TOP DOWN OR BOTTOM UP ?

The case for a pluralist account of the foundations of mathematics

A. R. D. MATHIAS
ERMIT, Université de la Réunion

SLIDE 1

Following the discovery of Russell's paradox there were many attempts to avoid contradiction. The problem arose in Frege's having both an unbridled scheme of comprehension

$$\exists y \forall x[x \in y \iff \Phi(x)]$$

and global variables, that is, variables ranging over the whole domain of discourse; so that the y just mentioned is a possible value for x; and when we take for Φ the formula $x \notin x$, a contradiction results:

$$y \in y \iff y \notin y$$

SLIDE 2

The two leading cures for this problem took different tacks: Zermelo kept the global variables but weakened the scheme of comprehension to a scheme of separation:

$$\forall a \exists y \forall x[x \in y \iff (x \in a \ \&\ \Phi(x))]$$

whereas Russell kept the schemes of comprehension as full as possible subject to his replacing global variables by typed variables. His original system was complicated by his principles of ramification; but later workers simplified it to the simple theory of types that we know, of which the following is the comprehension schema:

$$\exists y_{n+1} \forall x_n[x_n \in y_{n+1} \iff \Phi(x_n)]$$

SLIDE 3

Reaction to these proposals seems to have been varied: Skolem in his 1922 paper, "Some remarks on axiomatised set theory," to be found in the Heijenoort anthology, on page 291, wrote:

> *"Until now, so far as I know, only *one* such system of axioms has found rather general acceptance, namely that constructed by Zermelo. Russell and Whitehead, too, constructed a system of logic that provides a foundation for set theory; if I am not mistaken, however, mathematicians have taken but little interest in it."*

SLIDE 4

and in the *Schlussbemerkung* on page 300:

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

So Skolem thought that no-one was interested in type theory, and that set theory was plainly defective.

SLIDE 5

Alonzo Church, in a paper, written at the end of the 30s and reviewed in the *Journal of Symbolic Logic* volume V, page 78, about "The present situation in foundational studies" defined the central problem of the foundation of mathematics to be "the construction of a symbolic system within which the body of extant mathematics may be derived in accordance with sharply stated and immediately applicable formal rules," and contested the claims of Russell and Zermelo to have achieved that.

SLIDE 6

On the other hand, Alan Turing, in the *Journal of Symbolic Logic* volume XIII, number 2, June 1948: Practical Forms of Type Theory, pages 80 – 94, begins:

> *Russell's theory of types, though probably not providing the soundest possible foundation for mathematics, follows closely the outlook of most mathematicians.*

SLIDE 7

It appears, though, that many, at the time that Bourbaki began operations, thought that the simple theory of types and Zermelo's account of set theory were essentially the same, whether or not one accepted the claims of either to be a foundation for all of mathematics, so that it might have been said that a mathematician who preferred the one to the other was making a choice of style but not of content.

SLIDE 8

That view was refuted by Kemeny in his Princeton thesis of 1949, when he showed that Z proved the consistency of type theory.

In fact the two lines of thought — sets and types — were already starting to separate: set theory had forged ahead, through the addition to Zermelo's system proposed by Skolem and Fraenkel of the scheme of replacement. There had already been significant use of the axiom of replacement: two examples that come to mind are the discussion of measurable cardinals by Ulam and Tarski, and the consistency proof for AC and GCH as presented in Gödel's monograph.

SLIDE 9

I sense that Bourbaki fell between these two stools as they were starting to move in different directions. Their choice of the Hilbert operator seems to have stemmed from the early enthusiasm of Chevalley and Dieudonné. There is a review by Church in the JSL **4** 1939 of the latter's essay *Les Méthodes Axiomatiques* which praises Dieudonné — coming from Church that is praise indeed — but with these reservations:

> *The reviewer has only the criticism that the paper is disproportionately dominated by the great name of Hilbert; Frege receives but the briefest passing mention; and the resolution of the paradoxes by the (simple) theory of types is set forth in some detail with no indication at all of its source.*

SLIDE 10

Dieudonné in that essay declares that the paradoxes are solved through logistic methods and Hilbert; and one has the feeling that that, so far as Dieudonné was concerned, was the end of the story. Indeed, it would seem that the matter was settled, in Dieudonné's eyes, prior to 1925.

SLIDE 11

It seems to me that the difference that evolved at the beginning of the twentieth century between set theory and type theory is the same split that Dieudonné perceived in his essay of 1980 between "logic" and "mathematics", and is still with us today as the split between the *absolutists*, who are developing the insights that originate in set theory and are concerned with global mathematics, and the *structuralists*, who are developing the insights that originate in type theory and are concerned with local mathematics; and that the central problem of foundational studies today is to effect a reconciliation between these two camps.

SLIDE 12

In 1899 Hilbert published his *Grundlagen der Geometrie*: what I fear is that the completeness of Euclidean geometry led him to believe that arithmetic would also prove to be complete.

Hilbert wrote in 1922 a paper* in which he sketched a proof of the consistency of a weak fragment of arithmetic; he had a single unary arithmetical function f and he introduced the notation $\tau(f)$: he defined $\tau(f) = 0$ if $\forall a f(a) = 0$; $=$ least a such that $f(a) \neq 0$ otherwise; and was thus led to propose the axiom

$$f(\tau(f)) = 0 \implies f(a) = 0.$$

SLIDE 13

He sought to prove the consistency of a 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* $0 \neq 0$.

In a paper of 1924, † 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,

SLIDE 14

which supplies a witness to an existential statement rather than a counter-example to a universal one; he named that operator ε rather than 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 that Bourbaki followed the change of operator without changing the letter.

In 1927 von Neumann ‡ criticised Ackermann's paper and gave a consistency proof for first order number theory with induction for quantifier free formulæ.

SLIDE 15

Hilbert in 1927 gave a lecture, outlining Ackermann's paper and the method of "assign values then change your mind".

From "Logic in the twenties: the nature of the quantifier", by Warren Goldfarb, ¶ who emphasizes Hilbert's finitism:

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

---

* D. Hilbert 1922a: Die Logischen Grundlagen der Mathematik, *Mathematische Annalen* **88** (1923) 151–165.

† W. Ackermann 1924, Begründung des "tertium non datur" mittels der Hilbertscher Theorie der Widerspruchsfreiheit. *Mathematische Annalen* **93** (1924) 1–36.

‡ J. v. Neumann, Zur Hilbertschen Beweistheorie, *Mathematische Zeitschrift* **26** (1927) 1–46.

¶ *Journal of Symbolic Logic* **44** (1979) 351–368.

SLIDE 16

Hilbert-Bernays volume 2, of 1939, expounds the theory of the operator. Ackermann in 1940 proved the consistency of arithmetic by an induction up to the ordinal ε_0.

SLIDE 17

Bourbaki use the Hilbert operator but write it as τ rather than ε, which latter is visually too close to the sign $\in$ for the membership relation. 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 $\bullet$.

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 in English-speaking countries is usually called a well-formed formula.

SLIDE 18

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:

i) form τR, of length one more than that of R;
ii) link that first occurrence of τ to all occurrences of x in R
iii) 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.

SLIDE 19

Certain assemblies are *terms* and certain are *relations*. These two classes are defined by a simultaneous recursion:

T1: every letter is a term
T2: if A and B are terms, the assembly $\bullet AB$, in practice written (A, B), 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.
R5: If A and B are terms, the assembly $\in AB$ is a relation, in practice written $A \in B$.

SLIDE 20

Remark We get these two derived rules:
T3: if A and T are terms and x a letter, then $(A|x)T$ is a term.
R4: If A and B are terms, $=AB$ is a relation, in practice written $A = B$.

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

Quantifiers are introduced as follows:

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

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

SLIDE 21

Bourbaki's abbreviated structuralist definition of the number 1, when expanded into the primitive symbolism of the first edition of *La Théorie des Ensembles*, comprises 4,523,659,424,929 symbols together with 1,179,618,517,981 links between certain of those symbols.

If the ordered pair (x, y) is introduced by definition rather than taken as a primitive, then, according to a program in Allegro Common Lisp written by Solovay, the term defining 1 will have

$$2409875496393137472149767527877436912979508338752092897$$

symbols, with

$$871880233733949069946182804910912227472430953034182177$$

links.

SLIDE 22

In §2 of his text on Algebra, Godement 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.

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 set is closed under pairing. For that reason, an economical definition of ordered pair is desirable, such as is furnished by Kuratowski's definition.

SLIDE 23

In §2 N^{o}2 he declares that

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

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.

As a curiosity, let us show that his very refusal to pick a definition of ordered pair has implications for the strength of his system.

SLIDE 24

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

$\forall x \forall y \exists$ exactly one z with $F(x, y, z)$
$\forall z \forall u \forall v \forall x \forall y \big(F(u, v, z) \;\&\; F(x, y, z) \implies [u = x \;\&\; v = y]\big)$.

Let "$X \times_F Y \in V$" denote the formula

$$\exists W \forall w (w \in W \iff \exists x \exists y (x \in X \;\&\; y \in Y \;\&\; F(x, y, w)).$$

Write $\langle x,\, y\rangle_K$ for the Kuratowski ordered pair $\{\{x\},\, \{x,\, y\}\}$.

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.

SLIDE 25

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. ⊣

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 : by the scheme of separation, which we have been advised to take as an axiom, and by the axiom of union, which will be become available to us on a second reading of §3 N^{o}2, the lemma follows from the fact that $G``A \subseteq \bigcup\bigcup(A \times_F B)$. ⊣

SLIDE 26

Those lemmata yield the following

METATHEOREM *The scheme of replacement is provable in the theory whose axioms are those of extensionality, pairing, and union, plus the scheme of separation and 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.*

SLIDE 27

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. 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.

We have seen that that estimate, by Bourbaki, 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 adopt the von Neumann definition of 1 as $\{\varnothing\}$?

SLIDE 28

In a language where the class forming operator is a primitive symbol, the empty set, 0, can be defined in 6 symbols, and if, as I favour, one has primitive symbols for restricted quantifiers, the number 1 can be defined in nine symbols; otherwise with just ordinary quantifiers one can do it in thirteen.

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.

SLIDE 29

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 occur to him to wonder whether other formalisms might be possible ? One feels that his attitude towards logic is that of the Victorian schoolboy towards Latin, who in his heart thinks that no nonsense is too absurd to be a possible translation from a Roman author.

SLIDE 30

On page 25, there is a symptomatic footnote:

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

On page 31, another:

it is very difficult to use the signs $\exists$ *and* $\forall$ *correctly in practice, and it is therefore preferable to write "there exists" and "for all", as has always been done.*

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 ....".

SLIDE 31

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.*

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 aware of this.*

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.*

SLIDE 32

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.

SLIDE 33

Godement reminds me of a *mot* 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.

SLIDE 34

If their chosen system 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.

SLIDE 35

Turn now to a text-book that has had a wide following: *Cours de mathématiques, Tome 1, Algèbre,* by Jacqueline Lelong-Ferrand and Jean-Marie Arnaudiès, anciens élèves de l'École normale supérieure. Its first edition was in 1978; it has recently been reissued in a third edition. It cites two works in its Bibliographie:

R. Godement, *Cours d'algèbre,* published by Hermann in 1966

N. Bourbaki, *Théorie des ensembles,* published by Hermann in 1957.

The influence of those two works is very evident from the following passages concerning logic, set theory and the axiom of choice.

SLIDE 36

page 2: *une relation est alors dite vraie si elle peut être insérée dans une démonstration.*

page 3: *une relation est vraie si on peut l'insérer dans un texte démonstratif.*

..en pratique nous ne recontrerons pas de relation indécidable.

Finiteness is defined on page 32, but used earlier, on pages 10, 12, 17, 20 and 21. Some of these uses are really of finiteness in the metalanguage; but no such distinction is made by the authors.

It is stated that if A is not finite there is an injection of N into A: a covert use of the Axiom of Choice. About that axiom, they say the following:

SLIDE 37

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 object* x *pour lequel la relation* $A(x)$ *est effectivement vraie ?* [...]

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.*

[...]

SLIDE 38

Dans la mathématique formelle usuelle, l'axiome de choix est introduit dès le départ, à la'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.*

I have no idea what the authors understand by *"effectivement"*.

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 by Bourbaki.

SLIDE 39

They still say that they can have things provable without AC and provable with.

They are using "true" relative to some set of axioms, which they might change at will, but pretend that "true" has some absolute meaning.

On page 37: discussing the continuum hypothesis, they write:

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.

We shall see below that that last remark is very much in the style of Dieudonné's pronouncements on the foundations of mathematics.

SLIDE 40

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 pass an examination called the CAPES; and 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. Each of these examinations is on a syllabus specified by some national committee; and these syllabi serve as paradigms for the content of university licence and maîtrise courses, since universities seeking to attract students for theses course 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.

SLIDE 41

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. The programme for mathematics is divided into four sections; three of the sections are further divided into chapters, as follows:

SLIDE 42

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

SLIDE 43

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 vatiables réelles
VI. Notions de géometrie différentielle

4-Probabilités et statistiques

SLIDE 44

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 title of the first subdivision; but in section 1, a sentence is inserted:

Tout exposé de logique formelle est exclu.

Far be it from me to question the wisdom of this policy, or even the coherence with which it is applied; but is it too far-fetched to suggest that the origins of this prohibition is the Bourbachiste confusion over quantification ?

SLIDE 45

The syllabus for the Agrégation has not a word about logic.

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

SLIDE 46

A quotation from Quine's autobiography:

"A Logic Colloquium was afoot in the Ecole 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.' "

SLIDE 47

Dieudonné's philosophical opinions

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 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.

In this article Dieudonné emphasizes the split that he saw between *la logique mathématique et la théorie des ensembles d'une part, tout le reste de l'autre.* He goes on:

SLIDE 48

"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."*

SLIDE 49

He goes on to say that most mathematicians are formalists.

"Les spéculations sur les "grands" cardinaux ou ordinaux laissent froids 95% entre eux, car ils n'en rencontrent jamais."

Dieudonné makes both his position, and his ignorance of developments in logic, very clear. What is puzzling is his choice of the date 1925, but that might be an allusion to the completion of Ackermann's thesis.

SLIDE 50

Paragraph I 3 of the essay reads, in part:

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.")

SLIDE 51

Dieudonné claims to be speaking of facts; indeed he might almost be claiming, in 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*, Fraenkel and Bar-Hillel say 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:

SLIDE 52

"*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 refutes Dieudonné's suggestion that foundational work has had no influence: Weyl, at least, was influenced to move away from areas where the paradoxes had manifested themselves.

SLIDE 53

In his essay, *Liberté et Science Moderne,* Dieudonné 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.*"

SLIDE 54

In his last book, *Mathematics, the Music of Reason* 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.

That is close to an intuitionist position: so I am intrigued to find the following passage in the *Intelligencer* interview with Cartier:

SLIDE 55

"*The Bourbaki were Puritans, and Puritans are strongly opposed to pictorial representations of their faith. The number of Protestants and Jews in the Bourbaki group was overwhelming. And you know that the French Protestants especially are very close to Jews in spirit. I have some Jewish background and I was raised as a Huguenot. We are people of the Bible, of the Old Testament, and many Huguenots in France are more enamoured of the Old Testament than of the New Testament. We worship Jaweh more than Jesus sometimes.*"

SLIDE 56

During a lecture at Oxford in 1976 I ventured some remarks on a possible connection between religious and mathematical positions; they are summarised in the text of that lecture in the Oxford volume edited by Gandy and Hyland. Put crudely, my equations were

$$\begin{aligned} \text{Platonism} &= \text{Catholicism;} \\ \text{Intuitionism} &= \text{Protestantism;} \\ \text{Formalism} &= \text{Atheism;} \\ \text{Category Theory} &= \text{Dialectical Materialism.} \end{aligned}$$

They express similarities between possible attitudes to religious and to mathematical truth. Hence the above paragraph from Cartier revives my belief in the possibility of such speculative connections.

SLIDE 57

From the writings of Leo Corry in *Synthèse* and in his book on the evolution of twentieth-century algebra, I have learned that Bourbaki did not believe in their own foundations; it appears that the book on the theory of sets was desired by some members of Bourbaki, led perhaps by Chevalley who may have written it, but not by all; and that the notion of structure put forward in this book was later ignored.

It seems, indeed, that what Bourbaki was trying to do in achieving clarity and rigour for mathematics of a certain kind was later done much better by Mac Lane, Eilenberg and their school; the unarticulated aims of the two groups seem to have been much the same. When Bourbaki say that mathematics is structural and when Mac Lane says that it is protean, I believe that they are making the same statement.

SLIDE 58

Is it too fanciful to suggest that structuralism is the doctrine that naturally results if one is preoccupied with local mathematics ? And that this is the natural outgrowth of a type-theoretic mindset ?

For then there will be many localities for one's mathematics; so that one will believe that mathematics is structural (Bourbaki) or that it is protean (Mac Lane); and one's theories will have many applications, as shown by J.L.Bell in his book on *Toposes and Local Set Theory*, from the Epilogue of which I extract this long quotation:

SLIDE 59

p 236: Thus gradually arose the view that the essence of a mathematical structure is to be sought not in its internal constitution as a set-theoretical entity, but rather in the form of its relationship with other structures through the network of morphisms. (In particular, it came to be seen that the notion of equality appropriate for structures is not numerical identity, but isomorphism *an idea going back in principle to Dedekind and Klein.) This view, strikingly reminiscent of the operational structuralism associated with, for example, linguistics and psychology, was espoused most resolutely by the Bourbaki school in France, which has proposed a 'structuraliste' account of mathematics in the 1930s. However, although the account of mathematics they give in their justly famed* Éléments de Mathématique *is manifestly structural in intention,*

SLIDE 60

their uncritical employment of 'axiomatic' set theory in their formulation of the concept of mathematical structure prevented them from achieving the structuralist objective of treating structures as autonomous forms with no specified substance. In fact it was not until the 1940s that an axiomatic framework for mathematics was developed that was more in keeping with the spirit of operational structuralism, viz., the theory of categories and functors *devised by Eilenberg and Mac Lane. Here for the first time we have a theory which takes the notions of structure and morphism as primitive (as* objects *and* arrows *respectively) and which is indifferent to any set-theoretic construction that structures may possess.*

SLIDE 61

An application to algebra

EXAMPLE Let P_0 be the real vector space $\mathbb{R}[t]$ of all real polynomials. Let P_{n+1} be the dual of P_n. Then, using AC and setting β_n to be the size of a basis of P_n, one may show that $\beta_0 = \aleph_0$ and $\beta_{n+1} = 2^{\beta_n}$ for every n. Thus the operation taking each space to its dual, or taking each space to its bidual, necessarily raises the cardinality of the space at each step after the first. We may conclude that the operation of taking the dual of a space is not homogeneous.

SLIDE 62

It follows that if we write $DS(n)$ to mean that the sequence of spaces $P_0, \dots P_{n-1}$ exists — in other words a sequence of real vector spaces of length n starting from $\mathbb{R}[t]$ and taking the dual at each step, then MAC cannot prove that $\forall n DS(n)$; Z can prove that but cannot prove the existence of the infinite sequence $\langle P_n \mid n \in \omega \rangle$, nor the existence of the direct limit of the P_{2n}'s under the natural embedding of a space in its bidual, nor of the dual of that space, the projective or inverse limit of the P_{2n+1}'s, any of which existential statements imply in MAC the consistency of Z.

SLIDE 63

I invite readers to consult my paper The Strength of Mac Lane set theory for a discussion of these points:

- The equiconsistency of Mac Lane set theory and the simple theory of types with an axiom of infinity
- The relationship between Mac Lane, Kripke–Platek and Quine set theories
- The strange result that KPP + V=L proves the consistency of KPP.
- A strange model of KPP + AC in which every von Neumann aleph has a successor, but the well-ordering of the continuum is longer than any aleph.

SLIDE 64

Recently Joan Bagaria and I have been collaborating with Carlos Casacuberta on a paper. The theorem is not yet ready to be announced, but it is on the lines that if there is a supercompact cardinal then one can say something nice about certain functors, thus improving a special case of an earlier theorem of Casacuberta and others giving a consequence of Vopěnka's principle for the structure of certain functors.

SLIDE 65

Casacuberta addressed the set theory seminar earlier this year, and began with a formulation of Vopěnka's principle that was entirely clear (to me). He then said that he could not understand what he had just said, and so would have to translate it into CAT terms, which over the next half hour he did, reaching a formulation that I could not understand. Armed with that formulation he could then derive his result.

SLIDE 66

In our attempts to weaken his hypothesis, Joan and I had to translate the argument into set theory; and then once a simpler set theoretical argument had been found, translate it back into the language of category theory, so that Casacuberta could verify it.

So here we have a plain case of two groups of mathematicians making deductions by mutually incomprehensible arguments, but reaching the same conclusion.

To my mind this is plain evidence that set theory is for many mathematicians not the basis of their thought. On the other hand there are plainly many mathematicians for whom set theory is a very natural basis to their thought.

SLIDE 67

In an earlier draft of our paper we used the concept of cardinality (so natural to a set theorist); Andreas Blass commented that in many categories the concept of cardinality is problematical as objects don't necessarily have members; and we are accordingly recasting our arguments to avoid the use of cardinality.

SLIDE 68

Kai Hauser gave a talk in Barcelona in June about Cantor's frame of mind which brought out Cantor's attempt to reduce everything to arithmetic. As Hauser said, that is a Pythagorean attitude. The Pythagoreans tried to reduce everything to rational numbers; but the diagonal of a unit square is NOT. So there is a geometrical construction that transcends a particular arithmetical framework.

But then the cube root of 2 is not constructible by the classical geometrical means of ruler and compass; so the see-saw moves to the other side.

SLIDE 69

It seems to me undeniable that there are two fundamental mathematical intuitions, which I have called the arithmetical and the geometrical, or the temporal and the spatial, or the left-brain and the right-brain. Here is an example of the impossibility of sustaining two different notions at once:

SLIDE 70

Ernst Gombrich *Art and Illusion*, Princeton 1956, page 6, writes:

> *"A master of introspection, Kenneth Clark, has recently described to us most vividly how even he was defeated when he attempted to "stall" an illusion. Looking at a great Velasquez, he wanted to observe what went on when the brush strokes and dabs of pigment on the canvas transformed themselves into a vision of transfigured reality as he stepped back. But try as he might, stepping backward and forward, he could never hold both visions at the same time."*

SLIDE 71

So it has been my opinion that Cantor was misguided in seeking to reduce geometry to arithmetic. Something is achieved by the effort, as witness the success of Descartes' introduction of co-ordinates; but not everything.

There is a similar split between the two principal responses to Russell's paradox: the type-theoretic and the set-theoretic, regarding type theory as the superstructure of geometry, and set theory as an abstract version of arithmetic.

SLIDE 72

So there is a great division among mathematicians between those who are happy with the idea of set theory as the foundation of their discipline and those who are not. I have tried to account for that in various ways; recently a new approach suggested itself to me on reading Kai Hauser's paper "Is the Axiom of Choice self-evident ?" which I recommend to your attention.

Hauser in his paper has a striking image, that if you enter an auditorium (if say you were a concert pianist about to give a recital) you would notice the audience before you would notice any particular member of it. That is an example of what I mean by "Top down".

On the other hand, set theory is "bottom up", as comes across very clearly in this quotation from Gödel.

SLIDE 73

> *"A set is something obtainable from the integers (or some other well-defined objects) by iterated application of the operation 'set of', not something obtained by dividing the totality of all existing things into two categories."*

—Gödel, 1947

SLIDE 74

So I wonder whether this distinction is a more vital one than the others I have tried.

Perhaps a good example of an area where a top-down approach seems the most natural is that of mathematical physics, where one is starting from an external world and trying to give a description of it.

On the other hand, arithmetic itself seems incontrovertibly to be bottom up.

For example, there is a hidden induction at work every time you use the word "finite": which is why I do not buy the Category claim to be able to found mathematics without set theory. I need set theory to tell me what "finite" means.

SLIDE 75

There is a paper by Peter Johnstone called "the point of pointless topology", which I regard as a success for the top-down approach.

"Top down" seems to go back to Parmenides, with his obsession with The One. On the other hand, such concepts as induction, of which the Greeks seem to have had an idea, are "bottom up"; extending that to the transfinite, one sees for example that constructibility is indubitably "bottom up" and has been most fertile in solving problems originating in other parts of mathematics.

SLIDE 76

So let me nail my colours to the mast: I believe that some but not all of mathematics is correctly based on set theory, (specifically the "bottom up" part of mathematics) and that some but not all of mathematics (the "top down" part) requires a different foundation. Many claim that Category theory is that foundation; it plainly does a lot and is a source of inspiration; but I do not believe that it can claim to be a foundation for the whole of mathematics.

SLIDE 77

In the Library at Oberwolfach there is a copy of *Mathematics and Logic* by Mark Kac and Stanisław M.Ulam. On page 125, it is alleged that the statement

$$1 + 2 + \cdots + n = \frac{n(n+1)}{2}$$

cannot be proved without induction. An unknown hand, using red ink, has commented in the margin:

"This <u>can</u> be proved without induction: namely

$$\begin{aligned} 2a &= (1 + 2 + \cdots + n) + (n + (n-1) + \cdots + 1) \\ &= (1+n) + (2 + (n-1)) + \cdots + (n+1) = n(n+1).\text{''} \end{aligned}$$

Who is right ?

SLIDE 78

Answer: Neither. The induction is in the definition.

Notice the dots in the expression $1 + 2 + \cdots + n$, resolve to be precise and define the symbol $\Sigma_{i=1}^{n} a(i)$, by induction. For $n = 1$ it is $a(1)$; and $\Sigma_{i=1}^{k+1} a(i) = \left(\Sigma_{i=1}^{k} a(i)\right) + a(k+1)$; then the above proof uses four inductions:

$d + \Sigma_{i=1}^{n} c(i) = \Sigma_{i=1}^{n+1} d(i)$ where $d(1) = d$ and $d(i+1) = c(i)$.

$\Sigma_{i=1}^{n} a(i) = \Sigma_{i=1}^{n} b(i)$ provided $b(i) = a(n-i)$ for each i.

$\Sigma_{i=1}^{n} a(i) + \Sigma_{i=1}^{n} b(i) = \Sigma_{i=1}^{n} (a(i) + b(i))$

$\Sigma_{i=1}^{n} c(i) = n.c$ if each $c(i) = c$.

All those are concealed by the dot notation plus the visual appeal to common sense.

SLIDE 79

Euclid's theorem (due to Eudoxus ?) that there are infinitely many prime numbers is, when fleshed out — Euclid seems to have proved it by example — that given any finite S set of primes there is a prime not in S. Namely form the product of primes in S: **induction is used** here in defining the product of a finite set of primes; indeed the very notion of finite involves induction; add one; and appeal to another result of Euclid, **proved using induction**, that any integer has a prime divisor.

Contrast that with Euler's proof by considering an infinite product. Where has the induction gone ?

SLIDE 80

A theorem of Fermat

THEOREM *Let p be an odd prime number. Then p is expressible as the sum of two squares if and only if* $p \equiv 1 (\mathrm{mod}\, 4)$.

Fermat's method was to show first that some positive multiple mp of p is a sum of two squares, and then to show that if $m > 1$, an m' with $1 \leqslant m' < m$ can be found for which $m'p$ is also the sum of two squares; hence the least such m must be 1.

So in Fermat's proof there is a visible appeal to **the least number principle**. Details may be found in *The Theory of Numbers* by G. H. Hardy and E. M. Wright, or in *Fermat's Last theorem for Amateurs* by Paulo Ribenboim.

SLIDE 81

Contrast that with a more modern proof as given in *Algebraic Number Theory* by Jürgen Neukirch, which remarks as before that -1 is a square $\bmod p$; deduces that in the ring $\mathbb{Z}[i]$ of Gaussian integers, p is not a prime, and (**using induction behind the arras**) is therefore reducible; and concludes, by considering the norm of a product that equals p, p is a sum of two squares.

SLIDE 82

Hilbert's basis theorem was viewed with suspicion initially because of its proof. He later reminisced:

Mein erster Beweis für die Endlichkeit des vollen Invariantensystems ist von der Art, dass darin die transfinite Schlussweise wesentlich ist und nicht herausgeschafft werden kann...

.....P.Gordan hatte ein gewisses unklares Gefühl für die transfinite Schlussweise in meinem ersten Invarianten Beweise: er brachter dasselbe zum Ausdruck, indem er den Beweise als "theologisch" bezeichnete. Er modifizierte dann die Darstellung meines Beweises durch Einziehung seiner Symbolik und glaubte damit den Beweis seines "theologischen" Characters entkleidet zu haben. **In Wahrheit war die transfinite Schlussweise nur hinten dem Formalismus der Symbolik versteckt worden**.

SLIDE 83

See S. G. Simpson, Ordinal numbers and the Hilbert Basis Theorem, *Journal of Symbolic Logic* **53**, 1988, pp. 961–974, for a discussion of the necessity of an induction longer than ω.

SLIDE 84

In many texts on complex analysis, when singularities are discussed, it is desired to prove that the trichotomy:

i Laurent series about a with no non-zero coefficients for negative terms

ii Laurent series with some but only finitely many non-zero coefficients for negative terms

iii Laurent series with infinitely many coefficients for negative terms

coincides with the trichotomy

SLIDE 85

1 possesses a finite limit as $z \to a$

2 tends to infinity as $z \to a$

3 goes haywire as $z \to a$

Titchmarsh, Copson, ... give faulty proofs and merely say that removable singularities are of little importance. But the act of removing a singularity of that kind is an essential part of the discussion of poles.

Henri Cartan, *Theorie Elémentaire des fonctions analytiques d'une ou plusieurs variables complexes*, gives a sound treatment.

SLIDE 86

Bourbaki and the question of intellectual terrorism.

I see great dangers in succumbing to the essentially dictatorial idea formulated by Dieudonné that there is a unique best way of doing any given portion of mathematics.

From Miles Reid's book *Undergraduate Algebraic Geometry*, London Mathematical Society Student Texts, 12, 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.*"

SLIDE 87

"*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.*"

SLIDE 88

"*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.*"

SLIDE 89

Let us set against Reid's remarks a comment 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.*"

SLIDE 90

I quote from Paul Feyerabend, *Against Method.*

p30: *"It 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."*

SLIDE 91

More Feyerabend: here he describes modern science: I believe it is what might happen to mathematics if the "commissaire of police" mentality of Dieudonné were allowed to triumph:

p188: *"Late 20th century science has given up all philosophical pretensions and has become a powerful business that shapes the mentality of its practitioners. Good payment, good standing with the boss and the colleagues in their "unit" are the chief aims of these human ants who excel in the solution of tiny problems but who cannot make sense of anything transcending their domain of competence. — Let somebody make a great step forward, and the profession is bound to turn it into a club for beating people into submission."*

SLIDE 92

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."*

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 great impoverishment of mathematics.

SLIDE 93

I quote from the "peer review" of a research proposal submitted by set-theorists:

"Give examples (specifically) of classical mathematical problems which have been illuminated by modern set theory. If one wants to convince mathematicians that this type of research is as valuable to fund as geometry, number theory, analysis, etc..., the proposal should be much more specific about achieved and potential applications to other parts of mathematics."

SLIDE 94

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 for the following information concerning Ancient Egypt, Byzantium and mediæval China.

SLIDE 95

p33: *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. This led to the state socialism of the eighteenth dynasty described in another French work of the same date (Dairaines, 1934) 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)

Pirenne, J. (1934) *Histoire des Institutions et du droit privé de l'ancienne Egypte* (Brussels: Edition de la Fondation Egyptologique Reine Elisabeth)

SLIDE 96

p44: *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.

SLIDE 97

And the history of China provides many instances of government attempts to enforce so perfect an order that innovation became impossible.

p45: *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.*

Needham, Joseph (1954–85) *Science and Civilisation in China* (Cambridge: Cambridge University Press).

Baechler, Jean (1975) *The origin of capitalism* (Oxford: Blackwell).

SLIDE 98

In the CAT camp:

Bourbaki's chosen treatment of logic has led to widespread fear and misunderstanding of that subject; and the banning of its teaching in the schools of France is a sign of failure; and it would seem highly desirable for their opening volume to be recast on a more palatable logical basis.

Mac Lane's treatment of structuralist mathematics is a much better one, but when he claims that mathematics can do without set theory, his enthusiasm has taken him beyond what can be justified.

SLIDE 99

In the SET camp:

Set theory is principally concerned with the study of well-foundedness. It is a powerful language, into which large amounts of mathematics may formally be translated, although it may be doubted whether in some cases the underlying intuitions survive the translation.

SLIDE 100

Each camp can point to insights and successes denied to the other camp; but neither camp can claim to supply the conceptual needs of all mathematicians.