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IS MATHEMATICAL CERTAINTY ABSOLUTE?

THE place where most people would look for absolute certainty is in pure mathematics or logic. Indeed, "mathematical certainty" has become a byword. Now, just as Aristides was ostracized because people were tired of hearing him called "The Just" so much, so we become somewhat suspicious of the absolute certainty of mathematics through hearing it continually dwelt upon. Is, then, mathematics absolutely certain? To answer this question we must first consider a few points concerning the nature of pure mathematics.

Pure mathematics (or logic, which is merely the same discipline under another name) is defined by Mr. Russell as "the science whose propositions contain no constants." That is, all the "things" about which logic and mathematics seem to assert specific propositions—the truth-values, universes of discourse, classes, syllogisms, etc., with which logic deals, and the numbers, integral, fractional, real, and complex, which form the subject-matter of arithmetic and algebra, the points, lines, and planes of geometry, and the functions, definite integrals, etc., of analysis—are mere constructions, made to help us express and explain what certain sorts of propositions have in common, and not at all things of the real world. According to Professor Frege and Mr. Russell, a proposition such as "two plus two equals four" does not really involve such objects as two or four might be supposed to be, but merely asserts that if one considers a property that belongs to a thing *a*, and another thing *b*, distinct from *a*, and to nothing else, and another property that belongs to a thing *c*, and another thing *d*, distinct from *c*, and to nothing else, then the property consisting of the disjunction of these two properties is possessed by objects which we may term *m*, *n*, *o*, and *p*, which are all distinct from one another, and if it be possessed by an object *x*, then *x* is either *m*, *n*, *o*, or *p*.

But even if the things with which mathematics deals are fictions, it must be admitted that we can handle these fictions without knowing how they are put together. The average mathematician neither knows, nor, I grieve to say, cares, what a number is. You may say if you like that his analysis is blunted and his work rendered unrigorous by this deficiency, but the fact remains that not only can he attain to a very great degree of comprehension of his subject, but he can make advances in it, and discover mathematical laws previously unknown. The whole logical analysis of the concept of number scarcely dates back forty years, yet the first mathematical use of numbers is lost in prehistoric antiquity.

Now, if mathematics is essentially the science of propositions involving no constants, how is it that there were mathematicians before Frege? How is it that mankind was able to handle the notion of number for myriads of years with hardly the ghost of an idea of what a number was? It is almost infinitely improbable, as one sees at once from the illustration given above, that we have Frege's notion of number before we study Frege's work, for it is so unfamiliar to us when we first learn it, and it can not be argued that since Frege's numbers have the formal properties of our every-day numbers they are identical with them, for a little reflection will convince us that on the basis of Frege's and Mr. Russell's own work, we can produce other constructions different from those to which they give the name of number, yet having formal properties which, as far as we are interested in them from the standpoint of a definition of number, are identical with those of Frege's numbers, and that it will be, in general, impossible to say that one of these constructions more truly represents the proper analysis of our naïve notion of number than another, for all of them will seem almost equally unfamiliar to us when we first become acquainted with them. With regard to the ordinary integers with which elementary arithmetic deals, for example, it is even impossible to say whether, in the strict mathematical sense of the word, they are ordinal or cardinal numbers—that is, whether or not they imply an arrangement of the collections of objects to which they refer.

We can not, then, regard naïve mathematics, whether it be the naïve mathematics of a schoolboy or of a Leibniz, as merely a less explicit statement of what the modern analyst expresses with the aid of his involved technique and symbolism: whence, then, does it draw what certainty it possesses? Perhaps I can explain this best by reminding the reader of an experience which very many people must have had while they were learning mathematics. Every one, or almost every one, at any rate, must remember what agony his first lessons in geometry gave him when he was a schoolboy. The theorems seemed obvious enough to him, but how on earth, he probably wondered, can one get the theorems out of the axioms? No doubt, he thought to himself, two straight angles are always equal, but how is it that one is justified in proving it by superposing one on the other? The axioms did not tell him just when he could superpose one figure on another and when he could not. On the other hand, if he were to go by common sense, and not by his axioms, in proving his theorems, how did it happen, he must have puzzled, that he was not allowed to make use of such eminently sensible methods of proof as measuring the lengths of the lines occurring in his figures, determining the perimeter of a circle by rolling it along a straight line, etc.? After several months' practise in geometry, however, although he was still unable to give a formulation of the principles by which he worked which would satisfy the demand for rigor of the modern student of the axioms of geometry, he ceased to ask these questions, yet seldom went wrong in his geometrical reasonings. He used proofs involving superposition where, and only where, they led to valid results, and never tried to solve a problem by measuring his lines and angles, or by rolling a circle along a line. In short, although he was by no means able to analyze his geometrical proofs in detail, he had formed *habits* of handling the ideas of geometry which, as the time went on, became less and less likely to lead him astray. It was in the uniformity of these habits that all the certainty of his geometrical demonstrations lay—at any rate, until he had begun to correlate his geometry with arithmetic or logic—and the postulates and axioms of geometry served merely to help him fix these habits and render them uniform. As it was by no means impossible that these habits should have broken down in some particular instance—though, after he had studied geometry for years, it was extremely unlikely—the certainty of his geometrical demonstrations was not absolute.

Now, it is not merely in the schoolboy's study of geometry that habit plays a large part: the life of every branch of mathematics lies in a habit. Let us suppose the schoolboy of the previous example replaced by a practised mathematician, and the garbled collection of so-called "axioms" which form the introduction to most school geometries replaced by a genuine set of postulates, made as rigorous

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as any yet devised. How is the mathematician ever to apply his postulates to one another? His postulates themselves can not tell him how they should be applied, for then he would have to make a proposition form a part of its own subject-matter, and he would be involved in vicious circle paradoxes. He can not solve the problem by merely adjoining new postulates to his set, telling us how to use the old ones, for either he has still the problem before him, how is he to use these new postulates, or he has an infinite regress of postulates, each depending for the rule by which it is to be applied on the preceding one. The only alternative which seems to me really open is that he should apply his postulates to one another in some way, the uniformity of which is secured by the fact that he has got the habit of handling certain sorts of combinations of symbols and of ideas in a certain manner. He feels instinctively, as it were, that here one can substitute this term for that, there one can leave off that parenthesis, etc. And this habit of using his symbols and compounding his ideas in such a way as to produce the results which other mathematicians have produced, and of obtaining new propositions in a certain determinate manner, is so ingrained in him and so uniform that the chances of his being led to deduce different and conflicting theorems from the same premises are very nearly *nil.*

Yet that these chances are not necessarily entirely absent is best shown by the fact that in many cases, where mathematicians had uniformly deduced certain conclusions from certain premises for, perhaps, centuries, great mathematicians have been able to change deliberately the habits with which they drew conclusions from these premises, and to deduce an absolutely different set of consequences from the original postulates, conflicting with the former conclusions, by bringing to expression as an additional postulate part of what was latent in the original habit, and contradicting it. This is the way the non-Euclidean geometries were first discovered, and the way that, after them, a whole family of systems such as finite spaces, non-Archimedean geometries, etc., have been constructed. This is the way negative numbers, fractions, irrational numbers, and complex numbers were first introduced. Now, although it perhaps never happened before the recognition of the axiom of parallels that a mathematician ever introduced a proposition only true in non-Euclidean geometry in a chain of reasonings about Euclidean geometry, it is by no means certain on *a priori* grounds that such a slip could not have occurred. Therefore, the demonstrations in geometry before the days of non-Euclidean geometry were only relatively certain.

Some of the mathematicians among my readers will object, in all probability, that our habits of geometrical reasoning are now absolutely determined, because the sets of postulates recently set up for geometry are what is called *perfect* or *categorical*: that is, that any new postulate, involving no non-geometrical notions, adjoined to the set, would either be a consequence of the other propositions of the set, or would contradict them. This is perfectly true, as far as it goes, but to understand its implications we must ask, how does one prove it? and what does it mean? Now, the simplest of the modern ways of defining a system as geometrical is by expressing all the notions involved by it in terms of some fundamental relation, and stating certain limiting propositions about this. These defining propositions are of such a sort as to hold of all relations which are what is called *similar* to any given relation about which they hold. A set of defining propositions, or postulates is then perfect, if no proposition which will apply to any relation similar to *R* if it applies to *R*, and which will still further limit the class of relations to which the set applies, can be asserted.

It will be seen, then, that to prove the perfectness of a set of postulates of, say, geometry, we already need a theory of relations, which will, among other things, explain the notion of similarity, and that the certainty of the perfectness of the set, on which depends our knowledge that our way of compounding the postulates of the set needs no habit to make it unambiguous, is itself dependent on the certainty of the formal calculus of relations. Moreover, if one deduces the theorems of the relational calculus directly or indirectly from certain premises, one can not claim, without aruging in a vicious circle, that he can prove that these premises form a perfect set, and that therefore our habits of using them can not be ambiguous.

Yet the theory of relations, like every mathematical theory, must be grounded either in postulates or in some other mathematical theory. The best foundation which has yet been given for it is that expounded in the "Principia Mathematica" of Dr. Whitehead and Mr. Russell. In this work, the theory of relations is deduced indirectly from certain postulates about propositions and "propositional functions" or concepts. The first postulate stated by Mr. Russell is very interesting in this connection: it says, "Any proposition implied by a true proposition is itself true." Unlike most of its successors, this is stated in words, and not in symbols. This fact is not without importance. Mr. Russell intends to use this proposition to justify himself in leaving off a true hypothesis from an implication. Now, if the proposition justifying this appeared in a tangible form as a premise in such a case, we should need to assume it a second time to justify its elimination in its first occurrence, and so on *in infinitum*. We should never, that is, be able to make a single deduction, for we could never separate a conclusion from its premises. We must be able to drop true premises in a definite manner, and this first postulate of Mr. Russell's is expressed in words, and not in symbols in recognition of the fact that, while this is the case, our power of doing so resides, not in the formulæ of logic themselves, but in our habit of using them. Now it is not only possible, but highly probable, that there are habits in accordance with which we might deduce different results from Mr. Russell's postulates, and possible, but almost infinitely improbable, that we might at any time mistake one of these habits for the proper one. It seems also possible to me that this chance of uncertainty might be reduced to any desired degree by the insertion of new postulates in Mr. Russell's system defining the mode of application of the previous ones. The negation of these would lead to non-Russellian logics much as the negation of the postulate of parallels leads one to non-Euclidean geometries. It appears to me unlikely that such an amplification of Mr. Russell's set of postulates would ever render it possible for us to prove that no further ambiguities in the habits according to which we use these postulates would be possible.

Apparently, then, it is in any case highly probable that we can get no certainty that is absolute in the propositions of logic and mathematics, at any rate in those that derive their vadility from the postulates of logic. But are not the postulates themselves absolutely certain? Is there any conceivable room for uncertainty in the law of contradiction, or in the other axioms of logic? It appears to me that even here dogmatism is not the proper position to maintain. It seems a just maxim that we can not be absolutely sure that a proposition is true until we have a perfectly adequate knowledge of what it says—such a statement as, "Abracadabra, and I am sure of it" remains pure nonsense until one knows definitely what is meant by "Abracadabra," while even when we come to the relatively definite propositions of physics, such as the law of the conservation of energy, one of the chief sources of doubt as to their absolute validity is, in many cases, our lack of certainty as to what they really assert. Now, such "laws of thought" as the law of contradiction, or the law of identity, have already undergone a considerable change in their meaning on account of the analysis to which the new mathematical logic has subjected them—the law of contradiction, "Everything is either *A* or *not-A*," has been rendered a rather late inference in the "Principia Mathematica," limited in its meaning by the theory of types, and not derivable from any single one of the set of postulates there given. The law of identity has been shown to be a consequence of the definition of identity, which requires an elaborate logic for its very formulation. Even if one accepts "*p* is true or false" as the same proposition as the law of contradiction and "*p* is equivalent to *p*" as the law of identity, these may come in at a stage when the theory of propositions has already reached a high level of development, if we accept Sheffer's analysis of the calculus of propositions, and it is by no means inconceivable that this should make a certain difference in their complete meanings. Moreover, it is not impossible that the notion of a "proposition," in the sense in which this word is used in the "Principia," may itself be capable of analysis in terms of some more simple notion—it is part of mathematical and logical progress not only that our sets of postulates should be rendered more precise by the adjunction of new postulates, but that the "habit" by which we use a set of postulates pertaining to a certain mathematical or logical system we use should be made more unambiguous by the reference of the system as a whole to a finer system, which gives us a smaller opportunity for ambiguity in the habit by which we use its postulates, as a center of orientation, as it were. There is no need, then, of supposing that even the axioms of the "Principia" or any similar set we shall ever come to are not subject to further analysis, and that we have an absolutely adequate knowledge of the meaning of any logical proposition whatever. Hence, although our degree of uncertainty in logic is so infinitesimal as not to enter at all in the allowance we make for error in our scientific reasonings, we have no reason to suppose it is altogether absent.