MATHEMATICAL RIGOR, PAST AND PRESENT*

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1. *Introduction.* The Mengenlehre of Cantor, or the theory of aggregates (sets), has brought to light a number of paradoxes or antinomies which have profoundly disturbed the mathematical community for a quarter of a century. Mathematical reasoning which seemed quite sound has led to distressing contradictions. As long as one of these is unexplained in a final and conclusive manner there is no guarantee that other forms of reasoning now in good standing may not lead to other contradictions as yet unsuspected. For ages the reasoning employed in mathematics has been regarded as a model of logical perfection; mathematicians have prided themselves that their science is the one science so irrefutably established that never in its long history has it had to take a backward step.

No wonder then, that these paradoxes of Burali-Forti (1897), Russell, and others produced consternation in the camp of the mathematicians; no wonder that the foundations on which mathematics rest are being scrutinized as never before. Elaborate attempts are now in progress to give mathematics a foundation as secure as it was thought to have in the days of Euclid or of Weierstrass. Personally we do not believe that absolute rigor will ever be attained and if a time arrives when this is thought to be the case, it will be a sign that the race of mathematicians has declined. However, the aim of this paper is not to show this, but rather to pass in review some typical examples of what were regarded at the time as good mathematical demonstrations, somewhat as a

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historical pageant presents to our eyes famous persons in chronological sequence.

2. *The English School of the Eighteenth Century.* Let us start with the eighteenth century, a century in which the efforts of mathematicians were largely spent in perfecting the calculus and in applying it to geometry, astronomy, and the natural sciences. The founders, Newton and Leibnitz, have left no clear account of its principles, and those who took up the new science promptly became embroiled in labyrinthine disputes. We who regard the infinitesimal calculus as nothing more than a calculus of limits can for the most part read into the obscure and scanty statements of Newton and Leibnitz what they perhaps wished to say. But their contemporaries and immediate followers had no clear idea of limits as we have, moreover their difficulties were increased by the fact that neither Newton* nor Leibnitz is quite consistent with himself.

Thus two schools arose; the English school, following Newton rested their reasoning on "the motion of bodies accelerated according to various hypotheses" and on "prime and ultimate ratios"; the continental school, following Leibnitz, rested their reasoning on infinitely small, non-archimedian quantities or "little zeros." Let us exhibit a few examples from the two schools; we shall take first the English.

A highly esteemed work was the *Treatise on Fluxions*, by Thomas Simpson (1st edition, 1737; 2d edition, 1776). As is well known, the proof that Newton gave in his *Principia* that $(uv)' = uv' + vu'$ is not satisfactory. This apparently was recognized by Simpson, who gives an alternate proof resting on the derivative of $y = x^2$.

On page 1 we read: "All kinds of magnitudes are to be considered as generated by the continual motion of some of their bounds or extremes; as a line by the motion of a point;

* As for Newton, see an illuminating article by A. De Morgan, Philosophical Magazine, (4), vol. 4 (1852), p. 321.

a surface by the motion of a line; and a solid by the motion of a surface. Every quantity so generated is called a variable or flowing quantity; and the magnitude by which any flowing quantity would be uniformly increased in a given portion of time with the generating celerity at any proposed position or instant (was it from thence to continue invariable) is the fluxion of the said quantity at that position or instant."

Thus in our language the fluxion of a variable u at the time t is $(du/dt)\Delta t$. Simpson represents this by $\dot{u}$. Let us see now how Simpson gets the fluxion of $y=x^2$. On a straight, let the reader mark points A, r, R, B in order, and on another straight the points C, s, e, S, D. Simpson reasons textually as follows: "Conceive two points m, n to proceed at the same time from two points A, C, along the right lines AB and CD in such sort that $CS=y$ is always equal to the square of $AR=x$, which latter moves uniformly. Furthermore let r, s, R, S, be any contemporary positions of the generating points. If $rR=v$ we have $CS=y=x^2$, $Cs=(x-v)^2=x^2-2xv+v^2$ and hence $Ss=CS-Cs=2xv-v^2$. From whence we gather that while the point m moves over the distance v, the point n moves over the distance $2xv-v^2$. But this last distance (since the square of any quantity is known to increase faster in proportion than the root) is not described with an uniform motion (like the former) but with an accelerated one. It therefore is equal to, and may be taken to express, the uniform space that might be described with the mean celerity at some intermediate point e in the same time. Therefore, seeing the distances that might be described in equal times, with the uniform celerity of m and the mean celerity at e are as v to $2xv-v^2$, or as $\dot{x}$ to $2x\dot{x}-v\dot{x}$, it is evident that in the same time the point m would move uniformly over the distance $\dot{x}$ the other point n with its celerity at e would move uniformly over the distance $2x\dot{x}-v\dot{x}$. This being the case let r, R and s, S be now supposed to coincide, by the arrival of the generating points at R and S, then e (being always between s and S) will likewise coincide with S; and the distance $2x\dot{x}-v\dot{x}$ which might be uniformly described in the aforesaid time with the velocity at e (now at S) will become barely equal $2x\dot{x}$ which (by the definition) is equal to $\dot{y}$, the true fluxion of Cn or x^2."

To avoid the difficulty in Newton's proof, Simpson has introduced the point e, which merely replaces one stumbling block by another. To realize the great difference between this English school and the modern doctrine of limits, one has only to compare this bungling proof with the simple little proof in any calculus of today.

3. *The Continental School of the Eighteenth Century.* We now turn to the continental school founded by Leibnitz. Perhaps the first systematic presentation of Leibnitz's methods to be published was the *Analyse des Infiniment Petits*, by the Marquis de l'Hospital (1696), which enjoyed the most widespread popularity. An English translation *The Method of Fluxions*, by E. Stone, appeared in 1730. De l'Hospital's book was largely founded on a little treatise by John Bernoulli, *Die Differentialrechnung*,* written in 1691–92, but only published in 1922 on the occasion of the tercentenary celebration of the Bernoulli family in Basel.

The treatise begins on page 11 with three postulates, of which the first reads: "A quantity which is diminished or increased by an infinitely small quantity is neither increased nor decreased." On page 12, we find: "The differential of x^2 is $2x\ dx$, which is proved thus: $(x+e)^2=x^2+2ex+e^2$, subtracting x^2 gives $2ex+e^2$ as remainder, and this on account of postulate 1 is $2ex=2xdx$.

"The differential of x/y is $(y\,dx-x\,dy)/y^2$. For if we subtract x/y from $(x+e)/(y+f)$ we get $(ey-fx)/(y^2+fy)=$ by postulate 1, $(ey-fx)/y^2=(ydx-xdy)/y^2$."

The next writer of this school whom we wish to consider is the immortal Euler, one of the greatest figures in the whole history of mathematics. Let us look at his *Institutiones*

* We quote from *Die Differentialrechnung von Johann Bernoulli*, Ostwald's Klassiker, Nr. 211.

Calculi Differentialis (1775). In the preface he considers the differential of $y=x^2$. He gives x the increment ω, the corresponding increment of y is $2x\omega+\omega^2=\eta$; the ratio η to ω is $2x+\omega$ to 1. Hence this ratio approaches $2x$ the smaller ω is taken. He is thus led to define the differential calculus as the method of determining the ratio of evanescent increments. These evanescent quantities are called differentials "que, cum quantitate destituantur, infinite parva quoque dicuntur, quae igitur sua natura ita sunt interpretanda, ut omnino nulla seu nihilo aequalia reputentur." He admonishes the reader to bear in mind that these differentials are absolutely zero and that nothing can be inferred from them other than their mutual ratio, which is in the end reduced to a finite quantity (verum perpetuo tenendum est, cum haec differentialia absoluta sint nihila, ex iis nihil aliud concludi nisi eorum rationes mutual, quae utique ad quantitates finitas deducuntur).

Thus Euler accepted unqualifiedly the notion that there exist quantities which are absolutely zero, yet whose ratios are finite numbers. The reader who wishes further information regarding Euler's views may consult Chapter III of the above work, entitled *De infinitis atque infinite parvis.* He encourages the reader here by remarking that this notion does not hide so great a mystery as is commonly thought, and which in the mind of many renders the calculus suspect. Any doubts which may have arisen will be shown devoid of foundation as the theory is developed. This reminds one of d'Alembert's statement: "Allez en avant, la foi vous viendra."

Euler's *Differential and Integral Calculus* and his *Introduction to Infinitesimal Analysis* were the standard textbooks of the day; they were in everybody's hands. The fame of the author and their great popularity renders it necessary to give another example of his style of reasoning.

How does he find the differential of $y=\log x$? This is treated in §180 of the *Institutiones*. Replacing x by $x+dx$ gives

$$dy = \log(x + dx) - \log x = \log(1 + dx/x).$$

Now in Chapter VII of volume I of the *Introduction to Analysis* he has found

$$(1)\qquad \log(1+z) = z - z^2/2 + z^3/3 - z^4/4 + \cdots .$$

Replacing here z by dx/x gives

$$dy = dx/x - dx^2/(2x^2) + dx^3/(3x^3) - \cdots .$$

As all the terms of this series beyond the first are evanescent we have

$$d \cdot \log x = dx/x.$$

We turn thus to the *Introductio in Analysin Infinitorum* (1748) to learn how the series (1) is established. We find (§115, seq.) the demonstration rests on Newton's celebrated binomial formula

$$(2)\qquad (1+u)^m = 1 + mu + \frac{m(m-1)}{1 \cdot 2}u^2 + \cdots .$$

Euler gives no proof of this, which in those days was proved in algebra.* Let us see, however, how he uses (2) to prove (1). Euler starts with the relation $a^\omega = 1 + k\omega$, ω infinitely small; then a being taken as base, $\omega = \log(1+k\omega)$. Hence

$$a^{i\omega} = (1+k\omega)^i = 1 + \frac{i}{1} \cdot k\omega + \frac{i(i-1)}{1 \cdot 2}k^2\omega^2 + \cdots .$$

Set $i = z/\omega$, z finite; then $\omega i = z$, and

$$a^z = 1 + kz + \frac{(i-1)}{2i}k^2z^2 + \frac{(i-1)(i-2)}{2i \cdot 3i}k^3z^3 + \cdots .$$

As i is a "number larger than any assignable quantity"

$$\frac{i-1}{2i} = \frac{1}{2}, \quad \frac{(i-1)(i-2)}{2i \cdot 3i} = \frac{1}{2 \cdot 3}, \text{ etc.},$$

hence

* Judged by modern standards, these demonstrations are quite worthless.

$$a^z = 1 + kz + k^2z^2/2! + k^3z^3/3! + \cdots . \tag{3}$$

The larger i is taken, the nearer $(1+k\omega)^i$ is to 1. Euler thus sets

$$(1 + k\omega)^i = 1 + x, \quad \therefore k\omega = (1 + x)^{1/i} - 1,$$
$$i\omega = i[(1 + x)^{1/i} - 1]/k.$$

Hence

$$\log (1 + x) = i(1 + x)^{1/i}/k - i/k.$$

Now

$$(1 + x)^{1/i} = 1 + \frac{x}{i} - \frac{(i - 1)}{2i^2}x^2 + \cdots ;$$

hence if i is infinitely large

$$i(1 + x)^{1/i} = i + x - x^2/2 + \cdots ,$$

and thus

$$\log (1 + x) = [x - x^2/2 + x^3/3 - \cdots]/k. \tag{4}$$

Setting $z=1$ in (3) gives

$$a = 1 + k + k^2/2! + k^3/3! + \cdots .$$

As a has been left arbitrary, we may take it so that $k=1$; calling this value e, we have

$$e = 1 + 1 + 1/2! + 1/3! + \cdots ,$$

while (4) goes over into the desired formula (1).

This demonstration from a Weierstrassian standpoint is about as bad as it could be; but then, are we not told now by the intuitionalists that a large part of the Weierstrassian mathematics is devoid of proof, if indeed it is not nonsense? Let us therefore be charitable. We have taken space to give this proof because it is entirely typical.

We have not space to follow the further history of "the little zeros" of Leibnitz and Euler. As Americans, it may interest us sufficiently to note that our greatest mathematician of earlier days, B. Peirce, used them without hesitation in his remarkable treatise *Curves, Functions, and Forces* (2 vols., 1841), as may be seen in Book II, Chapter 2, page 172 of volume 1.

4. *The Method of Lagrange.* Another eddy in the current of mathematical thought was produced by Lagrange's *Théorie des Fonctions Analytiques* (1st edition, 1797; 2d edition, 1813). He is dissatisfied with the little zeros of Leibnitz, Bernoulli, and Euler "which although correct in reality are not sufficiently clear to serve as foundation of a science whose certitude should rest on its own evidence." He is as little satisfied with the fluxions of Newton, which introduces a foreign notion, that of motion; moreover "one can see by the learned *Treatise on Fluxions* by Maclaurin how difficult it is to demonstrate the method of fluxions and how many ingenious artifices we must employ to demonstrate the different parts of this method."

Lagrange therefore proposes to get rid of little zeros and of limits at one stroke by founding the calculus on the development of a function in a power series (Taylor's or Maclaurin's). With becoming modesty he remarks that it is strange that this method of establishing the calculus did not occur to mathematicians earlier, especially to Newton, the inventor of the method of series and of fluxions. Let us see what this grand idea is.

If in $f(x)$ we replace x by $x+h$, "it becomes $f(x+h)$ and by the theory of series one can develop it in a series

$$(1) \qquad f(x+h) = f(x) + ph + qh^2 + rh^3 + sh^4 + \cdots ."$$

"Not to make gratuitous assumptions," Lagrange begins by examining the form of the series (1), and shows that the exponents of h are in fact integers and not fractions. He next studies the coefficients $p, q, r, \cdots$. To this end he observes that if we replace h by $h+k$ in (1) we get

$$(2) \qquad \begin{cases} f(x+h+k) = f(x) + (h+k)p + (h+k)^2q + \cdots \\ \qquad\qquad = f(x) + hp + h^2q + h^3r + \cdots \\ \qquad\qquad\quad + kp + 2hkq + 3h^2kr + \cdots \text{ etc.} \end{cases}$$

Also, if we replace x by $x+k$ in (1), we get

$$(3)\quad f(x+h+k)=f(x+k)+hp(x+k)+h^2q(x+k)+\cdots .$$

Now

$$f(x+k)=f(x)+kf_1(x)+\cdots,$$
$$p(x+k)=p(x)+kp_1(x)+\cdots,$$
$$q(x+k)=q(x)+kq_1(x)+\cdots,\text{ etc.}$$

These in (3) give

$$(4)\quad \begin{aligned} f(x+h+k)&=f(x)+hp+h^2q+h^3r+\cdots\\ &\quad+kf_1(x)+hkp_1(x)+h^2kq_1(x)+\cdots. \end{aligned}$$

Comparing (2) and (4) gives

$$(5)\qquad p=f_1(x),\quad q=\tfrac{1}{2}p_1(x),\quad r=\tfrac{1}{3}q_1(x),\cdots .$$

Lagrange now observes that we get q from p in the same way that we get p from $f(x)$, and a similar remark holds for the other coefficients $r, s, \cdots$ in (1).

He calls $f_1(x)$ the first derivative of $f(x)$; it is merely the first coefficient in the development (1), and not a differential coefficient obtained by a passage to the limit. He denotes it by $f'(x)$. This function also has a first derivative which he denotes by $f''(x)$ etc. Thus the relations (5) give

$$p=f'(x),\quad q=\frac{1}{2!}f''(x),\quad r=\frac{1}{3!}f'''(x),\cdots,$$

which in (1) give

$$f(x+h)=f(x)+hf'(x)+\frac{1}{2!}f''(x)+\cdots .$$

Lagrange now observes: "This new expression has the advantage of showing how the terms of the series depend on each other, and especially how when one knows how to form the first derivative function, one can form all the derivative functions which enter the series." Then a little later he adds: "For one who knows the rudiments of the (traditional) differential calculus, it is clear that these derivative functions coincide with

$$dy/dx, \quad d^2y/dx^2, \cdots ."$$

This then is the grand scheme: no more infinitesimals, no more prime and ultimate ratios whose principles have given rise to endless disputes. Differential coefficients are merely the coefficients in the development of a function.

When a modern reader looks over reasoning like this and bears in mind that Lagrange was one of the greatest mathematicians of all time, he is amazed. The great gulf that separates mathematical reasoning of to-day from that of date 1813 is brought home very clearly to him. The consoling feature about the work that we have seen thus far is this: the results are right although the reasoning is faulty. The intuition of these great men is far in advance of their logic. Is it not quite likely that a similar state of affairs holds in the theory of aggregates today?

Lagrange's method of development of the calculus free from the knotty questions regarding infinitesimals and limits was received with considerable favor. It suffers however a mortal defect. It rests upon the assumption that a given function can be developed in a power series, and there is no known method of deciding this question independently of the thing he wishes to avoid, namely limits.

5. *The Standpoint of Cauchy.* With Euler and Lagrange, a well defined period in the history of mathematics is closed. Euler is the last great formalist. We have just seen that Lagrange is fully alive to the objectionable reasoning of many of his contemporaries, and we have witnessed his vain efforts to give the calculus "toute la rigueur des démonstrations des Anciens," as he says. The true method was not Newton's, nor Euler's, nor Lagrange's, and yet it lay close at hand and had already been clearly stated by

d'Alembert in the *Encyclopédie,** in his article *Différentiel.* Here he writes: "What we most need to treat here is the metaphysics of the differential calculus. This metaphysics about which one has written so much is more important and perhaps more difficult to develop than even the rules of this calculus." He remarks that certain mathematicians cannot admit the suppositions which are made concerning infinitesimals, as they are false in principle and capable of leading to wrong results. He then goes on to say: "But when one observes that all the facts that have been discovered by ordinary geometry may be established by the calculus much more easily, one cannot help concluding that this calculus furnishes certain simple and exact methods, and rests on principles just as simple and certain."

These he thinks form the metaphysics of Newton, which Newton has revealed only in part. D'Alembert reads into Newton what Newton ought to have said but which (if we are not mistaken) no one had seen before; and this we believe is d'Alembert's great discovery. He says: "Newton has never regarded the differential calculus as a calculus of infinitesimals, but as a method of prime and ultimate ratios, that is to say, a method of finding the limit of these ratios. Newton has never differentiated quantities, but only equations, since every equation embraces a relation between two variables, and the differentiation of equations is merely finding the limit of the ratios between finite differences of the two variables which the equation involves."

In another article, on *Limits,* he says: "The theory of limits is the true metaphysics of the differential calculus."

The first treatise to adopt this standpoint to the exclusion of any other was (as far as I have ascertained) the *Traité Élémentaire* of Lacroix (2d edition, 1806; I have not seen the first edition). It is, however, with Cauchy that the new

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* *Encyclopédie ou Dictionnaire Raisonné des Sciences,* vol. 10, Geneva, 1770. This is, I believe, a pirated copy of the original French edition. I have not seen this latter; probably d'Alembert's article on the calculus was published for the first time some years earlier.

era begins. In his *Cours d'Analyse* (1821), his *Résumé des Leçons sur le Calcul Infinitésimal* (1823), his *Leçons sur le Calcul Différentiel* (1829), and in the *Leçons de Calcul Différentiel et de Calcul Intégral* (2 vols., 1840), by his pupil L'Abbé Moigno, we behold for the first time the foundations of the calculus developed with a rigor which is near to that of our own today. De Moigno in his *Introduction* observes: "Before Cauchy published his treatises, the demonstrations of the fundamental theorems of the calculus rested too often on the consideration of certain series which were used without discernment, without having examined their convergence, or if indeed they represented the functions which gave them birth. It was a veritable abuse against which Cauchy never ceased to protest. Never did he employ the development of a function in a series without first establishing its possibility, its form, its convergence, in a word its right to represent the given function." This we think is rather exaggerated, but it is true on the whole.

If one asks what is one of Cauchy's most obvious oversights as to rigor, we would say that it is overlooking the care that one must take in a very common process in analysis, i.e., the interchange in the order of passing to the limit in a double limit. Thus in his *Cours d'Analyse* (p. 131), he states that $F(x)=\sum u_n(x)$ is continuous if F is convergent and the u_n are continuous. Under the same conditions (*Résumé des Leçons*, Œuvres (2), vol. 4, p. 237),

$$\int_a^b F\,dx = \sum \int_a^b u_n\,dx.$$

Also (ibid., p. 195)

$$\frac{\partial}{\partial u}\int_a^b f(x,u)\,dx = \int_a^b \frac{\partial f}{\partial u}\,dx,$$

etc.

The justification of these and similar interchanges of limits rests on the notion of uniform convergence, which was not discovered till a later date (Stokes, 1847; Seidel, 1848; Cauchy, 1853).

Cauchy's standard of rigor was immeasurably in advance of his contemporaries (we except Gauss of course); it served as model for a generation of mathematicians. Abel,* writing to Holmboe from Paris (1826), says of Cauchy: "He is at present the only one who knows how mathematics should be treated." In another letter,† speaking of Taylor's development, he says: "I have found only a single rigorous demonstration, that of M. Cauchy."

6. *The Standards of Weierstrass.* However, even after making various minor improvements relative to double limits, etc., the last word on rigor had not been said; there was a still higher standard for which to strive. Indeed, so vastly superior was this new standard in the minds of some of its proselytes that they looked on the rest of their fellow mathematicians as living in utter darkness.

What was the new doctrine and who was its founder? Both questions can be answered by one word, it is a name revered and honored the world around, Weierstrass.

What the Weierstrassian doctrine is, is too well known to you for me to dwell upon. I may be allowed, however, to mention one or two matters which will come up for discussion when we reach the next era, the era of to-day. The great step in advance that Weierstrass took was to arithmetize analysis. Before then, many analytical facts were accepted as self evident. For example: if $f(x)$ is continuous in (a, b) and has opposite signs at a and b, then $f(x)=0$ at some point within the interval. One has merely to picture the graph of this function to see intuitively the truth of this theorem. That this type of reasoning is inadmissible rests on the following observation. Let us define with Cauchy: The function $f(x)$ is continuous for $x=c$ if $\lim f(x)=f(c)$ for $x \to c$. One assumes now that such continuous functions are coextensive with the class of functions as pictured by our geometric intuition. Many examples show us the contrary.

* Œuvres, vol. 2, p. 250.

† Ibid., p. 257

Perhaps the one that startled the easy going world the most was Weierstrass' continuous function having at no point a derivative. Another hardly less remarkable example was Peano's curve which passes through every point of a square. Finally we note a fundamental problem: What is the meaning of the limit of a sequence $a_1, a_2, a_3, \cdots$?

To arithmetize analysis one had to ban geometric reasoning; all proof must rest entirely on pure analysis. This forced Weierstrass to find an arithmetic equivalent of our geometric notion of an interval or segment of a straight line, i.e., to lay down an arithmetic theory of irrational numbers. Only then was he able to prove arithmetically the simplest properties of continuous functions, properties which had been accepted previously as too obvious to require even a passing remark. To meet the requirements of the new standards of rigor a great deal of the reasoning which had been regarded as valid had to be supplemented, or when too bad, to be replaced by considerations of a different type. If one wishes to form an idea how great these changes were, one cannot do better than to compare attentively the first edition of C. Jordan's *Cours d'Analyse* (1882–1887) with the second (1893–1896). In the calculus of variations the devastation produced by the teaching of Weierstrass was even greater, and the work of repair far more difficult.

By the end of the last century the Weierstrassian standards of rigor had won over practically the whole world. It was believed that absolute rigor had been reached, that on the foundations of Weierstrass our mathematical edifice, whose giddy summits seem to reach the clouds, rests so securely that nothing can disturb its massive repose. Illustrious names can be cited to support this statement; one will suffice. In his address before the Paris Congress (1900), Poincaré,* reviewing the arithmetization of mathematics, asks: "Have

* Compte Rendu du Deuxième Congrès International des Mathématiciens, p. 115.

we at last attained absolute rigor? At each stage of its evolution our forerunners believed they too had attained it. If they were deceived, are we not deceived like them?" After discussing the question he gives his verdict: "One may say to-day that absolute rigor has been attained." Alas, to-day, a scarce quarter of a century after these memorable words were uttered some there are who think we have been living in a fool's paradise. The mighty edifice is tottering, they think, perhaps soon to fall leaving a horrible ruin in its place. I personally do not believe this to be the case and this view is shared by most of the mathematicians of to-day. And yet when one hears one of the greatest living mathematicians calmly telling the world that a considerable part of our analysis is devoid of proof, if it is not nonsense, and when one beholds the mighty efforts which the champions of Weierstrass are making to repel these attacks, it is only reasonable, in view of such facts, to ask ourselves, "Is all well?"

We shall use the remainder of this address to give an account of this last stage in the evolution of mathematical rigor. It goes back to Kronecker, who confessed one day to Netto* that he had spent far more time thinking in philosophy than he had in mathematics. Philosophy and mathematics are not good companions.

If I had entered more fully into the early history of our theme, you would doubtless have been bored by a great deal of talk about its metaphysics. When the mathematician has not been clear in his own mind he has had recourse in the past to metaphysics. Bertrand Russell once humorously defined philosophy as the science that makes simple things complicated. Now the mathematician trained in the school of Weierstrass was fond of referring to his science as the absolutely clear science. Any attempt to drag in metaphysical speculation was resented with indignant energy. How the times have changed! O tempora, o mores! Now see

* E. Netto, Mathematical Papers, International Mathematical Congress, Chicago, 1893.

what philosophy did to Kronecker; later we shall see what it has done to Brouwer and Weyl, and what a revolution it wishes to bring about.

7. *Kronecker's Position.* To make Kronecker's position clear we may illustrate it by an example taken from algebra. Every one knows how fundamental the notion of reducibility is. Before Kronecker, algebraicists were content to say a polynomial $f(x)$ might have a rational factor, in which case $f(x)$ is reducible; in the contrary case it is irreducible. This is merely a logical definition of these terms; it is a case of "either," "or" with no means in sight as far as the definition is concerned to decide whether the given $f(x)$ is reducible or is not.

In his great Festschrift *Grundzüge einer arithmetischen Theorie der algebraischen Grössen,* Journal für Mathematik, vol. 92 (1882), Kronecker announces for the first time the new doctrine: "The definition of reducibility given in §1 is devoid of a sure foundation until a method is given by means of which it can be decided whether a given function is irreducible or not by the definition." In a footnote, he calls attention to the fact that "a similar need (often indeed unnoticed) is manifest in many other cases, in definition as well as in proof, and I shall take this up on another occasion in a general and careful manner." Only to a very minor degree did Kronecker carry out his intention, and that only in the field of algebra and algebraic numbers. In the great field of analysis he did nothing except to criticize in lectures and conversation the work of his contemporaries. Let us see how deep the new program cuts. The foundation of exact analysis is the real number system, whether defined as by Weierstrass, or as by Dedekind, or as by Méray and Cantor. Of two real numbers a and b, either $a=b$ or $a>b$ or $a<b$, and definitions are given for each case. According to Kronecker they are only definitions in appearance, since these definitions do not give the means of deciding in each case which of these three alternatives holds.

Another example is the fundamental theorem of Bolzano-Weierstrass relative to the upper and lower limits of a limited function. The well known proof consists in establishing the logical existence of a sequence of intervals one within the other, but it does not in general give the means of calculating these limits. The definition, therefore, is not a valid definition and must be rejected, from Kronecker's standpoint.

In the same year (1882) that Kronecker promulgated his new doctrine, Lindemann succeeded in showing that π is not algebraic, and so showed the futility of trying to square the circle. In a conversation with Lindemann,* Kronecker asks "Of what use is your beautiful investigation regarding π? Why study such problems, since irrational numbers are nonexistent?" Kronecker's attitude is made still clearer by the following extract of a letter of Weierstrass† to Mme. Kowalevski (1885): "I say that a so-called irrational number has as real an existence as any other object of the mind; Kronecker on the contrary regards it now as an axiom that only equations between whole numbers exist . . . .

"It makes the matter worse that Kronecker uses his authority to support the view that all who have labored on the foundations of the function theory are sinners before the Lord. When a person like Christoffel says that in twenty or thirty years the present function theory will be buried, and that analysis will be reduced to a theory of forms, one answers with a shrug of the shoulders; but when Kronecker makes this assertion which I reproduce word for word: 'If I still have the time and the energy, I will myself show the mathematical world that not only geometry but also arithmetic can point the path to analysis, and certainly a more rigorous one. If I cannot do this, then another will who comes after me, and the world will recognize the inexactitude of the types of proof now employed in analysis,' such a statement I say,

* H. Poincaré, *Wissenschaft und Hypothese*, 3d edition, 1914, p. 252.

† G. Mittag-Leffler, *Une page de la vie de Weierstrass*, Paris Congress (1900), pp. 150–151.

. . . is not only humiliating to those who are thus recommended to acknowledge their error and abjure what has been the object of their unremitting thoughts and efforts, but it is also a direct invitation to the younger generation to leave their present leaders and to group themselves about him as the apostle of a new doctrine which, it is true, has yet to be established. Really this is sad and fills me with bitter pain." The revolutionary movement inaugurated by Kronecker in 1882 apparently died an early death from sheer inanition. It was easy for him dogmatically to assert: "Definitions must contain the means of reaching a decision in a finite number of steps and existence proofs must be conducted so that the quantity in question can be calculated with any required degree of accuracy," but outside of algebra he took no steps to realize his program, nor did any one for a quarter of a century seriously make the attempt. We therefore leave this movement for the present and consider briefly quite another subject.

8. *Rigor in Geometry.* So far we have said nothing of rigor in geometry. The reason is obvious; more than two thousand years before, geometry went through the process which we have just sketched for analysis. In the *Elements* of Euclid we have the results of a long period of critical analysis of the most acute Greek minds. After the downfall of the antique world, the intellectual world of Western Europe lay buried in darkness till the great awakening in the sixteenth and seventeenth centuries. No wonder that the achievements of Greek learning seemed almost superhuman to a people who had outgrown the futile subtleties of scholasticism. To the mathematicians of the Renaissance and long after, the *Elements* of Euclid was a work of superlative excellence. Let the following serve as an example.* It is "a work whose propositions have such an admirable connection and dependence that take away but one, and the whole falls; whose method is the most just, admitting and advancing nothing

* Benjamin Martin, *Biographia Philosophica*, London, 1764, p. 56.

without a demonstration, and no demonstration but from what foregoes, and these so convincing, elegant, and perspicuous that it is beyond the skill of man to contrive better. Here the most artful and diligent carpers have never been able to set a footing."

However the "artful and diligent carpers" have found first one "footing" and then another, so that to-day it is generally recognized that the *Elements* are far, very far, from being a perfect work. Definitions are given which do not define, axioms are implied but not stated, certain proofs are needlessly complicated. Perhaps the most unsatisfactory feature relates to congruence and the implication of motion or displacement. On the other hand there is much that we to-day admire not with the blind idolatry of the past but with the critical knowledge of a connoisseur. We have a great advantage over the Greeks therein that a study of various non-euclidean geometries has enabled us to realize the difficulties which beset an entirely rigorous treatment of euclidean geometry.

There is no space in this address to discuss the history of the critical movement in geometry; we must however mention one aspect of it on account of its great importance in the thought of to-day relative to the foundations of analysis. We refer to the axiomatic treatment of the foundations of geometry which reached its highest excellence in Hilbert's *Grundlagen der Geometrie* (1899). Instead of trying to define a point, a straight, a plane, this method introduces three sets of things and subjects them to certain relations called axioms. One has no mental picture of these things; the reasoning makes no call on our geometric intuition, it is purely formal. On this account it is absolutely necessary to give a proof of freedom of contradiction and this is done by showing that any contradiction would involve a contradiction in arithmetic.

9. *Arithmetic.* The non-contradiction of the axioms of geometry is thus referred to the axioms of our number

system. Are the laws of arithmetic non-contradictory? However far we carry our developments, can we ever arrive at a result $1=0$, for example? No one in his senses has ever believed this; yet mathematics is not a science which rests on faith, but on proof. We are thus led back to our main theme, the foundations of analysis. A most important part of this theory relates to the number system.

We may begin our discussion by asking "What are the integers 1, 2, 3, $\cdots$?" The question is as elusive as the question, "What is a straight line?" We do not expect the philosophers to agree as to what a number is, and indeed they have not agreed; but we do expect the mathematicians to be unanimous. Do not numbers form the very basis of all analysis? E. G. Husserl in his *Philosophie der Arithmetik* (1891) observes "Not one significant question do I know which those concerned have answered with even tolerable harmony."

The first attempt to establish rigorously the laws of arithmetic was, as far as I know, by R. Dedekind in *Was sind und was sollen die Zahlen* (1887). He bases his development on infinite aggregates, i.e., as he defines them, on sets which can be put in uniform correspondence with a subset. It was incumbent on him to prove the existence of such sets. His reasoning (page 17) rests on the set of all things and is thus open to the objection of Burali-Forti's paradox.

G. Frege's *Grundgesetze der Arithmetik* (vol. 1, 1893; vol. 2, 1903) also makes use of the aggregate theory. At the close of vol. 2 (p. 253) he remarks "A scientist can hardly meet with anything more undesirable than to have the foundation give way just as the work is finished. In this position I was put by a letter from Mr. Bertrand Russell as the work was nearly through the press." In his despair Frege's only comfort is "Solatium misereris, socios habuisse malorum," if indeed this is a comfort. His criticism of his predecessors was merciless; what a bitter pill it must have been for him to illustrate the old adage, "Errare humanum est!" Russell's criticism has to do with the paradox named after him, namely, the set of all sets which are not members of themselves.

These unsuccessful attempts of Dedekind and Frege to found the number system on the notion of infinite aggregates have brought us now face to face with the notorious contradictions in Cantor's Mengenlehre which we referred to at the beginning of this address. Paradoxes were no new thing in philosophy. Did not Epimenides, the Cretan, say that all Cretans are liars, and did not Zeno the Eleate show that Achilles could not overtake the tortoise? They were something very new, however, in mathematics. Burali-Forti's paradox (1897) was like a bomb, soon to be followed by other bombs of like nature. Consternation spread among mathematicians; what was to be done? The greatest authorities were called in consultation; their diagnoses were poles apart: quot homines, tot sententiae. Poincaré* at the Rome Congress (1908) went so far as to say "Later generations will regard the Mengenlehre as a disease from which one has recovered." To get rid of these paradoxes, various lines of action have been adopted. One may do as Cantor, Dedekind and Frege did: give up, quit the field. For such we would recall a saying of DeMorgan† prompted by the contradictions arising from divergent series: "The history of algebra shows us that nothing is more unsound than the rejection of any method which naturally arises on account of one or more apparently valid cases in which such method leads to erroneous results. Such cases should indeed teach caution, but not rejection; if the latter had been preferred to the former, negative quantities, and still more their square roots, would have been an effectual bar to the progress of algebra · · · and those immense fields of analysis over which even the rejectors of divergent series now range without fear, would have been not so much as discovered, much less cultivated and settled."

* O. Hölder, *Die mathematische Methode,* Berlin, 1924, p. 556.

† A. DeMorgan, *Differential and Integral Calculus,* London, 1842, p. 566.

Other lines of action rest on the answer to the query: when is a mathematical object or process defined? Some say, its definition can employ but a finite number of words. For example Poincaré, discussing Zermelo's well ordering of the continuum said:* "There are two cases; either one asserts that the method of well ordering is expressible in finite terms ('endlich aussagbar'), in which case the assertion is not proved; · · · or we allow the possibility that the method is not 'endlich aussagbar'? In this case I can attach no meaning to the procedure; for me it is only empty words."

In another place,† Poincaré asks: "Is it possible to reason on objects which cannot be defined by a finite number of words? Is it even possible to speak of them, knowing what one is speaking of, pronouncing only empty words? Or on the contrary should one not regard them as unthinkable? For my part, I do not hesitate to respond that they are pure nonentities." Mathematicians of this stamp are called finitists; their position is hotly attacked by the Cantorians.

Poincaré holds that the paradoxes of the theory of aggregates are due to the use of non-predicative definitions. In his *Science et Méthode* (Paris, 1912), page 207, he says: "Thus the definitions which should be regarded as non-predicative are those which contain a vicious circle." On page 208 he declares: "A definition which contains a vicious circle defines nothing." This view is also held by B. Russell. The Cantorians claim that this position cannot be maintained, since without such definitions modern analysis would be robbed of some of its most valuable results.‡

10. *The Logistic Group.* Without making too fine distinctions we may say that those engaged in laying the new foundations of analysis fall into three groups:

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* H. Poincaré, *Sechs Vorträge*, Leipzig, 1910, p. 48; they were held at Göttingen in 1909.

† *Dernières Pênsées*, Paris, 1913, p. 132.

‡ E. Zermelo, *Neuer Beweis für de Möglichkeit einer Wohlordnung*, Mathematische Annalen, vol. 65 (1907), p. 107.

(1) The logistic: the Italian school is led by Peano, the English by Russell and Whitehead.

(2) The axiomatic: led by Hilbert.

(3) The intuitionist: led by Brouwer.

The first point to note about the logistic group is the fact that their work is written in a sign-language invented by Peano and extended by Russell. Relatively few people are willing to learn this language and these works therefore are for the most part unknown. We may summarize some of their leading ideas as follows. Their cardinal idea is that mathematics is a part of logic. The concepts and processes of mathematics on careful analysis are found to be few in number and to admit a symbolic treatment analogous to algebraic manipulations. By giving each symbol a precise unique meaning one hopes to avoid pitfalls that beset ordinary mathematical reasoning due to the ambiguity of common language. Moreover the symbolism itself is an aid in reasoning in the same way that algebraic symbols help us in ordinary arithmetic. The logistic program is to reduce all mathematics to symbolic logic; this has already been partly accomplished.* One of the great difficulties in this program is to avoid the contradictions and paradoxes of the theory of aggregates. Russell observed that these contradictions could all be avoided by using his vicious circle principle.† "Whatever involves all of a collection must not be one of the collection." To make this observation effective he has invented a theory of types. In this theory propositional functions and classes form a hierarchy according to their possible arguments, also a distinction is made between the various functions belonging to the same argu-

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* Formulaire de Mathématique, vol. 1, 1895; vol. 2, 1897–99; vol. 3, 1901; vol. 4, 1902–03. The different parts are written by a variety of persons, Peano, Burali-Forti, Vailati, Padoa, Vivanti, etc. See also A. N. Whitehead and B. Russell, *Principia Mathematica* (3 vols.), 2d edition, 1925-27.

† B. Russell, American Journal of Mathematics, vol. 30 (1908), p. 225. Whitehead and Russell, *Principia Mathematica* (2d edition, 1925), p. 58.

ment. By this means all known contradictions of the aggregate theory are avoided, and mathematical induction is established. In order to establish other parts of analysis, for example the irrational numbers, Russell is obliged to introduce a certain axiom, the axiom of reducibility. This axiom states that any combination or disjunction of predicates is equivalent to a single predicate. By this means the order of a nonpredicative function can be lowered by one, so that after a finite number of steps we reach an equivalent predicative function. This axiom of reducibility seems to have excited universal opposition. One author states that its introduction is an act of harikari. Ramsey* holds that there is no reason to suppose that it is true, and if it were, it would be a happy accident and not a logical necessity. It has no place in mathematics, and what cannot be proved without it cannot be regarded as proved at all. Ramsey believes he has discovered how the work of Wittgenstein† can be utilized so as to free the *Principia* from the objections which have caused its rejection by the majority of German authorities.

11. *The Axiomatic Group.* The great leader of this group is Hilbert. His masterly treatment of the foundations of geometry already referred to quite prepared the mathematical public to lend a willing ear to his address *Ueber die Grundlagen der Logik und der Arithmetik* at the Heidelberg Congress, 1904. After referring to the difficulties which beset a rigorous development of the number system due in part to paradoxes of the aggregate theory, Hilbert announces his program as follows: "I believe that all the difficulties which I have touched upon may be overcome and an entirely satisfactory foundation of the number concept can be reached by a method which I call the axiomatic method, and whose leading idea I wish now to develop."

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* F. P. Ramsey, *The foundations of mathematics*, Proceedings of the London Mathematical Society, (2), vol. 25 (1926), p. 338.

† L. Wittgenstein, *Tractatus Logico-Philosophicus*, London, 1922.

Hilbert has treated this subject in five other papers, and I do not believe he has said his last word yet. These papers are:

Ueber den Zahlbegriff, Jahresbericht der deutschen Mathematiker Vereinigung, vol. 8 (1900).

Axiomatisches Denken, Mathematische Annalen, vol. 78 (1918), p. 405.

Neubegründung der Mathematik, Abhandlungen des Mathematischen Seminars der Hamburgischen Universität, vol. 1 (1922), p. 157.

Die Logischen Grundlagen der Mathematik, Mathematische Annalen, vol. 88 (1922–23), p. 151.

Ueber das Unendliche, Mathematische Annalen, vol. 95 (1926), p. 161.

As in the days of Newton and Leibniz, so now the notion of infinity is our greatest friend; it is also the greatest enemy of our peace of mind. We may compare it to a great waterway bearing the traffic of the world, a waterway however which from time to time breaks its bounds and spreads devastation along its banks. Weierstrass taught us to believe we had at last thoroughly tamed and domesticated this unruly element. Such however is not the case; it has broken loose again. Hilbert and Brouwer have set out to tame it once more. For how long? We wonder.

Hilbert thinks it can become our useful servant and preserve all its powers uncurtailed. Brouwer thinks this is impossible; we can at most build a canal through our territories and allow a fraction of the infinite to pass through it.

We use the notion infinity in two ways illustrated by the integers 1, 2, 3, 4, $\cdots$. In one sense, this is an endless sequence, such that after each element there follows another; in the other, we regard them in their totality, as a finished product. We have two other common infinite notions, space and time; neither has a bound. After each moment of time there is another; after each point on a straight there is another. We never think of these things as closed or finished.

Now Gauss wished* only such a conception of the infinite in any part of mathematics. For him there was no actual infinity, only an endless growing or becoming. It is this notion of an actual infinity which lies at the basis of Cantor's theory of transfinite numbers. Of this theory Hilbert says: "This seems to me the most admirable fruit of the mathematical mind and in fact one of the highest achievements of man's purely intellectual processes." After remarking on the catastrophic effects that the paradoxes of Russell etc. have caused, he declares: "No one shall drive us out of the paradise that Cantor created for us." We have seen what Poincaré thought of this paradise: that it is a non-entity. We will presently examine Brouwer's ideas about it.

Hilbert tells us that for a quarter of a century the questions relating to the foundations of mathematics have never been out of his thoughts, yet in his last paper *Ueber das Unendliche*, he is forced to admit that the present state of affairs relative to the paradoxes of the aggregate theory is intolerable. However, let us be comforted, for Hilbert assures us: "There is an entirely satisfactory way to escape the paradoxes without betraying our science." This goal can be reached only when the notion of the infinite has been made entirely clear.

To this end he turns to the physical sciences. He finds that the tendency in modern science is an emancipation from the infinite. Matter is not continuous, but atomic; so also is energy. The infinite space of our forefathers has shrunk to a finite volume, and has become elliptic.

How does this accord with our mathematical concepts? Does not nature show us that we are on the wrong track in dealing with the infinite? In opposition to Frege and Dedekind, who thought to develop the number system independently of intuition or experience by employing an actual infinity, Hilbert finds that a prerequisite of scientific knowledge is a fund of intuitive ideas; pure logic is not

* Werke, vol. 8, p. 216.

sufficient. Operations on the infinite can be rendered certain by an appeal to the finite. In Kant's philosophy, an idea is a concept of the intellect which transcends all experience, and by which the concrete of our senses is made complete as a totality. This is the role left to infinity: it is an idea.

Although Hilbert is quite opposed to Russell and Whitehead in the belief that mathematics is a part of logic, it is interesting to note how the methods of symbolic logic creep into Hilbert's work. Kepler found the conic sections of the ancients ready at hand. Einstein found the tensor analysis of Ricci and Levi-Civita likewise. Behold the same pre-established harmony is again made manifest. The symbolic logic of Peano, Russell, and Whitehead lies before him like a ripe fruit ready to be picked.

Hilbert takes this logic but reinterprets it. For him it is a sign-language which puts mathematical theorems into formulas and which expresses logical reasoning by formal processes. Signs and symbols of operation are freed from their significance with respect to content. The axioms of mathematics merely express the rules by which formulas follow one another. Beyond these meaningless signs and formulas which constitute mathematics there is a "meta-mathematics" which deals only with the concrete and never employs but a finite number of logical steps of a kind universally admitted.

The first axioms laid down therefore relate to the finite; for them the laws of ordinary logic hold; their freedom of contradiction is intuitive. We must however pass to the transfinite and use "all," "there is," "the excluded middle," "complete induction" etc. To this end Hilbert introduces the axiom

$$A(\tau A) \rightarrow A(a)$$

which he says means in ordinary language "If a predicate A applies to the object τA, it applies to all objects a." To illustrate this Hilbert supposes A is the predicate "is venal." Then $A(\tau A)$ is to be regarded as a definite person of such uncorruptible sense of justice that should it turn out that he were venal, it would follow that all mankind is venal.

Hilbert regards this transfinite axiom as adjoined to the finite axioms just as in algebra we adjoin to the real number system the imaginary numbers, and in geometry we adjoin to real space the ideal plane at infinity. Still more striking he finds the analogy with the ideals in the theory of algebraic numbers. How strange it comes about, Hilbert exclaims, that this (transfinite) form of reasoning, which Kronecker so passionately opposed, proves to be the exact analog of Kummer's ideals, these numbers which Kronecker so ardently admired and praised as the highest mathematical achievement.

Having found a system of axioms of sufficient generality for all the needs of modern analysis, there remains the final and most important step: the placing of the keystone to the arch, which in this case is the proof of the freedom from contradiction of these axioms. Having outlined how this is to be accomplished, Hilbert illustrates the power of his methods of proof by disposing (rather summarily) of such oft-debated problems as complete induction, the Weierstrass theorem of the existence of upper and lower limits, Zermelo's axiom.

12. *Intuitionalism.* The chief figure in this group is L. E. J. Brouwer, professor of mathematics at the University of Amsterdam. He early attained international eminence for his extraordinarily subtle and far reaching papers in the theory of point sets (analysis situs). His doctor's dissertation (1907) was *Over de grondslagen der wiskunde* (*On the foundation of mathematics*). His inaugural address as professor at the Amsterdam university (1912) was on *Intuitionism and formalism.* A translation of this by Professor A. Dresden appeared in this Bulletin, vol. 20 (1913–14). Beginning with 1918, Brouwer has expounded his theory in a series of papers in the Proceedings of the Amsterdam Academy of Sciences, in the Mathematische Annalen, and in the Journal für Mathematik.

It appears that Brouwer, like Kronecker and H. Weyl, is very much of a philosopher; his view of mathematics we would expect to be much influenced by his philosophical speculations. If he holds certain mathematical tenets radically different from the usual ones of to-day, his justification will probably be of a philosophical nature.* We recall that Kronecker declared that if he did not live to carry out his program of overhauling mathematics from the bottom up, another would come who would. The fulfillment of this prophecy seems to be Brouwer.

Brouwer not only agrees with Kronecker that a definition must contain the means of constructing the object defined, but he carries this idea to its logical end and declares the use of the logical form known as the *tertium non datur*, or excluded middle, is legitimate only for finite sets. Brouwer believes† this law arose from the consideration of finite sets in mathematics; after being adopted by logic it was given an a priori existence independent of its mathematical origin, and then by virtue of its a priori character it was unjustly extended by mathematicians to infinite sets. By adopting these two restrictions Brouwer robs himself of two of the most powerful aids of research in modern analysis. Now no one would have an objection to any mathematician's trying to see what he could do under certain restrictions; one might regret such efforts as a waste of time, or look at the affair as a sporting proposition, like swimming the English Channel with one's hands tied. This is not Brouwer's attitude, however. Like Kronecker, he does not hesitate to tell his contemporaries that they are wrong. Thus in his *Intuitionistische Mengenlehre*,‡ he says that the Komprehensionsaxiom, by virtue of which all things which have a certain property are united to form an aggregate (Menge), even in

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* The reader may consult a paper by A. Dresden, *Brouwer's contribution to the foundations of mathematics*, this Bulletin, vol. 30 (1924), p. 31.

† Jahresbericht der Vereinigung, vol. 28 (1919), p. 203.

‡ Proceedings, Amsterdam Academy, vol. 23 (1922), p. 949.

the limited form used by Zermelo, is inadmissible in founding the aggregate theory, or is at least unserviceable. Only in a constructive definition of aggregates is a trustworthy foundation of mathematics to be found. Further the principle of the excluded middle is an unpermissible means of proof, which can be allowed only a scholastic or heuristic value; so that theorems which cannot be proved otherwise are devoid of any mathematical content. There is no sufficient ground for admitting this principle; logic rests on mathematics, and not conversely. In his paper* *Ueber die Bedeutung des Satzes vom ausgeschlossen Dritten*, Brouwer remarks: "On this foundation, particularly in the last half century, extensive false theories have been erected. The contradictions which have been repeatedly encountered have brought to life the formalistic criticism." He grants that their axiomatic treatment will avoid contradictions, "but nothing of mathematical value will be attained in this manner; a false theory which is not halted by a contradiction is none the less false, just as a criminal policy unhindered by a reprimanding court is none the less criminal." These are strong words, and strong words are usually met by others. This is Hilbert's counterblast.† "What Weyl and Brouwer are doing, is mainly following in the path of Kronecker; they are trying to establish mathematics by throwing overboard everything which does not suit them and dictatorially promulgating an embargo. The effect of this is to dismember and cripple our science and to run the risk of losing a large part of our most valuable possessions. Weyl and Brouwer condemn the general notions of irrational numbers, of functions, even number theoretic functions, Cantor numbers of higher classes etc., the theorem that an infinite set of positive integers has a least, and even the 'tertium non datur', so for example the statement: either there is only a finite number of primes or there are infinitely

* Journal für Mathematik, vol. 154 (1925), p. 1.

† *Neubegründung der Mathematik*, Abhandlungen des Mathematischen Seminars der Hamburgischen Universität, vol. 1 (1922), p. 157.

many. These are examples of forbidden theorems and modes of reasoning. I believe that powerless as Kronecker was to abolish irrational numbers (Weyl and Brouwer do allow us to retain a torso), no less powerless will their efforts prove to-day. No, Brouwer's program does not signify a revolution, but merely the repetition of a vain coup de main with former means, but which then was undertaken with greater dash, yet totally failed. To-day the State is thoroughly armed and strengthened through the labors of Frege, Dedekind, and Cantor. Weyl and Brouwer's efforts are doomed in advance to futility."

We have stated the two main theses of Brouwer: the rejection of the comprehension axiom as used by Cantor and Zermelo, and the law of the excluded middle.

In general the rejection of the law of the excluded middle produces great complication, which we are inclined to believe represents an element of strength rather than of weakness in Brouwer's theory. To go further into Brouwer's theory would require fresh definitions, and space does not permit this.

All new theories have to struggle for existence and recognition. Brouwer's theory is not easy to read. So far his theory has not made matters simpler; quite the reverse. It may be that this is necessary. Cauchy's methods forced the reasoning of his predecessors into innocuous desuetude; Weierstrass showed Cauchy's reasoning was far from perfect. Is Brouwer destined to lay down the standards of the next generation?

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