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TENDENCIES IN THE LOGIC OF MATHEMATICS[1]

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1. INTRODUCTION

I APPROACH this subject which I have announced for to-day, "Tendencies in the Logic of Mathematics," with some trepidation, with many misgivings, and, I trust, with due humility. To many, the logic of mathematics is old, fixed, immutable. To shake faith in it, even to express a doubt concerning it, is to these nothing short of heresy. As Professor C. I. Lewis, of Harvard University, says in the October issue of *The Monist:* "From Aristotle down, the laws of logic have been regarded as fixed and archetypal; and as such that they admit of no conceivable alternatives. Often they have been attributed to the structure of the Universe or to the nature of human reason; and in general they have been regarded as providing an Archimedean fixed point in the realm of thought."

Yet long ago, as I shall indicate, doubts appeared; and within my own lifetime definite and undeniable difficulties have arisen which leave no room for a complacent acceptance of the logic of the ancients in unchanged form. I shall attempt to-day to state some of the difficulties and some of the proposed remedies. We shall see that there is to-day, if not universal agreement on the details of new systems, at least essential agreement that fundamental changes are necessary. As Lewis remarks in the article quoted: "There are no laws of logic which can be attributed to the universe or to human reason in the traditional form." Acceptance of such doctrine, however, like acceptance of other revolutionary changes in the history of human thought, of which the most recent is the theory of relativity, comes slowly; always men's minds revert to older ways of thought, always the interpretations placed upon the newer statements may be mistaken, always there is need for slow and detailed statement

[1] Address of the retiring chairman and vice-president of Section A—Mathematics, American Association for the Advancement of Science, Atlantic City, December 28, 1932.

of the essential grounds on which the need for changes rests.

If, then, I seem to some of you present, who know full well all—or well-nigh all—that I shall say, to go too slowly over the familiar grounds, I beg you to bear with me, since there is real need for patient and repeated statement of these things. I address myself to-day essentially to those to whom the reasons for change are not thoroughly known. To them, and to others who may later read these lines, I wish to make clear in not too lengthy form the larger outlines, with some details.

I am saved the necessity of a long introduction, recounting the slow growth of mathematical rigor, by the fact of easy access by all of you to two reports essentially on the same field: the Gibbs Lecture, by Professor James Pierpont, presented at the Nashville meeting of these organizations in 1927, and the invited address by Professor Arnold Dresden, presented at the same meeting, both of which were printed in Volume 34 (1928) of the *Bulletin of the American Mathematical Society.*

Pierpont gave a general résumé, going back at least to the beginnings of the Calculus, and coming down the centuries from Newton and Leibnitz through Euler and Lagrange and Cauchy to the days of Weierstrass and Kronecker and Cantor and Poincaré, and thence to our own generation and the present decade, with sketches of the three chief movements of this period, which are associated in men's minds most vividly with the names of Whitehead and Russell, Hilbert, and Brouwer, respectively. These remain the three chief directions of research in this field, and it is to them that I shall give particular attention in what follows. By name, these three schools of thought, or methods of procedure, are most often called the logistic, the postulational or formalistic, and the intuitional schools or methods, respectively. While my own sympathies will doubtless appear, I will attempt to give an impartial account of the progress in these fields, in grand outlines, and their present states.

Dresden, in his address, states what appear to him to be the underlying philosophical aspects. Both here, and in a former report published in Volume 30 (1924) of the *Bulletin,* he gives special attention to the work of Brouwer and to its underlying philosophy, but he does so with great impartiality, and his views on the doctrines which underlie mathematics and mathematical logic are still worthy of careful study. These two reports by Dresden were preceded by a translation by him of an article by Brouwer, published in the *Bulletin* in 1913. The two papers in 1913 and 1924 deal primarily with Brouwer's work; with the present article, they span two decades, and it may be of interest to observe by a comparison with them the slow but certain progress of the principal ideas during these two decades.

2. Introduction of Infinities

In a large sense, though with certain notable exceptions, such as the controversies over the parallel postulate, the origin of logical difficulties appears to lie in the introduction of infinities into mathematics. Even in the case of the parallel postulate, the rôle of infinity is clearly apparent, and might be held to be the paramount difficulty. With the advent of the Calculus, at any rate, with its troublesome concepts of infinitesimal and of limit, the effect of infinities upon mathematics and mathematical reasoning becomes striking and undebatable. It is with these difficulties that Pierpont begins in his address.

That essentially the same difficulties had arisen in the old Greek paradoxes is now clear to us; but such paradoxes were regarded mainly as philosophical playthings, and the mathematical controversies in the same field awaited the important development of the Calculus. At one other point, however, an objection had been raised before that time; and it is so important for our more recent studies that I wish to emphasize it, particularly because it is not mentioned in the addresses quoted above. Isaac Barrow, who was one of Newton's teachers, and one to whom Newton owed much, had objected, before the Calculus controversy, to the development of the theory of proportion in the Fifth Book of Euclid. He pointed out that Euclid's argument, and in particular his definition of the sense of inequalities for what we should now call irrational numbers, requires, for a determinate answer, the use of an infinite number of steps. This objection, which Barrow stated in clear language, makes him the forerunner of such modern scholars as Kronecker, Poincaré, Brouwer, Weyl and E. T. Bell, all of whom have reiterated this objection. In it, the notion of infinity enters in mathematical reasoning again, in a very different, and in a very disturbing fashion.

The position of Kronecker is stated in some detail by Pierpont. In his *Festschrift,*[2] Kronecker attacked particularly the definition of reducibility of a polynomial, which is ordinarily stated by saying that a polynomial is reducible, or not, according as it has, or has not, a rational factor. Without means for decision, and faced with a possible infinite number of trials, Kronecker declares such a definition not only unusable, but meaningless. For the same reasons, he attacked bitterly the whole theory of irrational numbers, and the entire structure of the Weierstrass theory of functions. To this day, these objections

[2] *Zeitschr. für Math.,* Vol. 92. 1882.

stand. They have been voiced again and again, most strenuously, perhaps, by Brouwer, which may be a misfortune. To many other great mathematicians, however, including Poincaré and Weyl, objections of this character, involving both definitions and existence proofs, have seemed weighty and soberly ominous. It is indeed surprising that so little heed has been paid to the reasonable warning given time and again by men of the first rank, from Barrow to Poincaré; and it must be admitted that every such definition, every such existence proof, which does not contain in itself an explicit means for setting up the concept discussed in a finite number of steps, is open to question, and can not be said to be established with logical rigor.

That insistence upon such a point would destroy much of mathematical literature, as has been urged by such equally great figures as Weierstrass and Hilbert, seems to me to be as much beside the point as is that argument for immortality which depends upon our displeasure at mortality. If there be no better argument, we may as well begin at once some effort toward reconstructing those parts of mathematics that depend on such debatable procedures.

In one instance, this has already happened. In 1897, Burali-Forti[3] published his now famous paradox on transfinite numbers. As a result, that theory is now quite re-formed, and parts of it are no longer used, while some theorems, such as the theorem that every closed set consists of a countable set together with a perfect set, once proved by means of that theory, is now commonly proved otherwise. Possibly due to this very paradox, and surely due to some similar considerations, Poincaré showed a most interesting change of attitude—a volte-face, indeed—from the Paris Congress in 1900, where he said, "One may say to-day that absolute rigor has been attained," to the Rome Congress in 1908, where he said, "Later generations will regard the Mengenlehre as a disease from which one has recovered." His other writings from that time on betray his lack of faith in that rigor which he had called "final" in 1900.

Such objections are climaxed and most vociferously proclaimed by Brouwer, as I have stated above, but he is by no means alone.[4] These objections, however, should be differentiated from what is the central part of Brouwer's thesis, the so-called intuitionism. It is by no means necessary to become an adept of intuitionism in order to adopt the objections of Kronecker and Poincaré, which Brouwer also advocates. Nor is Brouwer's objection to the Law of the Excluded Middle an integral part of intuitionism. One may, and, as I shall point out, one now must, admit that the Law of the Excluded Middle is not a necessary part of logic, whether one is an intuitionist or not. All these objections are advocated strongly—perhaps too strongly—by Brouwer, and his adherents as well as he will justly claim them as an integral part of his whole thinking and of his works. This I will not deny. What I wish to emphasize is that neither the objection to an infinite number of steps in an argument or definition or existence theorem, nor the objection to the Law of the Excluded Middle as a fixed principle, is so tied to intuitionism that one must accept it in order to accept these others. I believe that the general impression that these ideas can not be accepted without acceptance of all that is associated with Brouwer's name is one of the reasons why the mathematical world has so long delayed acceptance of them, and action based upon them.

3. BROUWER'S POSITION. EXCLUDED MIDDLE

In what precedes, I have dwelt upon one objection voiced by Brouwer, but also by many great scholars before him. His objection to the Law of the Excluded Middle is, as I have remarked, also independent of the method or school called intuitionism. Leaving that method for later discussion, I wish to call attention now to his position, and the position of others, regarding the Law of the Excluded Middle. His own statements sometimes lack complete clarity, and even the expositions of Dresden mentioned above may not be wholly obvious. Certainly the attitude of Brouwer was not clear to many of his opponents. Thus Barzin and Errera obviously misunderstood him, as they themselves state in a later article.[5]

That Brouwer rejects the law is quite obvious. His reasons also seem not hard to find for one who wills, and they certainly appear in the examples which he cites.[6] One such example is to know whether there exists a number k which expresses the number of digits in the number π at which, for the first time, the sequence 0123456789 begins in the decimal representation of π. It is not provable at present that k exists, for such a sequence does not occur in the decimal representation of π as far as its expression has been calculated.

It is then obvious that Brouwer is thinking, as others are thinking, that it may not be possible to arrive at a demonstration of a proposition p, or of its denial p′, by a finite sequence of syllogisms, without the use of the law mentioned. If not, there may be a proposition not provable, whose denial (p′) is also not provable. This may even be the case if argu-

[3] *Rend. Circ. Mat. de Palermo*, 11: 154–164. 1897.

[4] See Brouwer (trans. by Dresden), *Bull. Amer. Math. Soc.*, 20: 81 *ff.* (1913); Brouwer, *Jahresbericht D. M. V.*, 28: 203; 1920); Brouwer, *Zeitschr. für Math.*, 154: 1 (1925); Pierpont, *Bull. Amer. Math. Soc.* 34: 23–53 (1928); Dresden, *Bull. Amer. Math. Soc.*, 30: 31–40 (1924) and 34: 438–452 (1928).

[5] *Acad. de Belgique, Classe des Sciences*, 13: 56–71 (1927); *Arch. Soc. Belge Philos.*, pp. 3–26. 1928–29.

[6] *Zeitschr. für Math.*, 154: 1. 1925.

ments that are equivalent to the use of an infinite number of steps are allowed. Such propositions need not be *absurd,* as some have claimed: the preceding example does not introduce any new concepts, and is within what is sometimes called the "universe of discourse"; the question involved can not be called absurd, or rejected; indeed, it is entirely thinkable that the question can be answered.

The immediate reply of many persons is that the proposition that k exists must either be true or else false, on the basis of "ultimate" truth. This is, indeed, precisely the Law of the Excluded Middle. If the topics discussed form a part of the concrete universe, thought of as externally real, such a position would be justified. In how far such a position is tenable will be discussed in the next section. The belief in a real external existence of the objects and relationships under discussion is undoubtedly the basis for the law, as it is, no doubt, the basis for all logic. Again, as Dresden has remarked[7] in his discussion of Brouwer's position, the law would be granted if the proposition dealt with a finite system. That Brouwer's own statements are not particularly clear on these matters will appear from the quotations which Dresden makes in the article just cited, and from Dresden's own comments.

4. Reality. The External World

As I have just stated, a crucial element in any such discussion is the meaning of the words "true" and "false," and the associated question of the conceived real existence of the concepts and relationships under discussion in the real external world. It may be as well to grant at once that the law hold in an *a priori* sense in such an *a priori* system.

On the other hand, so far as I know, no modern school of mathematical thought, least of all the postulational or formalistic school, would accept such a tenet. Beginning with the discussions of the parallel axiom of Euclid, the whole course of mathematical logic has tended strongly toward the abandonment of the claim that an axiom or a postulate necessarily represents the realities of the external universe. Thus every present school of mathematical thought, in so far as I know, would accept either Euclidean geometry or any one of the non-Euclidean geometries, with no claim that one of them more than any other represents the realities. Thus, while I have not yet discussed the different schools nor the differences that exist in their fundamental thinking, I may assume that no one of these schools would attempt to base its systems on a claim of reality. The nearest approach to such a claim would be the school associated with Brouwer himself, but since he freely accepts the different geometries mentioned above as equally valid, for example, it is clear that any claim of the intuitionists for the reality of their systems would be quite limited; to this I shall return.

If the claim for reality, the claim that the objects and the relationships under discussion form a part of an existent external world, disappears, then the *a priori* nature of "truth" and "falsity" of propositions also disappears, and with it disappears also the *a priori* assumption that any proposition is either true or else false, that is, the Law of the Excluded Middle. To reestablish it, we need at the least a clear agreement on the meaning of the words "true" and "false"; and we need new assurance of the validity of the law.

That a confusion of thought arises continually from the tendency of the human mind to revert to the primeval definitions or *a priori* conceptions of "true" and "false" appears very frequently in the literature. Thus, such a careful thinker as Church,[8] in his keen criticism of Barzin and Errera, says of the Brouwer position: "The latter point" (a statement essentially quoted from Brouwer) "depends, of course, on identifying the truth of a proposition with the possibility of proving the proposition. But it seems more in accord with our usual ideas to think of truth as a property independent of our ability to prove it. Consequently we prefer to take the truth of a proposition merely as an undefined term subject to certain postulates, among them, if we choose to include it, the law of the excluded middle." While I must admit at once that Church's position is entirely tenable and logically sound, and while it is only just to Church to admit that he says elsewhere that one may also choose to deny the law, his position seems to be to avoid trying to define "true" and "false" in order to save the Law of the Excluded Middle. One must admit the correctness (logically) of his statement, since what he proposes is precisely what is done in the system of Principia, as we shall see. To admit that such a system is a possible system of logic, however, is far from admitting that it is the only system of logic; and to assert that such a system (*e.g.,* Principia) is "more in accord with our usual ideas," that is, with the *a priori* conception of "true" and "false" in an external world of realities, seems strange to one to whom the rudiments of the Principia are known.

Still closer to the *a priori* conception of "truth" is the position taken by Barzin and Errera. In the second article just cited, they criticize the definition given by Levy, which we shall mention in a moment. They say (page 5): "M. Levy . . . defines a truth or falsity which he calls *brouwerian,"* and they speak of the "usual" sense of truth and falsity, without further comment.

[7] Dresden, *Bull. Amer. Math. Soc.,* 30: 39. 1924.

[8] Church, *Bull. Amer. Math. Soc.,* 34: 75–78. 1928.

The definition given by Levy[9] deserves attention. Although he calls it "brouwerian," he has stated also that it does not agree precisely with Brouwer's own view. Levy says that a proposition is "true" if it is possible to demonstrate it (obviously without the use of L. E. M.), and "false" if it is possible to prove its denial p′. I myself had used essentially these same definitions[10] in a paper presented to this society, and I have been told by Weyl, in conversation, that this is probably Brouwer's point of view, though I am not aware that he has stated it distinctly and clearly.

The effect of such a definition upon the law (L. E. M.) is clear enough. If a proposition p is not "true," does it follow that it is "false"? This now has a distinct meaning, at least. It means: if p can not be proved (in a finite number of steps and without L. E. M.) then p′ can be proved. Of this there is no present guarantee. Hence the law (L. E. M.) has no present validity. What further effects such a position may have upon (say) the mathematics of a postulational system, and in how far the validity of the law (L. E. M.) may be secured in such a system, will be discussed later.

As a result of the present discussion, it is sufficient to point out that a crucial element in any treatment of these matters is to determine what the author means by "truth," that is, whether he reverts to the *a priori* concept based on a claim of reality; or assumes, with Church, what is essentially the Principia position, that "truth" is an undefined property; or uses what I shall call, with Levy, the "brouwerian" definition of "truth"; or gives some other definition of the term. Clearly, the law (L. E. M.) has totally different significances in these various cases.

5. THE FORMALISTIC OR POSTULATIONAL METHOD

It is probable that the postulational or formalistic view-point is the one most familiar to my audience, but a precise statement of it is not easy to give. A clear statement is given by Bernstein,[11] and I will not repeat it. Many other authors have given clear descriptions. One given with a clear consciousness of the existence of Brouwer's ideas is that of Dresden,[12] which is nevertheless, in its essence, a description of the postulational view-point.

I wish to refer hurriedly to some of the fundamental ideas present in modern discussions of any postulational system, but I shall assume that you are familiar with them. It is usual to prove that the postulates stated are *independent,* that is, that no one can be deduced from the others; that they are *consistent,* that is, that they contain no hidden contradiction, which is done (ordinarily) by exhibiting a set of objects and operations or relations whose existence is supposed known and which satisfy all the postulates. It is desired also to show that the system of postulates is *complete,* that is, that no new postulate may be added to the set without the introduction of a new undefined concept; and that they are *categorical,* that is, that two systems that satisfy the postulates can be made to correspond isomorphically.

The present status of this school of thought is also known to you perhaps better than is that of any other school. Its results have been quite successful in many fields of mathematical thought, and the extent to which such examination of systems of postulates has been carried surely exceeds any estimate which might have been made, say, in 1900. One serious bar remains. In general, the proofs of consistency have been made on the assumption of the soundness of the postulates of arithmetic. These postulates themselves have been discussed extensively, and most strenuous efforts have been made to prove that they themselves are consistent. Hilbert and his followers have been most hopeful that final success would be achieved, and a partial success has been attained by Hilbert himself,[13] von Neumann[14] and others. The means employed by Hilbert and others, however, particularly the employment of arbitrary marks, seem of doubtful validity to many, and one must hesitate to announce that a final success has been attained.

The present status, then, remains that we must assume the validity of the postulates of arithmetic, in some form. We need, indeed, thus much of assumed *reality,* or at least assumed *consistency.* If it be an assumption of *reality,* then the L. E. M. would have a certain *a priori* validity based on that assumption; but even that would not of itself carry through to other sets of postulates based upon this one, since a proof of consistency by no means demonstrates reality.

As for "truth," we may assume, with Church, that it is an undefined property, and we may adjoin (see Bernstein, *loc. cit.*) some such set of postulates for logic as that of the Principia. To do so, however, is clearly an act of *choice,* not of necessity; and this Church clearly indicates.

On the other hand, it would appear more in keeping to call these things "true," just as we call the postulates themselves "true," not because of an instinctive belief in their ultimate reality in an external world, but simply because they follow from the postulates.

9 *Acad. de Belgique Classe des Sciences,* 13: 256–266. 1927.

10 *Bull. Amer. Math. Soc.,* 34: 436. 1928.

11 *Bull. Amer. Math. Soc.,* 37: 484. 1931.

12 *Bull. Amer. Math. Soc.,* 34: 442. 1928.

13 Hilbert, *Jahresbericht D. M. V.,* 8: 1900; *Math. Annalen,* 78: 405. 1918; *Abh. Hamburg Univ.,* 1: 157. 1922; *Math. Annalen,* 88: 151. 1922–23, and 95: 161. 1926. Also references given by Dresden, *Bull. Amer. Math. Soc.,* 34: 440. 1928.

14 v. Neumann, *Math. Zeitschrift,* 26: 1 (1927), and *Math. Annalen,* 154: 219. 1925.

This is indeed no other than the position that I myself have taken, that Levy took, and that I have called, with Levy, "brouwerian." It seems to me, however, not primarily brouwerian; rather, it seems to me very decidedly in accord with the very spirit of the postulational school. In that case, however, some support would have to be given to establish the validity of the L. E. M., since it ceases to be obviously true.

If we could prove the set of postulates have the property that any proposition p can be proved, or else its denial p′ can be proved, from the set of postulates, we should have turned the trick. If we can not do this, then of what avail is it to have adopted rules of logic which essentially beg the question? Such rules of logic *do* essentially beg the question, for, after all, what we want to know is precisely whether or not we can prove p or else p′. To assume that we can always do one or the other of these things, or, what amounts to the same thing, to say (in no matter how disguised a form) that p is to be called "true" not only when we can give a direct proof of it, but also whenever we can not prove p′, seems strangely out of accord with the spirit of the postulational method. It is, however, logically possible to do this.

A dilemma to which we are led if we take this position has been stated by Church in his discussion of the Zermelo postulate.[15] Here he himself is dealing with a possible *addition* to the laws of logic, and, since alternatives present themselves, it is obvious that we can not decide all questions until some such additional law has been assumed. Realizing the relation of this question to the L. E. M., Church states (p. 186) that the fact that we might be led, after assuming one or the other of two additional postulates, to different conclusions regarding the same proposition, he concludes that we should then have not a violation of the L. E. M. but rather two universes of thought, each self-consistent, proceeding forever side-by-side, perhaps after the manner of the two universes of thought contained in Euclidean geometry and one non-Euclidean geometry. It seems obvious, however, that *just before* the additional postulate is chosen, we should certainly be in possession of a consistent set of postulates for which the L. E. M. does not hold. Nor is it clear how one may know when this situation exists. For example, before the explicit statement of the Zermelo postulate, would it have been clear that the L. E. M. did *not* then apply to the system as it was? Let me say distinctly, however, that the remark of Church seems to me to be sound in so far as it goes.

6. The Intuitional School

I have discussed already two phases of the work of Brouwer, namely, his attitude toward the L. E. M. and his attitude toward proofs or definitions that are based effectively upon the use of an infinite number of steps. Neither of these has a specific relation, however, to the intuitional method, in the sense that one may adhere to either or both of these other principles without attaching himself thereby to the intuitional school; indeed, as I have pointed out, many mathematicians of note have done so, without participating thereby in the intuitional movement.

Brouwer has stated his position on intuitionalism as the proper basis for mathematical work in numerous articles cited above. Two quotations from him that are given by Dresden[16] seem to give in brief space something of his view. "This intuition," says Dresden, "upon which not only mathematical thinking, but all intellectual activity is held to be based, is found in the abstract substratum of all observation of change, 'a fusion of continuous and discrete, a possibility of conceiving simultaneously several units, connected by a *between* that can not be exhausted by the interpolation of new units.'" And again: "It [the fundamental intuitive concept of mathematics] manifests itself in the intuition of time, which makes possible 'repetition, as being object in time, and again object.'"

Without pretending that this represents the whole of the point of view of the intuitionists, I may say that one concept which seems to stand out clearly is the acceptance on the basis of intuition, of some essential properties of *time*. In particular, the indefinite divisibility of it, the occurrence of a time between any two other times, appears; and the idea of *unit* is emphasized. These are indeed fundamental properties of pure number, as it is conceived in arithmetic. The recognition of *time* as a primordial prototype is not at all novel: Hamilton defined *mathematics* as "the science of pure time," and many another has done his fundamental *intuitional* thinking (which we may grant all mathematicians *must* do in some way) in terms of *time*.

While I reiterate that this does not exhaust the views of Brouwer on this field of thought, it does appear to me that there is here a common ground which I have not seen emphasized, as between intuitional and postulational thinking: it is that in both, if I have not too much distorted the facts, the essential properties of *time* (that is, of *real number*), are at present assumed to be valid upon the basis of our intuition, for I have pointed out that the present state of the postulational school is essentially just this.

At at least one other point, however, the two schools of thought part company in a more decisive manner. Brouwer contends that logic is not a sure guide toward the building-up of mathematical thought, and that the existence of a variety of possible logical sys-

[15] *Trans. Amer. Math. Soc.*, 29: 178–208. 1927.

[16] *Bull. Amer. Math. Soc.*, 30: 32. 1924.

tems leads one to feel that results obtained by one such system may not be wholly convincing. This position has been clearly stated by Boutroux.[17] It is admirably demonstrated, in fact, by the results which have been obtained by the use of the Zermelo postulate, as contrasted with results obtainable with no such postulate, or with a different one. I think that Brouwer would hold that the intuition must be called upon to decide when such a situation arises. That few of us would be able to do this with any degree of confidence, is, perhaps, not a valid objection. Certainly the formalistic or postulational school would not agree to such a procedure, except, perhaps, in an extremely restricted and narrow form. To what extent the modern developments of logic tend to confirm Brouwer's views, we shall see presently.

I have referred in what precedes chiefly to the Zermelo postulate because it is in some senses the best known case of an addition to the preceding logical system. There are others, however, which are nearly if not quite as prominent in the minds of mathematicians. Among these, I may mention the proposed postulate of Russell[18] that "whatever involves all of a collection must not be one of the collection," and the proposed postulate of Hilbert[19] to sanction the use of complete induction in the transfinite case. These notable instances of additions to the logical system, and their possible alternatives illustrate in themselves the possible variety of logical systems of which Boutroux speaks.

While I make no claim to have exhausted the views of intuitionists, I have presented some which seem to have their support. It is to be noted that the advances in logical theory tend rather to support them than otherwise, but perhaps not in precisely the manner that was intended by their originators.

7. LOGISTICS. THE PRINCIPIA

The logistic school has attempted to introduce a symbolism to replace ordinary language, in order to avoid the errors traceable to the ambiguities of ordinary language, and to express mathematics essentially as a part of logic by means of this symbolism. A first comprehensive attempt was begun by Peano in the "Formulaire de Mathématique" (Vol. 1, 1895), which was extended and completed in four volumes during the years 1895–1903 by Peano and several other Italian mathematicians. A still more comprehensive and exhaustive treatment began with a paper by Russell in the *American Journal* in 1908, and led to the monumental work "Principia Mathematica," by Whitehead and Russell (2d ed., 1925–27), to which reference has been made above. It was thought by the authors and by many followers that the procedure followed was subject to no errors, and that the resulting system was indeed final. Many mathematicians, however, including most German mathematicians, remained skeptical, and it is true that the authors felt obliged to introduce fundamental changes in the second edition, largely due to the work of H. M. Sheffer. A brief statement of the essential purposes and methods of the "Principia" is given by Bernstein,[20] and I may refer any who are not familiar with its nature to that article In his review of "Principia,." Bernstein pointed out certain difficulties of interpretation, and he has insisted upon these difficulties in his later articles[20] A simple instance is that the symbol "p" is often read "p is true," in spite of a warning by the authors of "Principia" against doing so. There are also certain "informal" statements in the "Principia" which were intended to clarify the more formal ones; but it appears that there are real differences between the "formal" and the "informal" systems, so that it may be said that the "Principia" consists of two different systems conducted simultaneously.

The definition of *implication* is stated essentially in the form: "p implies q if it is true that either not-p is true or else q is true." This definition is entirely sound, but it departs fundamentally from the Aristotelian system. Thus a false proposition implies any proposition whatever; and a true proposition implies any other true proposition. For this reason, the authors of the "Principia" took the position that the usual concepts of *independence* and of *consistency* do not apply to their own system, since any (true) postulate must imply any other (true) postulate. Bernstein has shown that this is not the case, however, by reducing the "Principia" assumptions to the form of postulates for a Boolean Algebra, since the definition of implication stated above essentially divides all propositions into two classes, which may be identified with the symbols 1 and 0 of a Boolean Algebra, for which postulates paralleling those of the "Principia" are then stated. These postulates are subject to examination for independence and for consistency, so that the original "Principia" postulates are also. It also proves possible to state the entire system in terms that make it a strict mathematical science in the sense of the postulational method. Another comprehensive survey of the situation is in type and will be published in the January, 1933, issue of the Transactions of the American Mathematical Society by E. V. Huntington. His system differs from that of Bernstein chiefly in the introduction, not only of the class K of all propo-

[17] Boutroux, ''L'Idéal des Mathématiciens.'' 1908.

[18] *Amer. Jour. Math.* 30: 225 (1908); Whitehead and Russell, ''Principia Mathematica'' (2nd ed., 1925), p. 58.

[19] Hilbert, *Math. Annalen*, 88: 151. 1922–23.

[20] Bernstein, *Bull. Amer. Math. Soc.*, 32: 711–713 (1926); 37: 480–488 (1931); 38: 388–390 (1932); 38: 589–593 (1932).

sitions, but also of another class T of propositions, a subclass of K, in terms of which the postulates are stated. The independence and consistency of these postulates are examined with great care. It is shown that the resulting system is equivalent to the "informal" "Principia" system, but the inherent difficulty of deriving it from the "formal" "Principia" system again manifests itself. Quite informally, one may think of T as the subclass of *true* propositions.

In either Bernstein's or Huntington's systems, and therefore in the "informal" "Principia" system, the L. E. M. follows from the postulates, though this is not the case for the "formal" "Principia" system. However, no immediate conclusion as to the universal validity of that law can be said to follow, since it is here simply postulated. The only conclusion is that the "informal" "Principia" is a dichotomy; that is, it is a two-valued system. This does not show that every mathematical system must also be dichotomous; it shows only that the "informal" "Principia" system is not applicable to a mathematical system unless that system is shown to be a dichotomy. This is quite a different statement.

8. Other Logical Systems. Abandonment of L. E. M.

Despite the monumental nature of the "Principia," it follows from what precedes that we must again face the question as to whether or not the L. E. M. is of universal application. The recent work of Bernstein, Huntington and others have not altered the inherent nature of the "Principia"; they have simply refined it, and have transformed it into a mathematical system, freed from essential disagreements between "informal" and "formal" system.

Attempts have been made to demonstrate the law. I have referred to the attempt of Barzin and Errera. Many replies were made to this attempt, of which two that are noteworthy are the papers of Church and Levy, in which the difficulties of the attempted argument are clearly established. Indeed, if the L. E. M. could be proved from the remaining standard laws of classical logic, it would follow, of course, that there would be no necessity of stating it. In a sense, therefore, any such attempt was predestined to fail, if any statement of the law is needed.

In 1930, however, a very definite settlement of all such controversy was given by Lukasiewicz and Tarski,[21] who actually set up a trichotomy which satisfies every requirement of implication, and which possesses a strict table of implications, including a *middle,* which I may call "uncertainty," as well as the customary "true" and "false." An excellent account has been given by C. I. Lewis in *The Monist* for October, 1932. It appears from their work, and from some later papers, that it is perfectly possible to set up and to operate perfectly sound systems which have just as definite "implication" systems as that of the "Principia," which have any desired number of intermediate "truth-values." One such which has been worked out with care is a quadruple system whose "truth-values" I may call "true," "probable," "improbable" and "false." Indeed the resulting system bears at least as close a relation to the popular interpretation of these four concepts as does the "Principia" to the popular interpretations of "true" and "false."

We are then obliged to conclude, with Lewis, that the L. E. M. is not "writ in the heavens," that it is not a law of universal application in logic. Rather it "reflects our stubborn adherence to the simplest of all possible modes of division, and our predominant interest in concrete objects as opposed to abstract concepts." These words of Lewis should have strong import to mathematicians, for in mathematics we deal habitually with just such abstract concepts.

9. Examples. Theory of Probability

Lewis, in the paper cited above, and Lewis and Langford, in their "Symbolic Logic" use the phrases "certainly true," "certainly false," "doubtful" as names for the "truth-values" of a trichotomy. I have insisted above upon such situations as actually existent, useful and commonly employed for practical ends, with at least as close adherence to the theoretical systems as obtains in the dichotomy "true" and "false" in the logic of ordinary reasoning.

It would seem that any desired scheme of "truth-values," however numerous, would find again a parallel in human thinking. The quadruple system "true," "probable," "improbable," "false," mentioned above, is such a case; though again I must insist that the precise rules for implication would be followed no more than in the case of ordinary reasoning. To obtain a similar parallel to any number of "truth-values," it is only necessary to appeal more openly to the theory of probabilities, and to assign to definite degrees of probability from 0 to 1 various "truth-values," that is, various gradations of certainty. I am attempting only to point out that such systems are by no means bizarre systems (or at least no more bizarre than is the "Principia" system), for something much akin to them is actually present in our minds, and forms habitually a basis for a great part of our actual thinking.

Nor is the limiting case of all this at all bizarre. It is quite legitimate to set up a scale of probabilities, and we do commonly do so, in which each rational

[21] *Comptes Rendus Soc. de Varsovie,* 23, III: 51–77. 1930.

proper fraction expresses a degree of probability, with the extreme limits 0 and 1, which express impossibility and certainty. The parallel logical system with an infinite number of graduations in "truth-values" from "false" to "true" appears to be just as feasible. Finally, a continuous scale might be established in either the popular sense of the theory of probability, or in the exact "implication" sense of a strict logical system. That the theory of probabilities may be thought of, at least informally, as constituting in itself a scheme of logical thinking in which there are many gradations of "truth-values" may form a clue toward rational appreciation of logical systems in which the L. E. M. is violated. It is unnecessary, however, to pretend to precise correspondence in this case, any more than there is precise correspondence between the "Principia" system and ordinary reasoning, for in each case independent and satisfactory proof exists that the logical system in question is legitimate.

10. CONCLUSIONS

I have attempted to outline in its major details the development of the logic of mathematics in recent years. If it appear to any that the acceptance of such ideas will destroy mathematics or parts of it, I would point out that a certain amount of discarding of what has been is necessary in order to assure progress. If we are to deny new developments whenever they require relinquishment of the ideas of the past, we shall be serving not truth but only our vanity.

It is indeed true that many famous mathematicians have given up in despair. Cantor, Dedekind, Frege, all consciously accepted defeat, and bitterly. I have mentioned the protest in a former generation of Weierstrass; in our generation, of Hilbert; these great men cry out that great portions of mathematics will fall if such ideas prevail. True, but that is no reason why they must not prevail if they be correct.

Nor is despair the note of all scholars. Brouwer, Weyl, Poincaré, show no sign of pessimism. They, and many lesser men, feel only that we may go on to new conquests of truth. New minds will arise who will carry mathematical truth in the next generation not to destruction, but to yet greater heights, to new and higher ideals of rigor.

For young men, there is a challenge in all this. Not that we can presume that Rome can be rebuilt after the conflagration in a single day. For a long time we must proceed as we now seem to be: asserting that our results are valid if we postulate the L. E. M. along with our other postulates. While this is a fundamentally weak situation, in view of our inability to show that our postulates are categorical, it is at worst defensible in the present state of our knowledge. We must proceed slowly. A vast amount of work that should and must be done lies in exploring the whole of mathematical literature to determine which theorems in a given field require the use of the L. E. M. for their demonstration, in the form in which we now do demonstrate them. Next, we may find out how to give better proofs, in which that law is not assumed, at least in some cases. On the basis of any philosophy, such discoveries would be valuable. For a reconstruction of mathematics they are quite necessary.

Can definitions and existence theorems be revised in every field, at least in some cases, so that the concepts discussed can be actually arrived at in a finite number of steps?

Such reconstructions are in fact already in progress. Not only Brouwer, but many others who have seen the need for such investigation, irrespective of an ultimate acceptance of all that now appears to be true, have been working toward these very goals. One need by no means accept the intuitionist point of view to see the value of such reconstruction. If it be only partial in our time, we should not fear the ultimate future. Generations yet unborn will doubtless face still some of our doubts, some of our problems.

There is, no doubt, some sense of tragedy in this, as in every previous instance of advancing ideals of logical exactness. We too lay down our task, unfinished. I shall only paraphrase the sentiments of great mathematicians of the past who have seen their labors end incompleted, if I quote to you some of the lines of a poem of Kipling which I treasure deeply, "The Palace"; in it, the master builder, after years of proud effort, lays down his task at last, saying:

> When I was a King and a Mason,
> In the open noon of my pride,
> They sent me word from the Darkness,
> They whispered and called me aside;
> They said: "The *end* is forbidden."
> They said: "Thy use is fulfilled;
> "Thy Palace shall stand as that other's,
> "The Spoil of a King who shall build."
>
> I called my men from my trenches,
> My quarries, my wharves, and my shears,
> All I had wrought I abandoned
> To the Faith of the faithless years.
> Only I cut on each timber,—
> Only I carved on each stone:
> "After me cometh a builder;
> "Tell him I too have known."