VOL. XXXVIII. No. 149.] [JANUARY, 1929.

MIND

A QUARTERLY REVIEW

OF

PSYCHOLOGY AND PHILOSOPHY

I.—MATHEMATICAL PROOF.[1]

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1. I HAVE chosen a subject for this lecture, after much hesitation, not from technical mathematics but from the doubtful ground disputed by mathematics, logic and philosophy; and I have done this deliberately, knowing that I shall be setting myself a task for which I have no sufficient qualifications. I have been influenced by three different motives. In the first place, the exercise will be good for me, since it will force me to think seriously about questions which a professional mathematician like myself is apt to neglect. Secondly, it is difficult to find a branch of pure mathematics suitable for popular exposition in an hour. Finally if, in a desperate attempt to be interesting, I lose myself in discussions where I am admittedly an amateur, then, whoever I may offend, I should certainly not have offended the founder of this lectureship and the Rouse Ball chair.

I do not regret my choice, but I am bound in self-defence to begin with a double apology. The first is to any real mathematical logicians who may be present. I am myself a professional pure mathematician in the narrow sense, and, in my own subject, quite as intolerant of amateurs as a self-respecting professional should be. I have therefore no difficulty in understanding that mathematical logic also is a subject for professionals; that it demands a detailed knowledge which I do not possess and, so long as I am active in my proper sphere, have hardly leisure to acquire; and that I am certain to be guilty of all sorts of confusions

[1] Rouse Ball Lecture in Cambridge University, 1928.

which would be impossible to a properly qualified logician. Indeed there is only one thought which gives me courage to proceed, and that is that I may be concerned less with strictly logical questions than with questions of general philosophy. However treacherous a ground mathematical logic, strictly interpreted, may be for an amateur, philosophy proper is a subject, on the one hand so hopelessly obscure, on the other so astonishingly elementary, that there knowledge hardly counts. If only a question be sufficiently fundamental, the arguments for any answer must be correspondingly crude and simple, and all men may meet to discuss it on more or less equal terms.

My second apology must be addressed to those mathematicians who dislike all discussions savouring of philosophy. But if I apologise to them, it is perhaps with less sincerity. I feel that this distaste is usually based on no better foundation than an unreasoning shrinking from anything unfamiliar, the distaste of the pragmatist for truth, of the engineer for mathematics, of the pavilion critic at Lords for the in-swinger and the two-eyed stance. It is reasonable to ask an audience like this to put aside this dislike of the fundamental for its own sake.

You must also remember that ordinary mathematics has a good deal at stake in some of these recent controversies. These controversies have seemed to threaten methods which we have used with confidence for nearly one hundred years. There are familiar elementary theorems—that any aggregate of real numbers has an upper bound, that any infinite aggregate has a point of condensation—the truth of which is simply denied by the 'intuitionist' school of logicians. There are also theorems of an apparently much less abstract or suspicious type, theorems for example in the theory of numbers, the only known proofs of which depend, in appearance at any rate, on principles which they reject.

2. It may not be possible to distinguish precisely between mathematics, mathematical logic, and philosophy, as the words are currently used. We can, however, by considering a few typical problems, recognise roughly the disputed tracts across which the boundaries must be drawn.

(i) *Is Goldbach's Theorem true?* Is any even number the sum of two primes? This is a strictly mathematical question to which all questions of logic or philosophy seem irrelevant.

(ii) *Is the cardinal number of the continuum the same as that of Cantor's second number class?* This again appears to be a mathematical question; one would suppose that, if a proof were found, its kernel would lie in some sharp and characteristically mathematical idea. But the question lies much nearer to the borderline of logic, and a mathematician interested in the problem is likely to hold logical and even philosophical views of his own.

(iii) *What is the best system of primitives for the logic of propositions?* This is a question of mathematical logic in the strict professional sense. A logician qualified to discuss it will probably belong to some more or less definite philosophical school, but it is hardly likely that his philosophical views will have any very noticeable influence on his choice.

(iv) *What is a proposition, and what is meant by saying that it is true?* This, finally, is a problem of simple philosophy.

It is often said that mathematics can be fitted on to any philosophy, and up to a point it is obviously true. Relativity does not (whatever Eddington may say) compel us to be idealists. The theory of numbers does not commit us to any particular view of the nature of truth. However that may be, there is no doubt that mathematics does create very strong philosophical *prejudices*, and that the tests which a philosophy must satisfy before a mathematician will look at it are likely to be very different from those imposed by a biologist or a theologian. I am sure that my own philosophical prejudices are as strong as my philosophical knowledge is scanty.

One may divide philosophies into *sympathetic* and *unsympathetic*, those in which we should like to believe and those which we instinctively hate, and into *tenable* and *untenable*, those in which it is possible to believe and those in which it is not. To me, for example, and I imagine to most mathematicians, Behaviourism and Pragmatism are both unsympathetic and untenable. The philosophy of Mr. Bradley may be just tenable, but it is highly unsympathetic. The Cambridge New Realism, in its cruder forms, is very sympathetic, but I am afraid that, in the forms in which I like it best, it may be hardly tenable. 'Thin' philosophies, if I may adopt the expressive classification of William James, are generally sympathetic to me, and 'thick' ones unsympathetic. The problem is to find a philosophy which is both sympathetic and tenable; it is not reasonable to hope for any higher degree of assurance.

3. The crucial test of a philosophy, for a mathematician, is that it should give some sort of rational account of *propositions* and of *proof*. A mathematical theorem is a proposition; a mathematical proof is clearly in some sense a collection or pattern of propositions. It is plain then that if I ask what are, to a mathematician, the most obvious characteristics of

a mathematical theorem or a mathematical proof, I am inviting philosophical discussion of the most fundamental kind. I wish to begin, however, by being as unsophisticated as I can, and I will therefore try to sketch what seems to be the view of mathematical common sense, the sort of view natural to a man who does not profess to be a logician but has spent his life in the search for mathematical truth. It is after all the misapprehensions of such a man that a logician may find the least fundamentally unreasonable and the least hopeless to remove.

I will begin then by enumerating some rough criteria which I think that a philosophy must satisfy if it is to be at all sympathetic to a working mathematician. I know too well how probable it is that just the most sympathetic philosophies will prove untenable.

(1) It seems to me that no philosophy can possibly be sympathetic to a mathematician which does not admit, in one manner or another, the immutable and unconditional validity of mathematical truth. Mathematical theorems are true or false; their truth or falsity is absolute and independent of our knowledge of them. In *some* sense, mathematical truth is part of objective reality.

'Any number is the sum of 4 squares'; 'any number is the sum of 3 squares'; 'any even number is the sum of 2 primes'. These are not convenient working hypotheses, or half-truths about the Absolute, or collections of marks on paper, or classes of noises summarising reactions of laryngeal glands. They are, in one sense or another, however elusive and sophisticated that sense may be, theorems concerning reality, of which the first is true, the second is false, and the third is either true or false, though which we do not know. They are not creations of our minds; Lagrange discovered the first in 1774; when he discovered it he discovered *something*; and to that something Lagrange, and the year 1774, are equally indifferent.

(2) When we know a mathematical theorem, there is something, some object, which we know; when we believe one, there is something which we believe; and this is so equally whether what we believe is true or false.

It is obvious that by this time we have escaped only too successfully from the domain of platitude and triviality. We have done no more than to make explicit a few of the instinctive prejudices of the 'mathematician in the street'. Yet with our first demand we have antagonised at least two-thirds of the philosophers in the world; and with the second we have reduced our first indiscretion to entire insignificance, since we have committed ourselves, in one form or another, to the objective reality of propositions, a doctrine rejected, I believe, not only by all philosophers, but also by all three of the current schools of mathematical logic.

(3) In spite of this I am going farther, and in a direction relevant to the recent controversies concerning 'transfinite' mathematics to which I shall return later. Mathematicians have always resented attempts by philosophers or logicians to lay down dogmas imposing limitations on mathematical truth or thought. And I am sure that the vast majority of mathematicians will rebel against the doctrine—even if it is supported by some of themselves, including mathematicians so celebrated as Hilbert and Weyl—that it is only the so-called 'finite' theorems of mathematics which possess a real significance. That 'the finite cannot understand the infinite' should surely be a theological and not a mathematical war-cry.

No one disputes that there are infinite processes which appear to be prohibited to us by the facts of the physical world. It is true, as Hilbert says, that no mathematician has completed an infinity of syllogisms. It is equally true that there is no mathematician who has never drunk a glass of water, and, so far as I can see, one of these facts has neither more nor less logical importance than the other. There is no more *logical* reason why a mathematician should not prove an infinity of theorems in this world than why he should not (as he has been so often encouraged to hope) emit an infinite sequence of musical notes in the next.

The history of mathematics shows conclusively that mathematicians do not evacuate permanently ground which they have conquered once. There have been many temporary retirements and shortenings of the line, but never a general retreat on a broad front. We may be confident that, whatever the precise issue of current controversies, there will be no general surrender of the ground which Weierstrass and his followers have won. 'No one', as Hilbert says himself, 'shall chase us from the paradise that Cantor has created': the worst that can happen to us is that we shall have to be a little more particular about our clothes.

4. Such then are the presuppositions and prejudices with which a working mathematician is likely to approach philosophical or logical systems. How far are they satisfied by the existing schools of mathematical logic? There are three such schools, the logisticians (represented at present by Whitehead, Russell, Wittgenstein, and Ramsey), the finitists or intuitionists (Brouwer and Weyl), and the formalists

(Hilbert and his pupils). I am primarily interested at the moment in the formalist school, first because it is perhaps the natural instinct of a mathematician (when it does not conflict with stronger desires) to be as formalistic as he can, secondly because I am sure that much too little attention has been paid to formalism in England, and finally because of the title of my lecture and because Hilbert's logic is above everything an explicit theory of mathematical proof. I must begin by a rapid summary of the most striking differences between these schools and of the difficulties which have brought them into existence. It is not my object to discuss these difficulties in detail, but what I have to say later can hardly be intelligible unless I give some sort of general explanation of their character. I can fortunately base this explanation on the extremely clear account of the situation given recently by Ramsey.

5. (1) I shall refer to the logisticians generally under the short title of 'Russell'. It is necessary to say that by 'Russell' I mean the Russell of *Principia Mathematica*. *Principia Mathematica* is not a treatise on philosophy, but it has a philosophical background, with which I am in general sympathy. I think that I can understand, in broad outline, how the logical edifice can support itself on *that* foundation. The problem of erecting it on the foundation of Russell's latest philosophical writings is one which I prefer to leave to bolder minds.

To Russell, then, logic and mathematics are substantial sciences which in some way give us information concerning the form and structure of reality. Mathematical theorems have *meanings*, which we can understand directly, and this is just what is important about them. In this, I may observe, Russell and the so-called 'intuitionists' are in complete agreement; and (since it is something of this sort which seems to me the natural implication of the word) I should prefer to avoid the use of 'intuitionism' as distinguishing one school from the other.

Mathematics is to Russell, up to a certain point at any rate, a branch of logic. It is concerned with particular kinds of assertions about reality, with particular logical concepts, propositions, classes, relations, and so forth. The propositions of logic and mathematics share certain general characteristics, in particular complete generality, though this is not an adequate description of them. There is no particular reason that I can see why any of this should be distasteful to us as mathematicians. It does not seem to conflict with the criteria which I suggested a moment ago; it seems likely at first sight even to indulge our desire for real propositions, though here we are ultimately disappointed.

There are certain definite points at which Russell's attempted reduction of mathematics to logic fails. In this, of course, there is nothing likely to astonish an unsophisticated mathematician. That mathematics should follow naturally, up to a point, from purely logical premisses, premisses to whose simplicity and 'self-evidence' no one can reasonably take exception, when proper allowance is made for the element of sophistication inevitable in a highly complex structure; but that it should then prove necessary to import fresh raw material and add new assumptions—all this is only what a mathematician might expect. In particular, I think that this is true of two of the three 'non-logical' axioms necessary in Russell's scheme; the Axiom of Infinity, that the universe contains an infinity of individuals, and the Multiplicative Axiom or Axiom of Zermelo, which is very famous but required only in particular theorems which might conceivably be discarded, and which I need not stop to explain, since I shall not refer to it further.

6. The situation is quite different with the third axiom, the notorious Axiom of Reducibility. The point here is much more important and also much more difficult. It is essential that I should say something about it, impossible that I should explain it fully. I cannot hope to find popular language clearer than Ramsey's, and I shall follow him very closely.

The theory of aggregates, in the classical form of Cantor and Dedekind, leads to certain antinomies, of which the most famous is Russell's paradox of the class of classes which are not members of themselves, a concept which may be shown to lead at once to flat contradiction. Russell met the difficulty by his Theory of Types.

Suppose that we are given a set S of properties, defined as being all properties of a certain kind K. Given an object x, we can ask whether x possesses *any* property of the kind K. If x has *any* such property, this is another property of x, say Σ; and we can then ask whether Σ can be itself a property of the set S, that is to say of the kind K. It is natural to suppose that the answer must be negative, since the idea of Σ already presupposes the totality S; and this is in fact Russell's answer. The property Σ is, he says, a property 'of higher order' than any property belonging to S; and so generally we must classify properties according to their orders, and any property defined by reference to all properties of a certain order must be a property of higher order. It is impossible to make any statement which is significant for properties of all orders simultaneously. Further, since, in Russell's logic, statements about classes are merely disguised statements about their defining properties, classes also must be divided into orders, and any statement about 'all' classes must really be confined to all classes of a certain order. This doctrine seems inherently plausible, and leads to an easy solution of Russell's and similar antinomies.

The theory of types has, however, very unfortunate mathematical consequences, since it appears to destroy some of the most fundamental theorems of analysis. The typical theorem is the theorem that any aggregate of numbers has an upper bound, a theorem which is substantially the same as what, in my *Pure Mathematics*, is called 'Dedekind's Theorem'. A real number is defined as a class of rationals. Suppose now that we are given a set S of real numbers x, *i.e.* a set of classes of rationals. The upper bound U of S is defined as the class of rationals which is the logical sum of the classes defining the various members of X, and it is taken for granted that this class stands on the same footing as the classes of which it is the sum. But a moment's consideration shows that this is not so. The classes which are the members of X are defined by certain properties of rationals, and the class which is U is defined by the property of belonging to *some one or other* of these classes, that is to say of possessing some one or other of these properties. Thus the defining characteristic of U involves a reference to *all* the defining characteristics of members of X, and is therefore a characteristic of higher order. It follows that, if we were to attempt to develop analysis without further assumptions, we should have to distinguish real numbers of different orders. We should have to say that the upper bound of an aggregate of real numbers of order n was a real number of order $n + 1$, and so on; and this, whether practicable or not (a point about which I express no opinion) would certainly be extremely inconvenient and probably intolerable.

Russell meets this difficulty by the Axiom of Reducibility, which asserts roughly that there is a property of the lowest order equivalent to any property of any order, not of course equivalent in *meaning*, but equivalent in *extension*, so that any object which possesses the one possesses the other, and they define the same class. The upper bound U may then be defined, not only by the property used to define it above, but also by the equivalent property of lower order, and it is thus a real number in the same sense as each of the numbers of which it is the upper bound. It is not disputed by anybody, so far as I know, that the axiom does yield a solution of the problem. Analysis can be developed in the classical manner and without further difficulty when once the truth of the axiom is granted; and there seems to be no ground for supposing that the axiom will lead to contradiction.

There are, however, objections to the axiom, about the force of which opinions may perhaps differ, but which have proved sufficient to prevent all other logicians from accepting it. It is complicated and (what is more important) very *unconvincing*. It has none of the 'self-evidence' of the properly logical assumptions; and it is obvious that Russell himself dislikes it very heartily and regards its presence in his system as a most regrettable necessity. Finally, an argument suggested in the rough by Ramsey, and developed in a more precise form by Waismann, appears to show conclusively that the axiom is definitely *not* a 'truth of logic' in the same sense as the other primitive propositions of *Principia Mathematica*. It is therefore impossible to regard Russell's solution as satisfactory, and this is about the only point on which the logicians, Russell himself included, are unanimous.

7. (2) I pass to the finitists, Brouwer and Weyl, and I shall dismiss them very shortly. Much as I admire the contributions of Brouwer and Weyl to constructive mathematics, I find their contribution to logic singularly unsympathetic. Finitism rejects, first, all attempts to push the analysis of mathematics beyond a certain point, and for this I see no sort of justification. I have no particular desire to be committed to the extreme Russellian doctrine, that all mathematics is logic and that mathematics has no fundamentals of its own. If it should turn out that there are parts of mathematics irreducible to logic, I do not see why I should be particularly distressed. On the other hand I see no reason for denying that, up to a point, the reduction has actually been made, and the arguments for denying in principle the possibility of a further reduction seem to me entirely inconclusive. That there is some particular sanctity about the notion of an integer which should protect it against the humiliation of further analysis, that general existential propositions have no real significance, that there is some peculiar certainty in knowledge based, in some sense, in immediate perception of a finite number of sensible things—all these are dogmas to which the finitists seem to be committed; and all of them seem to be founded on philosophical doctrines with which I have no sympathy, which indeed I find it extremely difficult to understand, and which seem to me, so far as I can understand them, to rest on all

sorts of questionable assumptions, and in particular on an impossibly naïve attitude towards our knowledge of the physical world.

This, however, is a minor point for a mathematician. What is much more serious to a mathematician is that the mathematical consequences of finitism involve rejection not (like those of denying the Multiplicative Axiom) of particular isolated outworks of mathematics but of integral regions of ordinary analysis. It is no use trying to deny that the finitists have the better of the argument up to a point; the parts of analysis which they admit are unquestionably, at present, in a more secure position than the rest; and so long as finitism merely insists on this its position is unassailable. I cannot believe that mathematicians generally will be so ready to accept a check as final, so anxious to find metaphysical reasons for supposing that the prettiest path is that which passes on the side of the hedge away from the bull.

8. (3) I go on then to consider the logic of Hilbert and his school; and here I find it very necessary to distinguish between Hilbert the philosopher and Hilbert the mathematician. I dislike Hilbert's philosophy quite as much as I dislike that of Brouwer and Weyl, but I see no reason for supposing that the importance of his logic depends in any way on his philosophy.

I am sure that the Hilbert logic has been unreasonably neglected by English logicians. 'The formal school', says Ramsey, 'have concentrated on the propositions of mathematics, which they have pronounced to be meaningless formulæ to be manipulated according to certain rules, and mathematical knowledge they hold to consist in knowing what formulæ can be derived from what others consistently with the rules. Such being the propositions of mathematics, the account of its concepts, for example the number 2, immediately follows: "2" is a meaningless mark occurring in these meaningless formulæ. But, whatever may be thought of this as an account of mathematical propositions, it is obviously hopeless as a theory of mathematical concepts; for these occur not only in mathematical propositions, but also in those of everyday life. Thus "2" occurs not merely in "$2 + 2 = 4$", but also in "it is 2 miles to the station", which is not a meaningless formula but a significant proposition, in which "2" cannot conceivably be a meaningless mark. Nor can there be any doubt that "2" is used in the same sense in the two cases, for we can use "$2 + 2 = 4$" to infer from "it is 2 miles to the station and 2 miles on to the

Gogs" to "it is 4 miles to the Gogs *via* the station", so that these ordinary meanings of "2" and "4" are clearly involved in "2 + 2 = 4".'

Let me say at once that this argument seems to me to be unanswerable and that, if I thought that this really was the beginning and the end of formalism, I should agree with Ramsey's rather contemptuous rejection of it. But is it really credible that this is a fair account of Hilbert's view, the view of the man who has probably added to the structure of significant mathematics a richer and more beautiful aggregate of theorems than any other mathematician of his time? I can believe that Hilbert's philosophy is as inadequate as you please, but not that an ambitious mathematical theory which he has elaborated is trivial or ridiculous. It is impossible to suppose that Hilbert denies the significance and reality of mathematical concepts, and we have the best of reasons for refusing to believe it: 'the axioms and demonstrable theorems,' he says himself, 'which arise in our formalistic game, are the images of the ideas which form the subject-matter of the ordinary mathematics'.

I must, however, begin with a few remarks about the philosophical background which seems to lie behind Hilbert's views; and here of course I need not be alarmed if I find myself disagreeing with him as hopelessly as with the finitists. Hilbert's philosophy appears indeed to be in broad outline much the same as Weyl's, as Weyl himself has very fairly pointed out. There is the same rejection of the possibility of any purely logical analysis of mathematics: 'mathematics is occupied with a content given independently of all logic, and cannot in any way be founded on logic alone.' There is the same insistence on some sort of concrete, perceptible basis, for which Hilbert (with what justice I have no idea) claims the support of 'the philosophers and especially Kant': 'in order that we should be able to apply logical forms of reasoning, it is necessary that there should first be something given in presentation, some concrete, extra-logical object, immediately present to intuition and perceived independently of all thought. . . . In particular, in mathematics, the objects of our study are the concrete signs themselves.' There is, I think, no doubt at all that Hilbert does assert, quite unambiguously, that the subject matter of mathematics proper is the actual physical mark, not general formal relations between the marks, properties which one system of marks may share with another, but the black dots on paper which we see.

I had better state at once what is to me a fatal objection to this view. If Hilbert has made the Hilbert mathematics with a particular series of marks on a particular sheet of paper, and I copy them on another sheet, have I made a *new* mathematics? Surely it is *the same* mathematics, and that even if he writes in pencil and I in ink, and his marks are black while mine are red. Surely the Hilbert mathematics must be in some sense something which is common to all such sets of marks. I make this point here, because there are two questions which suggest themselves at once about Hilbert's marks. The first is whether we are studying the physical signs themselves or general formal relations in which they stand, and the second is whether these signs or relations have 'meaning' in the sense in which the symbols of mathematics are usually supposed to have meaning. It seems to me that the two questions are quite distinct.

9. It is no doubt this philosophical outlook, and this consequent insistence on the importance of the physical mark or sign, that inspire Hilbert's finitism, which appears at first sight as extreme as that of Brouwer and Weyl themselves. I naturally find this attitude very disappointing; it seems to me that formalism is bound to die for want of air within the narrow confines of a finitistic system. But on the face of it Hilbert is entirely uncompromising: 'there is no infinite anywhere in reality', he says, and again 'is it not clear that, when we think we can recognise the reality of the infinite in any sense, we are merely allowing ourselves to be deceived by the enormity of the largeness or smallness which confronts us everywhere . . . ?'.

Hilbert says that 'infinite theorems', theorems such as 'there are infinitely many primes', are not genuine propositions but 'ideal' propositions. I am not at all sure what he means by an 'ideal proposition', but I suppose that one thing at any rate that he would say (if he used Russell's language) is that the infinite is essentially *incomplete*. We know that mathematics is full of 'incomplete symbols', symbols which have no meaning in themselves, though larger collections of symbols of which they are parts have perfectly definite meanings. There are, for example, the ordinary 'operational' symbols; '$\frac{d}{dx}$', '∇^2', '$\int_a^b \ldots dx$'. The most striking example is the '∞' of elementary analysis; we define '$\sum\limits_0^\infty$' and '$f(x) \to \infty$', but (at any rate in the ordinary presentations of the subject) we never define '∞' standing by itself. There is, in the classical analysis, no number ∞ standing on all fours with e or π; there is a sharp contrast here between the infinite of analysis and the infinite of geometry, in which 'the line at infinity', say $z = 0$, is on just the same footing as any other line.

It is one of Russell's admitted achievements to have recognised in a precise and explicit manner the immense importance of 'incomplete symbolism' in logic and philosophy also, and so to have shown how widely the correct analysis of a proposition may diverge from the analysis of unreflecting common sense. The standard example is that of propositions containing denoting phrases or *descriptions*, 'the so-and-so', 'the murderer', 'the author of Waverley'. The 'Waverley' argument applies to all propositions of the form 'a is the b', and shows that the proposition cannot be analysed, as the words expressing it suggest, into an assertion of identity between 'a' and 'the b'. I wish to know whether a is the b, whether Dr. Sheppard was the murderer of Roger Ackroyd; and in fact he was. If 'a' and 'the b' are the same object, I can substitute one for the other in any proposition without destroying its sense or its truth; and therefore it appears that what I really wanted to know was whether Dr. Sheppard was Dr. Sheppard, which is obviously false. It follows that the analysis was wrong, and that there is no such object in reality as 'the b'; 'a is the b' must be analysed in an entirely different manner.

I am not suggesting that Hilbert would accept the statement that the infinite is incomplete as an adequate account of his attitude towards it. No doubt he would want to go very much further. I have inserted this explanation merely (1) because I shall need it later and (2) because rival views about the infinite are apt to differ more violently in expression than reality, and the notion of an incomplete symbol might in some cases be a basis for a reconciliation between them. I have the less hope that it would do so in this case because Hilbert uses, as instances in support of his thesis that all 'infinite theorems' are in some sense 'ideal theorems', such divergent illustrations as (a) the infinite of analysis, (b) the infinite of geometry, and (c) the ideal numbers of higher arithmetic, and it seems to me quite impossible to regard all these as inspired by the same logical motive, the first representing a *purification* of mathematics by an agreement to regard certain notions as 'incomplete', the others an *enlargement* of it by the introduction of new elements as 'complete' as those which they generalise.

10. It is time, however, to proceed to some description of Hilbert's system, and I do this in language based upon that

of v. Neumann, a pupil of Hilbert's whose statement I find sharper and more sympathetic than Hilbert's own.

(1) Hilbert's logic is a theory of proof. Its object is to provide a system of formal axioms for logic and mathematics, and a formal theory of logical and mathematical proof, which (*a*) is sufficiently comprehensive to generate the whole of recognised mathematics, and (*b*) can be proved to be consistent. The system of *Principia Mathematica* fulfils the first but not the second criterion.

(2) If we can do this, we shall be troubled by antinomies no more. But for this end the whole existing apparatus of axioms, proofs and theorems must first be formalised strictly, so that to every mathematical theorem a formula will correspond. The structure of the formal system will of course be *suggested* by the current logic and mathematics. Every formula will *seem* to have a meaning, a meaning which we must afterwards forget.

(3) For example, we have the 'logical' formula

$$a \rightarrow (b \rightarrow a).$$

This is suggested by an obvious 'logical truth', the truth that (in Russell's symbolism) $a \,.\, \supset . \, b \supset a$, that a true proposition is a consequence of any hypothesis. This formula is an 'axiom', which means *simply* that it is one of the formulæ with which we start.

Similarly we have the formula (again an axiom)

$$Za \rightarrow Z(a + 1),$$

which is suggested by the 'mathematical truth' that $a + 1$ is an integer if a is one. We thus start with a finite system of axioms or 'given formulæ'. They are, so to say, the chessmen, the bat, ball and stumps, *the material with which we play.*

(4) We also need *rules for the game*, of which there are two. Rule (1) is that we may substitute one formula inside another, in the first instance inside an axiom, while Rule (2) is embodied in the 'scheme of demonstration'

$$\begin{array}{c} a \\ a \rightarrow b \\ b \end{array} \qquad \text{(A)}$$

(which corresponds to the 'non-formal principle of inference' in *Principia Mathematica*). Such a scheme is called a *demonstration*, a the *hypothesis*, b the *conclusion*. A formula is said to be *demonstrable* (1) if it is an axiom, or (2) it is b, a and $a \rightarrow b$ being axioms, or (3) it is b, a and

$a \rightarrow b$ being demonstrable, or (4) it is derivable from an axiom or a demonstrable formula by substitution. We have thus a quite precise concept of 'demonstration'. To use Weyl's illustration, we are playing chess. The *axioms* correspond to the given position of the pieces; the *process of proof* to the rules for moving them; and the *demonstrable formulæ* to all possible positions which can occur in the game.

(5) Let us observe in passing that there are far more axioms in Hilbert's scheme than in such a scheme as that of *Principia Mathematica,* and *no definitions* in the sense of *Principia Mathematica.* This is inevitable, since it is cardinal in Hilbert's logic that, however the formulæ of the system may have been suggested, the 'meanings' which suggested them lie entirely outside the system, so that the 'meaning' of a formula is to be forgotten immediately it is written down. The definitions of *Principia Mathematica* are the most important elements of the system, and embody 'philosophical' analyses of the meanings of the symbols used. The definition of a cardinal number, for example, presents to us at any rate one possible meaning of number, and tells us that that is the meaning with which Russell proposes to use the word. Hilbert is not concerned with that, or any, 'meaning' of 'number', and the only conceivable sense of a definition in his system is that of a symbolic convention which instructs us to replace a prolix formula by a more concise one.

11. (6) Mathematics proper, then, is reduced to a game like chess. We can, however, regard a game like chess from two quite different standpoints. In the first place we can *inspect,* or *construct,* chess, by reading the games whose aggregate constitutes chess, or playing new ones. Secondly, we can think and theorise *about* chess; we make judgements about it, and these judgements contain theorems which are in no sense part of the game. To take a definite illustration, which is in one form or another essential to the understanding of the Hilbert logic, we can judge, and in a sense *prove,* that *certain positions cannot occur.* There cannot be more than eighteen queens on the board; two knights cannot mate; these are true and provable theorems, not theorems *of* chess —the theorems *of* chess are the actual positions—but theorems *about* chess.

Similarly there is the Hilbert mathematics on the one hand, and what Hilbert calls 'metamathematics' on the other, the metamathematics being the aggregate of theorems *about* the mathematics; and of course it is the metamathematics which is the exciting subject and affords the real justification for our interest in this particular sort of mathematics. Suppose, for example, that we could find a finite system of rules which enabled us to say whether any given formula was demonstrable or not. This system would embody a theorem of metamathematics. There is of course no such theorem, and this is very fortunate, since if there were we should have a mechanical set of rules for the solution of all mathematical problems, and our activities as mathematicians would come to an end.

Such a theorem is not to be expected or desired, but there are metamathematical theorems of a different kind which it is entirely reasonable to expect and which it is in fact Hilbert's dominating aim to prove. These are the negative theorems of the kind which I illustrated a moment ago; they assert, for example, in chess, that two knights cannot mate, or that some other combination of the pieces is impossible, in mathematics that certain theorems cannot be demonstrated, that certain combinations of symbols cannot occur. In particular we may hope (and it is this hope that has inspired the whole construction of the logic) to show the impossibility of the combination

$$a \,.\, - \, a,$$

where $-$ is the symbol corresponding to the 'negation' of *Principia Mathematica*.

Let us suppose that our analysis of the game has established this, and then recur to the 'meanings' which suggested the game but were afterwards discarded. We may think about meanings *now*, because we are engaged in metamathematics, *outside* the game. It will plainly follow that the concepts and propositions which we symbolised cannot lead to contradiction. If this has been done, and for a formal system rich enough to be correlated with the whole of mathematics, the purpose of the Hilbert logic will have been achieved.

12. It is now time for me to interpolate a remark which gives the justification for the title of my lecture. It is obvious that to Hilbert *proof* means two quite different things. I have tried to anticipate the point in my choice of words: we fortunately have two words, *proof* and *demonstration*.

'Proof' has always meant at least two different things, even in ordinary mathematics. *We* distinguish vaguely and half-heartedly; in the Hilbert logic the distinction becomes absolutely sharp and clear. First, there is the *formal, mathematical, official* proof, the proof inside the system, the pattern (A), what I called the *demonstration*. These inside official proofs are, in the mathematics, the actual formulæ or patterns, in the metamathematics, the subject matter for discussion.

Secondly there are the proofs of the theorems of the metamathematics, the proof that two knights cannot mate. These are *informal, unofficial, significant* proofs, in which we reflect on the meaning of every step. The structure of these proofs is not dictated by our formal rules; in making them we are guided, as in ordinary life, by 'intuition' and common sense. 'Prof. Hardy will lecture at 12.0 to-day, because it says so in the *Reporter*, and because statements in the *Reporter* are always true.'

You must not imagine that the unofficial, metamathematical, non-formal, intuitionist proof is in any sense slacker or less 'rigorous' than the formal mathematical proof. The subject matter is abstract and complicated, and every step has to be scrutinised with the utmost care. We may even find it necessary to guide our thoughts by the introduction of new formalism, and it is quite likely that, if we do, we shall use over again the same symbols that we have used already. And here, of course, lies a danger; for we may be tempted to forget that we are using the same symbols in different contexts and with different aims; even Russell has been accused of making this mistake by logicians of the more formal schools. In the Hilbert logic at any rate the distinction is quite precise; the unofficial proof lies entirely outside the official system, and its object is simply *to produce conviction*, unofficial conviction of the absence of official contradiction —which is what we want.

13. At this point I should like to leave the Hilbert logic for a moment, and make a few general remarks about mathematical proof as we working mathematicians are familiar with it. It is generally held that mathematicians differ from other people in *proving* things, and that their proofs are in some sense *grounds* for their beliefs. Dedekind said that 'what is provable, ought not to be believed without proof'; and it is undeniable that a decent touch of scepticism has generally (and no doubt rightly) been regarded as some indication of a superior mind.

But if we ask ourselves why we believe particular mathematical theorems, it becomes obvious at once that there are very great differences. I believe the Prime Number Theorem because of de la Vallée-Poussin's proof of it, but I do not believe that $2 + 2 = 4$ because of the proof in *Principia Mathematica*. It is a truism to any mathematician that the 'obviousness' of a conclusion need not necessarily affect the interest of a proof.

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I have myself always thought of a mathematician as in the first instance an *observer*, a man who gazes at a distant range of mountains and notes down his observations. His object is simply to distinguish clearly and notify to others as many different peaks as he can. There are some peaks which he can distinguish easily, while others are less clear. He sees A sharply, while of B he can obtain only transitory glimpses. At last he makes out a ridge which leads from A, and following it to its end he discovers that it culminates in B. B is now fixed in his vision, and from this point he can proceed to further discoveries. In other cases perhaps he can distinguish a ridge which vanishes in the distance, and conjectures that it leads to a peak in the clouds or below the horizon. But when he sees a peak he believes that it is there simply because he sees it. If he wishes someone else to see it, he *points to it*, either directly or through the chain of summits which led him to recognise it himself. When his pupil also sees it, the research, the argument, the *proof* is finished.

The analogy is a rough one, but I am sure that it is not altogether misleading. If we were to push it to its extreme we should be led to a rather paradoxical conclusion; that there is, strictly, no such thing as mathematical proof; that we can, in the last analysis, do nothing but *point*; that proofs are what Littlewood and I call *gas*, rhetorical flourishes designed to affect psychology, pictures on the board in the lecture, devices to stimulate the imagination of pupils. This is plainly not the whole truth, but there is a good deal in it. The image gives us a genuine approximation to the processes of mathematical pedagogy on the one hand and of mathematical discovery on the other; it is only the very unsophisticated outsider who imagines that mathematicians make discoveries by turning the handle of some miraculous machine. Finally the image gives us at any rate a crude picture of Hilbert's metamathematical proof, the sort of proof which is a *ground* for its conclusion and whose object is to *convince*.

On the other hand it is not disputed that mathematics is full of proofs, of undeniable interest and importance, whose purpose is not in the least to secure conviction. Our interest in these proofs depends on their formal and æsthetic properties. This is almost always so with *logical* proofs; Theorem 3·24 of *Principia Mathematica* is the law of contradiction, and it is certainly not because we require to be convinced of its truth that we are prepared to study its elaborate deduction from equally 'self-evident' premisses. Here we are interested in the pattern of proof *only*. In our practice as mathematicians, of course, we cannot distinguish so sharply, and our proofs are neither the one thing nor the other, but a more or less rational compromise between the two. Our object is *both* to exhibit the pattern and to obtain assent. We cannot exhibit the pattern completely, since it is far too elaborate; and we cannot be content with mere assent from a hearer blind to its beauty.

14. Let us return to the Hilbert logic. The very structure of the logic, its mere existence, are enough, I think, to prove two propositions of great importance. The first is that it is possible to establish the consistency of a system of axioms *internally*, that is to say by direct examination of its structure; and the second is that it is possible to prove a system consistent even when the axioms embody logical principles such as the law of contradiction itself. Each of these propositions has been disputed.

Consider for a moment the ordinary procedure of axiomatic geometry. In abstract geometry we consider unspecified systems of things, a class S of objects A, B, C, . . . which we call *points*, and sub-classes of these objects which we call *lines*. We make certain assumptions about these points and lines, which we call *axioms*, such as that there is a line which contains any given pair of points, that there is only one such line, and so on. To lay down a system of axioms in geometry is simply to limit the subject matter, to say that we propose to consider only objects of certain kinds. Thus, in a geometry which contains the two axioms I have mentioned, our 'points' might be the players in a tournament, and our 'lines' the opponents in a game, but the points and lines could not be undergraduates and colleges, because then the axioms would be untrue.

In a geometry we are not concerned with any *particular* meaning of 'point' or 'line'. We may say, if we like, that we are concerned with *all possible* meanings, or that we are not concerned with meanings at all; we might accept Hilbert's language, and say that we are concerned simply with *marks*, or we might say (what would, I think, be at any rate one stage nearer to the truth) that we are concerned with what Wittgenstein calls *forms*. It is possible that the question is mainly one of words. We assume merely that our unspecified subject matter possesses the properties stated in our axioms, and we set out to investigate its other properties, the theorems of our geometry, by the usual processes of logical inference.

Every geometry demands a *consistency theorem*, which is naturally not a theorem of the geometry. We have to prove that the axioms do not contradict one another. We produce an example, an 'interpretation', of the geometry, a set of objects which actually have the properties attributed by the axioms to our points and lines. In general in these discussions we take arithmetic or analysis for granted, and our example is one in which points and lines are sets of numbers. Thus our points might be the numbers 1, 2, and 3, and our lines the classes 23, 31, 12: these objects do in fact satisfy the particular axioms which I mentioned. It was by this process, for example, that the old difficulties about the possibility of non-Euclidean Geometry were ultimately settled. It has always been held, and no doubt correctly, that in Geometry, where only the 'subject-matter' is symbolised, and there is no attempt to symbolise the process of inference itself, there is no other possible method.

If we try to apply a similar process to arithmetic, we are met by a difficulty. It is natural that a mathematician should wish to treat arithmetic axiomatically, to say not (with *Principia Mathematica*) that a number is such or such a particular object, but that numbers are any set of objects which have certain properties: there are so many plausible definitions of a number, and the reasons for selecting one rather than another seem so purely technical. There is, however, an obvious difficulty about the inevitable proof of consistency. When we wanted such a proof for a geometry, we could appeal to arithmetic; but there is nothing in ordinary mathematics which comes before arithmetic, and it is not easy at first to see where any 'example' is to be found. There seems only one possibility, if we are to pursue the established method, and that is to find an example in which the rôle of number is played by some logical construct, such as the Frege-Russell class of similar classes, which can be shown to have the properties required. If we approach the subject from a standpoint different from that of *Principia Mathematica*, we may say that this is what the authors of that work have actually done.

Finally, if we have established consistency in geometry and arithmetic, can we do so in logic, or in a subject which includes logic? It has been held, and I think by Russell, that we cannot, because our formulæ symbolise, among other things, the logical processes which we use in examining it, because the rules of the game are required in forming the judgement that what purports to be an instance of the game really is one. Other logicians, with whom here I agree, have held that this is a misunderstanding, due to a failure to distinguish between the use of our symbolism inside and outside the formal system.

My own view is that even here the classical method, the method of instances, is available in principle, and that, in restricted subjects such as the logic of classes or of propositions, it can be and has been successfully carried through. If, however, we are as ambitious as Hilbert, so that our system is to cover the whole field of abstract thought, I imagine that the attempt to do what we want on these lines is hopeless. I cannot imagine where we could find an adequate image of so comprehensive a symbolism, except in the whole field of thought which it was actually constructed to symbolise. There remains only the 'internal' method followed by Hilbert, based on study of the formal properties of the rules themselves. Whatever we may think about the philosophical basis on which Hilbert has erected his system, and with whatever success he or his followers may pursue it, it seems to me unquestionable that this method is valid in principle, in mathematics in exactly the same sense as in chess. And in this case Hilbert is entirely justified in his claim that he has found a necessary condition for all systems of mathematical logic, and that 'even the assertions of intuitionism, however modest they may be, require first a certificate of authorisation from this tribunal'.

15. My remarks up to this point have been mainly explanations of things which I think I understand. The rest of what I have to say amounts to little more than a confession of a series of perplexities.

The first question which you will naturally ask is this: granted that Hilbert's method is valid in principle, what has it *done?* How far has the proof of consistency progressed? *Does* it establish freedom from contradiction in a domain co-extensive with mathematics? So far as I know the answer is, up to the present, *No.* There has been very substantial progress, and consistency has been proved up to a point beyond the point up to which success might be expected to be easy. The region accounted for includes the mathematics of the finitists, and that part of *Principia Mathematica* which is independent of the Axiom of Reducibility; but this region does not cover analysis.

It would be very reasonable to ask me, as an analyst, to explain my own attitude towards this hiatus in the foundations of analysis, and I do not profess to be able to give any satisfactory answer. I could only say this: in the first place, I am no finitist; I believe that the analysis of the text-books is true. Secondly, Ramsey has advanced a solution, which he does not profess to regard as entirely satisfactory, but in which I can find a good deal of encouragement. Ramsey makes a distinction, which seems to me obviously valid, between the properly mathematical antinomies, those which (like Russell's) would appear, unless precautions against them were taken, in the structure of mathematics itself, and those which appear to arise from some epistemological or psychological confusion concerning 'meaning' or 'definition'. He observes that Russell's theory of types can be divided into two parts, of which only the first, which is harmless, is required in order to dispose of the first category of antinomies, the second, from which all the trouble arises, being needed only for the antinomies of the second kind. He then puts forward a new theory, which might be described roughly as a revival, with appropriate safeguards, of the old-fashioned theory of classes in extension. In this theory there is no need for any axiom of reducibility; and this is at any rate the *sort* of solution that I should like to see. I cannot really doubt that there is a class which is the logical sum of any given set of classes, and this, or something like it, is all that is required by the Dedekind theory.

16. I will return for a moment in conclusion to the properly 'philosophical' question to which I referred at the beginning, about the reality or 'completeness' of propositions. I am entirely unable to exorcise my craving for real propositions, a weakness which is after all only natural in a mathematician, to whom mathematical theorems ought to be the first basic reality of life. But I can find no sort of encouragement wherever I turn.

Our first instinct is to suppose that a judgement, whether true or false, must be analysable into a mind and an object in relation. In a sense this is admitted to be true by everybody; it is undisputed that there is something objective, what Russell and Wittgenstein call the 'proposition as fact', which enters into any judgement. When we judge, we form a picture of the reality about which we are judging, a form of words, a set of marks or noises, which we suppose, rightly or wrongly, to afford an image of the facts. This is the 'proposition as fact'; the question is, what, if anything, is there more?

It can hardly be questioned that there is *something* more, something which is common to a whole class of factual propositions. If I say that 'George is the father of Edward', I create a factual proposition. If I and all other men say it, in all languages printed, written, or spoken, and formalise it in every conceivable symbolism, we create a class of facts, and there will plainly be something common to all these facts. This also is admitted; all such factual propositions have something in common, something which may be called their *form*. This, however, is by no means enough to satisfy me, since 'Edward is the father of George' has *the same* form as 'George is the father of Edward', while the propositions, if such there be, are plainly different.

In Russell's 'multiple relation' theory, the theory of truth accepted provisionally in the first edition of *Principia Mathematica*, no such entity as the proposition is recognised. A judgement is a complex of objects, of which a mind is one, my mind and 'George' and 'Edward' and 'fatherhood', if we treat all these for simplicity as simple objects. If George *is* the father of Edward (so that the judgement is true) then there is a smaller complex, the 'fact' that George is the father of Edward, which is a part of the larger complex which is the judgement. If the judgement is false, there is no such subordinate complex. In neither case is there anything which can be called the 'proposition'. First descriptions, then classes, then propositions have been washed away into the ocean of the incomplete.

I have myself always detested this theory of truth. Apart from my bewilderment about how a structure such as that of *Principia Mathematica* could possibly be built up on so bottomless a foundation, Russell's theory has always seemed to me to banish entirely the element of correspondence which I have felt to be essential in any theory. My own difficulty has always been this, that I find it impossible not to believe in false or uncertain propositions and almost equally difficult to believe in true ones. When we judge truly, there is something which is admitted, namely the fact; and it seems unreasonable to insist on the independent existence of the proposition as something distinct from either the judgement or the fact. When we judge falsely, there is no fact, and, unless we admit the proposition, there seems to be no foundation for our judgement. It seems, therefore, that there must be some subsidiary complex present in *any* judgement, and this is just what Russell's theory denies.

It was therefore with great relief that I found that Wittgenstein rejects Russell's theory, for a variety of reasons of which the most convincing seems to be that Russell's theory leaves it entirely unexplained why it should be impossible to judge a *nonsense*. It would seem, on Russell's theory, that if you can judge that Edward is the father of George, you should be equally capable of judging that Edward is the father of blue.

Wittgenstein's own theory, if I understand it correctly, is something like this. We begin with reality, the facts. Of these facts we construct pictures, the factual propositions. A factual proposition consists of objects, words, noises, chairs or tables, arranged in a certain *form*. This form is *the same* form as that of reality; it is only because the picture and the facts have the same form that they can be compared with one another. If the fact is that George is the father of Edward, then the picture 'Edward is the father of George' has the same form, and it is just because of this that we can say that the picture is a bad one, the proposition is false. 'The picture can represent every reality whose form it has. . . . The picture, however, cannot represent its form of representation; it shows it forth. . . . The picture has the logical form of representation in common with what it pictures. . . . It agrees with reality or not; it is right or wrong, true or false. . . .'

There is, however, something beside the picture or factual proposition, namely the proposition in the sense which is relevant to logic. What is relevant to logic is not the factual proposition but what is common to all the factual propositions that can be pictures of a given state of affairs. A proposition is thus, in some sense, a *form*. The propositions of Hilbert's logic are also forms, but Wittgenstein's forms are more substantial than Hilbert's, since they contain what Russell and Wittgenstein call the 'logical constants', 'and', 'or', 'not' and so forth, whereas Hilbert's can hardly be said to 'contain' anything at all. These logical constants do not represent and are not represented, but are present in the proposition (that is to say the factual proposition) as in the fact. The proposition (that is to say here the logical proposition) is thus a form of logical constants, whereas Hilbert's propositions are so to say *pure* form.

I ask then, finally, whether there is anything in the proposition, as relevant to logic and as Wittgenstein seems to conceive it, which affords any justification for my belief in 'real' propositions, my invincible feeling that, if Littlewood and I both believe Goldbach's theorem, then there is something, and that the same something, in which we both believe, and that that same something will remain the same something when each of us is dead and when succeeding generations of more skilful mathematicians have proved our belief to be right or wrong. I hoped to find support for such a view, when I read that 'the essential in a proposition is that which is common to all propositions which can express the same sense' and that 'the proposition is the propositional sign in its projective relation to the world'. When I read further, both in the book itself and in what

Russell says about it, I concluded that I had been deceived. I can find nothing, in Wittgenstein's theory, that is common to all the ways in which I can say that something is true and is not common also to many of the ways in which I can say that it is false. So here I can find no support for my belief; and if not here, where am I likely to find it? Yet my last remark must be that I am still convinced that it is true.

POSTSCRIPT.

I have left this lecture as it was delivered, but I should like to add two remarks.

(1) My quotation from Mr. Ramsey at the beginning of § 8 may lead to a misinterpretation of his general view of formalism. I understand from what Mr. Ramsey has written later, and from conversation with him, that his attitude towards Hilbert's logic is, up to a point at any rate, somewhat like my own, that is to say that he accepts the logic without accepting its philosophical foundation. In saying this, of course, I must not be interpreted as claiming Mr. Ramsey's approval for anything in particular that I say in the lecture.

(2) Prof. J. W. Alexander of Princeton has made the following remark to me concerning Hilbert's 'ideal theorems'. The fact that a great part of a formalism has been suggested by 'significant' concepts and propositions does not show that *all* its theorems must be capable of interpretation; there will generally be formulæ to which 'no meaning' can be attributed, and the study of these 'meaningless' formulæ may well advance our understanding of the relations of those which can be interpreted. Indeed (as v. Neumann has pointed out) the formalism *must* contain formulæ of this kind, since (*e.g.*) we can substitute a 'numerical' symbol inside a 'logical' formula, 2 for a and b in '$a \rightarrow (b \rightarrow a)$': no one has suggested any 'meaning' for '$2 \rightarrow (2 \rightarrow 2)$'.

It is natural to interpret Hilbert as meaning that his 'ideal theorems' are all of this kind; and that his logic does contain theorems 'ideal' in this sense is obvious after what I have just said. It is one thing to admit this, and another to admit that a particular proposition such as 'there are infinitely many primes' is 'ideal'. If I cannot admit that 'there are infinitely many primes' has no 'meaning', it is simply because it seems evident to me what the 'meaning' is.