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mere modesty but fact. As for Leibniz, he was one of the great intellects. We have already cited his contributions to many fields (Chapter III). He ranks with Aristotle in intellectual strength and breadth. Of course, the calculus involved new and very subtle ideas, and the best of creative minds do not necessarily grasp even their own creations firmly.

Not able fully to clarify concepts and justify operations, both men relied upon the fecundity of their methods and the coherence of their results and pushed ahead with vigor but without rigor. Leibniz, less concerned about rigor though more responsive to critics than Newton, felt that the ultimate justification of his procedures lay in their effectiveness. He stressed the procedural or algorithmic value of what he had created. Somehow he had confidence that if he formulated clearly the rules of operation and that if these were properly applied, reasonable and correct results would be obtained, however vague the meanings of the concepts involved might be. Like Descartes, he was a man of vision who thought in broad terms. He saw the long-term implications of new ideas and did not hesitate to declare that a new science was coming to light.

The foundations of the calculus remained unclear. The proponents of Newton's work continued to speak of prime and ultimate ratios while the followers of Leibniz used infinitesimals, the infinitely small, non-zero quantities. The existence of these dissimilar approaches added to the difficulty of erecting the proper logical foundations. Moreover, many of the English mathematicians, perhaps because they were in the main still tied to Greek geometry, were more concerned with rigor and so distrusted both approaches to the calculus. Other British mathematicians chose to study Newton instead of mathematics and so made no progress toward rigorization. Thus the 17th century ended with the calculus, as well as arithmetic and algebra, in a muddled state.

Despite the muddle, uneasiness, and some opposition, the great 18th-century mathematicians not only vastly extended the calculus but derived entirely new subjects from it: infinite series, ordinary and partial differential equations, differential geometry, the calculus of variations, and the theory of functions of a complex variable, subjects which are at the heart of mathematics today and are collectively referred to as analysis. Even the doubters and critics used the various types of numbers and the algebraic and calculus processes freely in these extensions as though there were no longer any problem of logical foundations.

The extension of the calculus to new branches introduced new concepts and methodologies which compounded the problem of rigorizing the subject. The treatment of infinite series may serve to illustrate the infinite series. see (170)

additional complications. Let us note first just what problems infinite series presented to mathematicians.

The function $1/(1+x)$ can be written as $(1+x)^{-1}$ and, by applying the binomial theorem to the latter form, one finds that

$$\frac{1}{1+x}=(1+x)^{-1}=1-x+x^2-x^3+x^4\cdots, \tag{8}$$

wherein the dots indicate that the terms continue indefinitely and follow the pattern indicated by the few terms already shown. Now the original intent in introducing infinite series into the calculus was to use them in place of the functions for operations, such as differentiation (finding derivatives) and antidifferentiation because technically it is easier to work with the simpler terms of the series. Moreover, series for functions such as $\sin x$ were used to calculate the values of the functions. In all these uses, it is important to know that the series is the equivalent of the function. Now functions have numerical values when values are assigned to x. The first question then one must raise about the series is what value does it yield for a given value of x. In other words, what do we mean by, and how do we obtain, the sum of an infinite series? The second question is whether the series represents the function for all values of x, or at least for all values for which the function has meaning.

In his first paper on the calculus (1669), Newton proudly introduced the use of infinite series to expedite the processes of the calculus. Thus, to integrate (antidifferentiate) $y=1/(1+x^2)$, he used the binomial theorem to obtain

$$y=1-x^2+x^4-x^6+x^8-\cdots,$$

and integrated term by term. He noted that if, instead, one writes the same function as $y=1/(x^2+1)$, one obtains by means of the binomial theorem

$$y=\frac{1}{x^2}-\frac{1}{x^4}+\frac{1}{x^6}-\frac{1}{x^8}+\cdots.$$

He then remarked that when x is small enough, the first expansion is to be used, but when x is large, the second one is to be used. Thus he was somewhat aware that what we now call convergence is important, but he had no precise notion about this.

The justification given by Newton for his use of infinite series exemplifies the logic of the time. He said in this paper of 1669:

> Whatever the common Analysis [algebra] performs by Means of Equations of a finite Number of Terms (provided that can be done) this [new analysis] can always perform the same by Means of infinite Equa-

> tions [series] so that I have not made any question of giving this the name of Analysis likewise. For the reasonings in this are no less certain than in the other; nor the equations less exact; albeit we Mortals whose reasoning powers are confined within narrow limits, can neither express, nor so conceive all the Terms of these Equations, as to know exactly from thence the quantities we want.

Thus for Newton infinite series were just a part of algebra, a higher algebra which dealt with an infinite instead of a finite number of terms.

As Newton, Leibniz, the several Bernoullis, Euler, d'Alembert, Lagrange, and other 18th-century men struggled with the strange problem of infinite series and employed them in analysis, they perpetrated all sorts of blunders, made false proofs, and drew incorrect conclusions; they even gave arguments that now with hindsight we are obliged to call ludicrous. A brief examination of some of the arguments may show the bewilderment and the confusion in their handling of infinite series.

When $x = 1$, the series (8) which represents $1/(1+x)$,

$$\frac{1}{1+x} = 1 - x + x^2 - x^3 + x^4 \cdots \tag{8}$$

becomes

$$1 - 1 + 1 - 1 + 1 \cdots.$$

The question of what the sum of this series is engendered endless disputation. It seemed clear that by writing the series as

$$(1 - 1) + (1 - 1) + (1 - 1) \cdots$$

the sum should be 0. It seemed equally clear, however, that by writing it as

$$1 - (1 - 1) - (1 - 1) \cdots$$

the sum should be 1. But it is also true that if S denotes the sum of the series, then

$$S = 1 - (1 - 1 + 1 - 1 + \cdots)$$

or

$$S = 1 - S.$$

Hence $S = \frac{1}{2}$. This last-mentioned result was supported by another argument. The series is geometric with common ratio -1, and the sum of an infinite geometric series whose first term is a and whose common ratio is r is $a/(1-r)$. In the present case then the sum is $1/[1-(-1)]$ or $\frac{1}{2}$.

Guido Grandi (1671–1742), in his little book *Quadratura circuli et hyperbolae* (*Quadrature of Circles and Hyperbolas*, 1703), obtained the third result, ½, by another method. He set $x = 1$ in equation (8) and obtained

$$\frac{1}{2} = 1 - 1 + 1 - 1 \cdots .$$

Grandi therefore maintained that 1/2 was the sum of the series. He also argued contrariwise that the sum was 0, and so he had proved that the world could be created out of nothing.

In a letter to Christian Wolf published in the *Acta eruditorum* of 1713, Leibniz treated the same series. He agreed with Grandi's result but thought that it should be possible to obtain it without resorting to the original function. Instead Leibniz argued that if one takes the first term, the sum of the first two, the sum of the first three, and so forth, one obtains 1, 0, 1, 0, 1, · · ·. Thus 1 and 0 are equally probable; one should therefore take as the sum the arithmetic mean, namely 1/2, which is also the most probable value. This argument was accepted by James, John, and Daniel Bernoulli and Lagrange. Leibniz conceded that his argument was more metaphysical than mathematical but went on to say that there is more metaphysical truth in mathematics than is generally recognized.

In a letter of 1745 and a paper of 1754/55, Euler took up the summation of series. A series in which by continually adding terms we approach closer and closer to a fixed number is said to be convergent and the fixed number is its sum. This according to Euler will happen when the terms continually decrease. A series whose terms do not decrease and may even increase is divergent and since this type, he said, comes from known explicit functions, one can take the value of the function as the sum of the series.

Euler's theory led to additional problems. He took up the expansion:

$$\frac{1}{(1+x)^2} = (1+x)^{-2} = 1 - 2x + 3x^2 - 4x^3 + \cdots .$$

For $x = -1$, he obtained

$$\infty = 1 + 2 + 3 + 4 + \cdots .$$

This sum seemed reasonable. However, Euler then considered the series for $1/(1-x)$, namely,

$$\frac{1}{(1-x)} = 1 + x + x^2 + x^3 + \cdots ,$$

and let $x = 2$. Then

$$-1 = 1 + 2 + 4 + 8 + \cdots .$$

Since the sum of the right-hand side of this series should exceed the sum of the preceding one, Euler concluded that -1 is larger than infinity. Some of Euler's contemporaries argued that negative numbers larger than infinity are different from those less than zero. Euler objected and argued that ∞ separates positive and negative numbers just as zero does.

Euler's views on convergence and divergence were not sound. Even in his day, series were known whose terms continually decrease but which do not have a sum in his sense. Also he himself worked with series that do not come from explicit functions. Hence his "theory" was incomplete. Further, in a letter (now lost) of 1743, Nicholas Bernoulli (1687–1759) must have pointed out to Euler that the same series may come from different expressions and so, according to Euler's definition, one would have to give the sums of these series different values. But Euler replied (in a letter to Goldbach in 1745) that Bernoulli gave no examples and he himself did not believe that the same series could come from two truly different algebraic expressions. However, Jean-Charles Callet (1744–1799) did give an example of the same series coming from two different functions, which Lagrange tried to brush aside by an argument that was later seen to be fallacious.

Euler's treatment of infinite series was inadequate for additional reasons. Series are differentiated and integrated, and the fact that the differentiation and integration of a series also yield the derivative and antiderivative of the function must be justified. Nevertheless, Euler declared, "Whenever an infinite series is obtained as the development of some closed expression [formula for a function], it may be used in mathematical operations as the equivalent of that expression, even for values of the variable for which the series diverges." Thus, he said, we can preserve the utility of divergent series and defend their use from all objections.

Other 18th-century mathematicians also recognized that a distinction must be made between what we now call convergent and divergent series, though they were not at all clear as to what the distinction should be. The source of the difficulty was of course that they were dealing with a new concept, and like all pioneers they had to struggle to clear the forest. Certainly, the initial thought of Newton, adopted by Leibniz, Euler, and Lagrange—that series are just long polynomials and so belong in the domain of algebra—could not serve to rigorize the work with series.

The formal view dominated 18th-century work on infinite series. Mathematicians even resented any limitations on their procedures, such as the need to think about convergence. Their work produced useful results and they were satisfied with this pragmatic sanction.

They did exceed the bounds of what they could justify, but they were on the whole prudent in their use of divergent series.

Though the logic of the number system and of algebra was in no better shape than that of the calculus, mathematicians concentrated their attacks on the calculus and attempted to remedy the looseness there. The reason for this is undoubtedly that the various types of numbers appeared familiar and more natural by 1700, whereas concepts of the calculus, still strange and mysterious, seemed less acceptable. In addition, while no contradictions arose from the use of numbers, contradictions did arise from the use of the calculus and its extensions to infinite series and the other branches of analysis.

Newton's approach to the calculus was potentially easier to rigorize than Leibniz's, though Leibniz's methodology was more fluid and more convenient for application. The English still thought they could secure rigor for both approaches by tying them to Euclidean geometry. But they also confused Newton's moments (his indivisible increments) and his use of continuous variables. The Continentals followed Leibniz and tried to rigorize his concept of differentials (infinitesimals). The books written to explain and justify Newton's and Leibniz's approaches to the calculus are too numerous and too misguided to warrant examination.*

While these efforts were being made to rigorize the calculus, some thinkers were attacking its soundness. The strongest attack was made by the philosopher Bishop George Berkeley (1685–1753), who feared the growing threat to religion of the mathematically inspired philosophy of mechanism and determinism. In 1734 he published *The Analyst Or a Discourse Addressed to an Infidel Mathematician.* [The infidel was Edmund Halley.] *Wherein It Is Examined Whether the Object, Principles, and Inferences of the Modern Analysis Are More Distinctly Conceived, or More Evidently Deduced, than Religious Mysteries and Points of Faith. "First Cast the Beam Out of Thine Own Eye; and Then Shalt Thou See Clearly To Cast Out the Mote Out of Thy Brother's Eye."* Berkeley rightly complained that the mathematicians were proceeding mysteriously and incomprehensibly: they did not give the logic or reasons for their steps. Berkeley criticized many arguments of Newton, and, in particular, he pointed out that Newton in his paper "Quadrature of Curves" (using x for the increment which we have denoted by h) performed some algebraic steps, and then dropped terms involving h because h was now 0. (Compare equation (4) above.) This, Berkeley said, was a defiance of the law of contradiction. Such reasoning would not be allowed in theology. He

* An account of these books can be found in Florian Cajori: *A History of the Conceptions of Limits and Fluxions in Great Britain from Newton to Woodhouse,* The Open Court Publishing Co., Chicago, 1915. Also Carl Boyer: *The Concepts of the Calculus,* reprint by Dover Publications, 1949; original edition, Columbia University Press, 1939.

the work of the latter part of the 19th century that would justify far more the name Age of Reason (Chapter VIII).

While most mathematicians were content to pursue innovations without much concern for proof, a few of the leading mathematicians became alarmed by the illogical state of mathematics. The desperateness of the situation in analysis was underscored by the brilliant, precocious Norwegian Niels Henrick Abel (1802–1829) in a letter of 1826 to Professor Christoffer Hansteen. He complained about:

> the tremendous obscurity which one unquestionably finds in analysis. It lacks so completely all plan and system that it is peculiar that so many men have studied it. The worst of it is, it has never been treated stringently. There are very few theorems in advanced analysis which have been demonstrated in a logically tenable manner. Everywhere one finds this miserable way of concluding from the special to the general and it is extremely peculiar that such a procedure has led to so few of the so-called paradoxes.

Apropos of divergent series in particular, Abel wrote in January 1826 to his former teacher Berndt Holmböe:

> The divergent series are the invention of the devil, and it is a shame to base on them any demonstration whatsoever. By using them one may draw any conclusion he pleases and that is why these series have produced so many fallacies and so many paradoxes. . . . I have become prodigiously attentive to all this, for with the exception of the geometrical series, there does not exist in all of mathematics a single infinite series the sum of which has been determined rigorously. In other words, the things which are most important in mathematics are also those which have the least foundation. That most of these things are correct in spite of that is extraordinarily surprising. I am trying to find the reason for this; it is an exceedingly interesting question.

Among people at large, some are not content to drown their sorrows in alcohol. So among mathematicians, some were not content to drown their concern about the illogical state of mathematics in the inebriation of physical successes. Whatever solace these more courageous men may have derived from the belief that they were uncovering pieces of God's design was nullified by the late-18th-century abandonment of that belief (Chapter IV). Having lost that support they felt obliged to reexamine their work, and they faced the vagueness, the lack of proofs, the inadequacies in the existing proofs, the contradictions, and the sheer confusion about what was correct in what had been created. These men realized that mathematics had not been the paradigm of reason it was reputed to be. In place of reason, it was intuition, geometrical diagrams, physical arguments, ad hoc principles such as the principle of permanence of form, and the recourse to metaphysics that justified what had been accepted.

The ideal of a logical structure had certainly been made clear and proclaimed by the Greeks. And so the few mathematicians who undertook to achieve it in arithmetic, algebra, and analysis were buoyed up in their efforts by the belief that mathematicians had practically done so in at least one highly significant case, Euclidean geometry. They thought that if some had scaled Olympus once, others might scale it again. What these men did not foresee was that the task of supplying rigorous foundations for all of the existing mathematics would prove to be far more difficult and subtle than any mathematician of 1850 could possibly have envisioned. Nor did they foresee the additional troubles that were to ensue.