening circumstances uniformity leads to a failure of critical understanding, and I fear that something of that kind has happened in mathematics as a result of the excessive influence of Bourbaki. Once a mistake is embedded in a monolithic system, it is hard to remove.□

A possible and regrettable consequence of the uniformising tendency of Bourbaki is Grothendieck's withdrawal from the group and subsequently from contact with other mathematicians: one wonders whether he felt threatened by Bourbaki in the same totalitarian way that Chevalley may have been shouted down by Dieudonné, Cantor was blocked by Kronecker, and Nikolai Lusin was menaced by, and Giordano Bruno[54] actually suffered, a death sentence.

H·22 On the question of the unity of mathematics, I should stress that I see the advent of structuralism in mathematics as a bifurcation from, not a development of, set theory. In some areas of mathematics equality up to an isomorphism is good enough whereas in others it is not, and structuralism and set theory are on opposite sides of this divide. So while in certain areas of mathematics, structuralism has had a great success, I believe that were the foundational ideas of the structuralists to extinguish all other ideas about the foundations of mathematics, it would be to the

□ The mathematical micro-society might here be suffering damage similar to that, analyzed by Hayek in [vHay3], done to macro-society by planning the unplannable.

54 Links between the conceptions of infinity of Nicholas of Cusa, Bruno and Cantor are discussed in the thoughtful essay of Hauser [Hau].

great impoverishment of mathematics.[55] Equally, mathematics would be the poorer were set-theoretic ideas to extinguish structuralist ideas: and in [M13] I shall develop the approach outlined in [M12] to probe the nature of the above divide and argue for a pluralist account of the foundations of mathematics. Dieudonné once wrote that we have not begun to understand the relationship between combinatorics and geometry; and I shall hope in [M13] to show that as in a classical tragedy, the Bourbachistes do not realise that what they seek is already to hand.

H·23 Meanwhile something of the dual nature of mathematics is conveyed by the friendly exchange [M]⋆ and [M4] between Mac Lane and myself; and the technically-minded will find in [M7] a close scrutiny of the system of set theory—a subsystem of ZC—that seemed natural to Mac Lane, and in [M9] an even closer scrutiny, if possible, of certain systems of set theory that are important to set theorists.

Mac Lane did not, be it noted, see the issue between topos theory and set theory as an ontological competition. My own view of set theory is that I think of particular superstructures of abstract ideas being called into being to solve particular problems, different superstructures being invoked at different times. I would not be perturbed if the superstructure that solves one problem is not right for another.

Personally I define set theory as the study of well-foundedness, and regard its foundational successes as occurring when it meets a need for a new framework for a "recursive" construction (in a suitably abstract sense). I don't think it succeeds at all in accounting for geometric intuition. That failure should not be allowed to obscure its successes; but nor should its successes be judged a reason for sweeping its failures under the foundational carpet.

There remains the eternal challenge of conveying to others the limitations they are putting on their conceptual universe by adopting exclusively one mode of thought. How does one prove to someone that he is colour-blind ? The victim has to be willing to notice that others have perceptions denied to him.⋆⋆

H·24 But, in mathematics at least, there are signs of these different perceptions. Cartier writes in [Sen]:

"Following the collapse of the Soviet Union, the Russians

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55 and of economics, as discussed in [M12].

⋆ Is its title an allusion to the "supreme insult" of *La Tribu*, N^o 15, page 5 ?

⋆⋆ A friendly critic of an earlier draft supplies the example of green and red tomatoes, which a red-green colour-blind person can distinguish by taste or by feel but not by sight alone, unlike fully-sighted persons.

have brought a different style to the West, a different way of looking at the problems, a new blood."

The group centred around Baire, E. Borel, and Lebesgue created a new view of analysis growing out of the insights of Cantor. Both Lusin and Janiszewski came from the East to sit at their feet, and returned home with a positive message. I wonder to what extent the Russian style that Cartier has noticed descends through Lusin from Baire. The writings of Graham and Kantor [GrK1, GrK2, GrK3] are here highly relevant, developing in detail, as they do, topics only touched on in [Sen] and [M1].

Another collapse

H·25 There is a collapse of intellectual level in French schools: see [Coi]. So far as mathematics goes, I believe that the cause of the collapse of mathematical understanding is the suppression of the teaching of logic, which, I suggest, is the consequence of Bourbaki's disastrous treatment of logic.

H·26 Aside I am not saying that nothing but logic should be taught, far from it, any more than I should say that an aspiring pianist should play nothing but scales. But training in logic will stand you in good stead in many fields, just as working at the studies of Liszt will give you greater command of the works of Beethoven and Chopin. Logic strengthens the mind just as Liszt strengthens the fingers; and both strengthenings then permit you to go on to greater things.

H·27 For further evidence, let us explore another story of intellectual collapse, namely that of the Italian school of geometry. Mumford writes[56] that the three leaders were Castelnuovo, Enriques and Severi; and that Castelnuovo was totally rigorous, whereas Enriques gave incomplete proofs but was aware of the gaps and tried to fill them; and Severi, after a brilliant start, wrote rubbish; and in effect killed the whole school.

Mumford thinks that the collapse started around 1930. The Italians were not short of ideas, but no one knew what had been proved. Zariski and Weil sorted out the mess; then Grothendieck revolutionised the subject. With Zariski were associated the Bourbachistes Weil, Chevalley, and Samuel.

H·28 Peano died in 1932 and was teaching until the day before his death. Bourbaki listed in their historical note these co-workers of Peano: Vailati, Pieri, Padoa, Vacca, Vivanti, Fano and Burali-Forti. Peano's biographer,

[56] email to Thomas Forster of 23.xi.94, visible at
`http://ftp.mcs.anl.gov/pub/qed/archive/209`

Hubert Kennedy, comments that most of them were kept out of university life:

Vailati (1863 - 1909) read engineering at Turin 1880-84. He came under Peano's influence and then read for a mathematics degree which he got in 1888. He went home to Lode, then was assistant to Peano (1892) and and then to Vito Volterra. He resigned his university position as assistant in 1899, and became a high school teacher. He then worked on logic and philosophy.

Pieri (1860-1913) graduated at Pisa in 1884, then taught at the Military Academy in Turin. He obtained a doctorate from Turin in 1891, and then taught projective geometry courses there. He got a job in 1900 at the University of Catania in Sicily, then in 1908 moved to Parma.

Padoa (1868-1937) taught in secondary schools and at a Technical Institute in Genoa. He applied unsuccessfully for university lectureships in 1901, 1909 and 1912. Late in life he held a lectureship in mathematical logic at the University of Genoa 1932-1936.

Vacca (1872-1953) was left-wing in politics in his youth; graduated in mathematics from Genoa in 1897; moved to Turin and became Peano's assistant. He discovered the importance of the unpublished works of Leibniz and told Couturat about them. He later took up Chinese language and literature.

I have been unable to discover anything about Vincenzo Vivanti.

Fano (1871-1952) had a rich father. He worked under Felix Klein, and then in Rome (1894) Messina (1899) and from 1901 as Professor at the university of Turin. He was expelled in 1938 by the Fascists. He worked mainly on projective and algebraic geometry.

Cesare Burali-Forti (1861-1931) graduated from Pisa in 1884; from 1887 he taught at the military academy in Turin. He gave an informal series of lectures on mathematical logic at the University of Turin in 1893/4 and was Peano's assistant 1894-6.

H·29 After the second world war, logical studies in Italy revived with the 1948 translation of and commentary on Frege by Ludovico Geymonat (1908–91), and furthered by Ettore Casari, (1933–) who learnt about the work in Vienna and Germany from Geymonat at Pavia and then completed his studies at Münster, where Heinrich Scholz had built up a school of logic. In the collection [GP] he asks:

> "*When - in the light of what later occurred - we look at Peano's and his followers' metalogical achievements, a crucial question arises: how could it be that these skills, these competences, not many*

> *years after what Hans Freudenthal liked to call "the Parisian triumph of the Italian phalanx" should have ceased not only to be a reference point for world-wide research, but even to appear on the Italian cultural scene ?"*

H·30 PROBLEM Is it possible that the collapse of Italian geometry was brought on by the suppression of Italian logic? The dates fit uncomfortably well.

Back to St Benedict

H·31 We must wind up our discussion. So far as Bourbaki's treatment of logic is concerned, the picture is rather sad: Hilbert attacks an admittedly difficult problem, entrusts the work to younger colleagues, and can, perhaps from embarrassment, barely bring himself to acknowledge the Gödel revolution. Bourbaki copy Hilbert's pre-Gödelian position, with its belief that foundational problems can be settled "once and for all", its identification of consistency, truth and provability, and its attempt to declare mathematics to be an uninterpreted calculus; and others copy Bourbaki. Progressively the misunderstanding spreads.

H·32 Eilenberg acknowledged that Bourbaki hadn't thought through their position on foundations clearly and that what they had provided was a mess. But then he felt foundations was always a work in progress, an outlook shared by several members of Bourbaki: but when, later, after the impact of category theory had become evident—especially after the adjoint functor theorem and Grothendieck's work—it was suggested they go back and do a fresh treatment of foundations from scratch, Weil vetoed the idea as a mis-application of energy and resources. Thus Weil, the tyrant, imposed his static view of logic on his colleagues.

It is striking that Chevalley who (as did Cartier) exerted himself to read Gödel's monograph on AC and GCH, spoke out against the boorish conduct of "an ex-member of Bourbaki" in [Ch].

H·33 A distinguished, non-French, mathematician, on reading an earlier draft of this essay, wrote that whilst he had noticed that Bourbaki's logic is very bad, and whilst he acknowledges that I have carefully explained how and why it is bad, nevertheless he does not understand how it is possible not to see the great unifying force and amplitude of the rest of Bourbaki's work. In his view,

> *"Bourbaki's epoch is gone, but it was a great epoch, and their achievements are as undying as Euclid's. We go forward starting where they ended."*

Those comments summarise our dilemma. Bourbaki's structuralist conception advanced certain areas of mathematics but tended to stifle others. How might we undo the admitted harm Bourbaki have done to the understanding and teaching of set theory and logic—subjects not at the forefront of their thought—whilst retaining the benefit of their work in the areas in which they were very much involved ?

H·34 That is a non-trivial tactical problem, and I have little faith in the ability of a central committee to solve it by decree. Indeed this whole lamentable saga calls into question the intellectual adequacy of "modern" managerialist centralising universities. It would be better, as I suggest below, to have many independent scholarly bodies such as are almost called for in Article 101 of Napoleon's university statutes.

H·35 One aspect of this problem, for votaries of mathematical logic, is the challenge of imbuing mathematicians with a lively post-Gödelian sense of the vitality of logic. Two encouraging signs for France are the pleasing, if ironical, circumstance that despite Bourbaki's dead hand, Paris has now acquired one of the largest concentrations of logicians on the planet, and the fact that since 2000, of the Sacks prizes bestowed by the Association for Symbolic Logic on doctoral dissertations of outstanding quality written on topics in logic, four have gone to dissertations written at French universities. It is greatly to be hoped that in consequence the trickle-down phenomenon will, over the next twenty years, work in the reverse direction, to restore the teaching of logic to schoolchildren in France and elsewhere.

Such a change is much needed, for on the educational front, a new dark age approaches. Following the dropping of logic in the curriculum, schoolchildren in France are no longer taught to prove theorems; they are given theorems as statements and then given exercises in their application. A generation is growing up without the urge towards rigour. When the battery of their calculator runs down and the calculator starts to make mistakes, how will they know ?

H·36 The phenomenon of creativity being arrested by excessive bureaucratic control is well-known to historians of past cultures. I quote from *The Fatal Conceit* by F. A. von Hayek [vHay3] for the following information and references concerning Ancient Egypt, Byzantium and mediæval China.

p 33 *In his study of Egyptian institutions and private law, Jacques Pirenne describes the essentially individualistic character of the law at the end of the third dynasty, when property was 'individual and inviolable, depending wholly on the proprietor' but records the beginning of its decay already during the fifth dynasty.*

Pirenne, J. (1934) *Histoire des Institutions et du droit privé de* *l'ancienne Egypte* (Brussels: Edition de la Fondation Egyptologique Reine Elisabeth)

This led to the state socialism of the eighteenth dynasty described in another French work of the same date which prevailed for the next two thousand years and largely explains the stagnant character of Egyptian civilization during that period.

Dairanes, Serge (1934) *Un Socialisme d'Etat quinze Siècles avant Jesus-Christ* (Paris: Libraire Orientaliste P.Geuthner)

p 44 *It would seem as if, over and over again, powerful governments so badly damaged spontaneous improvement that the process of cultural evolution was brought to an early demise. The Byzantine government of the East Roman Empire may be one instance of this.*

Rostovtzeff M. (1930) 'The Decline of the Ancient World and its Economic Explanation', *Economic History Review, II*; *A history of the Ancient World* (Oxford: Clarendon Press); *L'empereur Tibère et le culte impérial* (Paris: F.Alcan), and *Gesellschaft und Wirtschaft im Römischen Kaiserreich* (Leipzig: Quelle & Meyer).

Einaudi, Luigi (1948) 'Greatness and Decline of planned economy in the Hellenic world', *Kyklos* II, pp 193–210, 289–316.

p 45 *And the history of China provides many instances of government attempts to enforce so perfect an order that innovation became impossible*

Needham, Joseph (1954–85) *Science and Civilisation in China* (Cambridge: Cambridge University Press).

p 46 *What led the greatly advanced civilisation of China to fall behind Europe was its governments' clamping down so tightly as to leave no room for new developments, while Europe probably owes its extraordinary expansion in the Middle Ages to its political anarchy.*

Baechler, Jean (1975) *The origin of capitalism* (Oxford: Blackwell), page 77.

H·37 The general challenge to our mathematical culture might be seen as that of regeneration, such as, Charlemagne thought, faced Europe after barbarism engulfed Roman civilisation. But the "green shoots of recovery" spread only slowly from a centre: though the Institut Henri Poincaré might have had logic seminars in 1956, even if not part of a degree course, the Rue d'Ulm began teaching logic only 33 years later. So, like Charlemagne, let us turn for a solution to the decentralist Benedictine idea, the nature and strength of which is well-summarised in a passage [T-R, p. 121] of a book by a former Master of Peterhouse:

> *"In the darkening, defensive days of the sixth century, the Benedictine monastery had been the cell of Christendom: every cell independent, so that if one cell failed, another might survive."*

That independence still holds today: though they all follow the Rule of St Benedict, the Benedictine abbeys do not form an order; each is independent and each abbot is sovereign. The Benedictine idea led, in Oxford and Cambridge, to the founding of colleges, the founding statutes of the oldest colleges being expressly based on the Rule of St Benedict. The strength of Oxford and Cambridge as universities derives from the traditional independence of each college within the university, exemplified by the comment of an earlier Master of Peterhouse, the mathematician Charles Burkill, on a University proposal to establish a centralised Needs Committee for the totality of colleges, that "such a committee can only be mischievous".

There is no reason why the Oxbridge system of colleges, that is, of self-governing, self-recruiting, property-owning communities of scholars, should not be permitted to develop in other countries, and thereby encourage the "free market" approach to education expressed in my final quotation, from Feyerabend's most famous book, *Against Method*:

p 30 *"[Knowledge] is not a gradual approach to the truth. It is rather an ever increasing ocean of mutually incompatible and perhaps even incommensurable alternatives, each single theory, each fairy tale, each myth that is part of the collection forcing the others into greater articulation, and all of them contributing via this process of competition to the development of our consciousness."*

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