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(c) H' is **not** Riemann integrable on any closed interval $[a, b]$, for assume that it is. Then H' is continuous a.e. on $[a, b]$. But it is clear that $H'(t) = 0$ if H' is continuous at t, and so $H' = 0$ a.e. on $[a, b]$. It follows from (b) that H is a constant on $[a, b]$—a palpable contradiction.

(d) H' is of Baire class one, being the pointwise limit of the continuous functions $H_n(x) = n[H(x + 1/n) - H(x)]$, and so the set of points at which H' is continuous is residual; i.e., its complement is of first category.

(e) Write $A = \{x: H'(x) > 0\}$ and $B = \{x: H'(x) < 0\}$. Thus $A \cap I$ and $B \cap I$ both have positive Lebesgue measure for every interval I. In fact, assuming that there exists some interval $I = [a, b]$ such that $B \cap I$ has measure zero, it follows that $H' \geqq 0$ a.e. on I. Therefore, since

$$H(x) = \int_a^x H'(t)dt + H(a)$$

for all $x \in I$, we conclude that H is nondecreasing on I—a contradiction. Similarly, if $A \cap I$ had measure zero, then H would be nonincreasing on I.

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IS MATHEMATICAL TRUTH TIME-DEPENDENT?

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1. Introduction. Is mathematical truth time-dependent? Our immediate impulse is to answer no. To be sure, we acknowledge that standards of truth in the natural sciences have undergone change; there was a Copernican revolution in astronomy, a Darwinian revolution in biology, an Einsteinian revolution in physics. But do scientific revolutions like these occur in mathematics? Mathematicians have most often answered this question as did the nineteenth-century mathematician Hermann Hankel, who said, "In most sciences, one generation tears down what another has built, and what one has established, the next undoes. In mathematics alone, each generation builds a new story to the old structure." [**20**, p. 25.]

Hankel's view is not, however, completely valid. There have been several major upheavals in mathematics. For example, consider the axiomatization of geometry in ancient Greece, which transformed mathematics from an experimental science into a wholly intellectual one. Again, consider the discovery of non-Euclidean geometries and non-commutative algebras in the nineteenth century; these developments led to the realization that mathematics is not about anything in particular; it is instead the logically connected study of abstract systems. These were revolutions in thought which changed mathematicians' views about the nature of mathematical truth, and about what could or should be proved.

Another such mathematical revolution occurred between the eighteenth and nineteenth centuries, and was focussed primarily on the calculus. This change was a rejection of the mathematics of powerful techniques and novel results in favor of the mathematics of clear definitions and rigorous proofs. Because this change, however important it may have been for mathematicians themselves, is not often discussed by historians and philosophers, its revolutionary character is not widely understood. In this paper, I shall first try to show that this major change did occur. Then, I shall investigate what brought it about. Once we have done this, we can return to the question asked in the title of this paper.

2. Eighteenth-century analysis: practice and theory. To establish what eighteenth-century mathematical practice was like, let us first look at a brilliant derivation of a now well-known result. Here is how Leonhard Euler derived the infinite series for the cosine of an angle. He began with the identity

$$(\cos z + i \sin z)^n = \cos nz + i \sin nz.$$

He then expanded the left-hand side of the equation according to the binomial theorem. Taking the real part of that binomial expansion and equating it to cos nz, he obtained

$$\cos nz = (\cos z)^n - \frac{n(n-1)}{2!}(\cos z)^{n-2}(\sin z)^2 + \frac{n(n-1)(n-2)(n-3)}{4!}(\cos z)^{n-4}(\sin z)^4 - \cdots.$$

Let z be an infinitely small arc, and let n be infinitely large. Then:

$$\cos z = 1,\ \sin z = z,\ n(n-1) = n^2,\ n(n-1)(n-2)(n-3) = n^4, \text{ etc.}$$

The equation now becomes recognizable:

$$\cos nz = 1 - \frac{n^2z^2}{2!} + \frac{n^4z^4}{4!} - \cdots.$$

But since z is infinitely small and n infinitely large, Euler concludes that nz is a finite quantity. So let $nz = v$. The modern reader may be left slightly breathless; still, we have

$$\cos v = 1 - \frac{v^2}{2!} + \frac{v^4}{4!} - \cdots.$$

(See [**16**, sections 133–4] and [**32**, pp. 348–9].)

Now that we have worked through one example, we shall be able to appreciate some generalizations about the way many eighteenth-century mathematicians worked. First, the primary emphasis was on getting results. All mathematicians know many of the results from this period, results which bear the names of Leibniz, Bernoulli, L'Hospital, Taylor, Euler, and Laplace. But the chances are good that these results were originally obtained in ways utterly different from the ways we prove them today. It is doubtful that Euler and his contemporaries would have been able to derive their results if they had been burdened with our standards of rigor. Here, then, is one major difference between the eighteenth-century way of doing mathematics and our way.

What led eighteenth-century mathematicians to think that results might be more important than rigorous proofs? One reason is that mathematics participated in the great explosion in science known as the Scientific Revolution [**19**]. Since the Renaissance, finding new knowledge had been a major goal of all the sciences. In mathematics, ever since the first major new result — the solution to the cubic equation published in 1545 — increasing mathematical knowledge had meant finding new results. The invention of the calculus at the end of the seventeenth century intensified the drive for results; here was a powerful new method which promised vast new worlds to conquer. One can imagine few more exciting tasks than trying to solve the equations of motion for the whole solar system. The calculus was an ideal instrument for deriving new results, even though many mathematicians were unable to explain exactly why this instrument worked.

If the overriding goal of most eighteenth-century mathematics was to get results, we would expect mathematicians of the period to use those methods which produced results. For eighteenth-century mathematicians, the end justified the means. And the successes were many. New subjects arose in the eighteenth century, each with its own range of methods and its own domain of results: the calculus of variations, descriptive geometry, and partial differential equations, for instance. Also, much greater sophistication was achieved in existing subjects, like mathematical physics and probability theory.

The second generalization we shall make about eighteenth-century mathematics and its drive for results is that mathematicians placed great reliance on the power of symbols. Sometimes it seems to have been assumed that if one could just write down something which was symbolically coherent, the truth of the statement was guaranteed. And this assumption was not applied to finite formulas only. Finite methods were routinely extended to infinite processes. Many important facts about infinite power series were discovered by treating the series as very long polynomials [**30**].

This trust in symbolism in the eighteenth century is somewhat anomalous in the history of mathematics, and needs to be accounted for. It came both from the success of algebra and the success of the calculus. Let us first consider algebra.

General symbolic notation of the type we now take for granted was introduced in 1591 by the French mathematician François Viète [**6**, pp. 59–65] and [**32**, pp. 74–81]. This notation proved to be the greatest instrument of discovery in the history of mathematics. Let us illustrate its power by one example. Consider the equation

$$(x-a)(x-b)(x-c) = x^3 - (a+b+c)x^2 + (ab+ac+bc)x - abc. \tag{2.1}$$

Symbolic notation lets you discover what dozens of numerical examples may not: the relation between the roots and the coefficients of any polynomial equation of any degree. Equation (2.1), furthermore, has degree three, and has three roots. Relying on results like (2.1), Albert Girard in 1629 stated that an nth degree equation had n roots — the first formulation of what Gauss later called the Fundamental Theorem of Algebra.

But why are algebraic formulas like (2.1) considered true by eighteenth-century mathematicians? Because, as Newton put it, algebra is just a "universal arithmetic" [**29**]. Equation (2.1) is valid because it is a generalization about valid arithmetical statements. What, then, about infinite arguments, like the one of Euler's we examined earlier? The answer is analogous. Just as there is an arithmetic of infinite decimal fractions, we may generalize and create an algebra of infinite series [**28**, p. 6]. Infinite processes are like finite ones — except that they take longer.

The faith in symbolism nourished by algebra was enhanced further by the success of the calculus. Leibniz had invented the notations dy/dx and $\int ydx$ expressly to help us do our thinking. The notation serves this function well; we owe a debt to Leibniz every time we change variables under the integral sign. Or, suppose y is a function of x and that x is a function of t; we want to know dy/dt. It is not Leibniz, but Leibniz's notation that discovers the chain rule:

$$dy/dt = (dy/dx)\,(dx/dt).$$

The success of Leibniz's notation for the calculus reinforced mathematicians' belief in the power of symbolic arguments to give true conclusions.

In the eighteenth century, belief in the power of good notation extended beyond mathematics. For instance, it led the chemist Lavoisier to foresee a "chemical algebra," in the spirit of which Berzelius in 1813 devised chemical symbols essentially like those we use today. Anybody who has balanced chemical equations knows how the symbols do some of the thinking for us. The fact that the idea of the validity of purely symbolic arguments spread from mathematics to other areas shows us how prevalent an idea it must have been.

What has been said so far should not lead the reader to believe that eighteenth-century mathematicians were completely indifferent to the foundations of analysis. They certainly discussed the subject, and at length. I shall not here summarize the diverse eighteenth-century attempts to explain the nature of dy/dx, of limits, of the infinite, and of integrals, during a century that Carl Boyer has rightly called "the period of indecision" as far as foundations were concerned [**7**, Chapter VI]. What must be emphasized for our present purposes is that discussions of foundations were not the basic concern of eighteenth-century mathematicians. That is, discussions of foundations do not generally appear in research papers in scientific journals; instead, they are relegated to Chapter I of textbooks, or found in popularizations. More important, the practice of mathematics did not depend on a perfect understanding of the basic concepts used. But this was no longer the situation in nineteenth-century mathematics, and, of course, is not the situation today.

Nineteenth-century analysts, beginning with Cauchy and Bolzano, gave rigorous, inequality-based treatments of limit, convergence, and continuity, and demanded rigorous proofs of the theorems about these concepts. We know what these proofs were like; we still use them. This new direction in nineteenth-century analysis is not just a matter of differences in technique. It is a major change in the way mathematics was looked at and done. Now that we have sketched the eighteenth-century approach, we are ready to deal with what are — from the historical point of view — the most interesting questions of this paper. What made the change between the old and new views occur? How did mathematics get to be the way it is now?

Two things were necessary for the change. Most obviously, the techniques needed for rigorous proofs had to be developed. We shall discuss the history of some major techniques in Section 4, below. But also, there had to be a change in attitude. Without the techniques, of course, the change in attitude could never have borne fruit. But the change in attitude, though not sufficient, was a necessary condition for the establishment of rigor. Our next task, accordingly, will be to explain the change in attitude toward the foundations of the calculus between the eighteenth and nineteenth centuries. Did the very nature of mathematics force this change? Or was it motivated by factors outside of mathematics? Let us investigate various possibilities.

3. Why did standards of mathematical truth change? The first explanation which may occur to us is like the one we use to justify rigor to our students today: the calculus was made rigorous to avoid errors, and to correct errors already made. But this is not quite what happened. In fact, there are surprisingly few mistakes in eighteenth-century mathematics. There are two main reasons for this. First, some results could be verified numerically, or even experimentally; thus, their validity could be checked without a rigorous basis. Second, and even more important, eighteenth-century mathematicians had an almost unerring intuition. Though they were not guided by rigorous definitions, they nevertheless had a deep understanding of the properties of the basic concepts of analysis. This conclusion is supported by the fact that many apparently shaky eighteenth-century arguments can be salvaged, and made rigorous by properly specifying hypotheses. Nevertheless, we must point out that the need to avoid errors became more important near the end of the eighteenth century, when there was increasing interest among mathematicians in complex functions, in functions of several variables, and in trigonometric series. In these subjects, there are many plausible conjectures whose truth is relatively difficult to evaluate intuitively. Increased interest in such results may have helped draw attention to the question of foundations.

A second possible explanation which may occur to us is that the calculus was made rigorous in a spirit of generalization. The eighteenth century had produced a mass of results. The need to unify such a mass of results could have led automatically to a rigorous, axiomatic basis. But there had been large numbers of results for a hundred years before Cauchy's work. Besides, unifying results does not always make them rigorous; moreover, the function of rigor is not just to unify, but to prove. Still, there is something to be said for the hypothesis that the calculus became rigorous partly to unify the wealth of existing results. At the end of the eighteenth century, several mathematicians thought that the pace of getting new results was decreasing. This feeling had some basis in fact; most of the results obtainable by the routine application of eighteenth-century methods had been obtained. Perhaps, if progress was slowing, it was time to sit back and reflect about what had been done [**31**, pp. 136–7]. This feeling helped get some mathematicians interested in the question of rigor.

A third possible explanation depends on the prior existence of rigor in geometry. Everybody from the Greeks on knew that mathematics was supposed to be rigorous. One might thus assume that mathematicians' consciences began to trouble them, and that as a result analysts returned their new methods to the old standards. In fact, Euclidean geometry did provide a model for the new rigor. But the old ideas of rigor were not enough in themselves to make mathematicians strive to make the calculus rigorous — as the hundred and fifty years from Newton to Cauchy shows. This is true even though the discrepancy between Euclidean standards and the actual practice of eighteenth-century mathematicians did not go unnoticed. George Berkeley, Bishop of Cloyne, attacked the calculus in 1734, on the perfectly valid grounds that it was not rigorous the way mathematics was supposed to be. Berkeley wanted to defend religion against the attacks of unreasonableness levelled against it by eighteenth-century scientists and mathematicians. Berkeley said that his opponents did not even reason well about mathematics. He conceded that the results of the calculus were valid, but attacked its methods. Berkeley's attack, *The Analyst*, is a masterpiece of polemics [**32**, pp. 333–338] and [**3**]. He said of the "vanishing increments" that played so crucial a role in Newton's calculus, "And what are these ...vanishing increments? They are neither finite quantities, nor quantities infinitely small, nor yet nothing. May we not call them the ghosts of departed quantities?" Berkeley's attack — which included point-by-point mathematical criticisms of some basic arguments of Newton's calculus — provoked a number of mathematicians to write refutations. However, neither Berkeley's attack nor the replies to it produced the change in attitude toward rigor which we are trying to explain. First of all, the replies are not very convincing [**8**]. Besides, the subject of foundations was still not considered serious mathematics. Berkeley did get people thinking, more than they would have without him, about the problem of foundations. The discussions of foundations by Maclaurin, D'Alembert, and Lagrange were all at least somewhat influenced by Berkeley's work. Nevertheless, Berkeley's attack in itself was not enough to cause foundations to become a major mathematical concern.

In bringing about the change, there is one other factor which, though seldom mentioned in this connection, was important: the mathematician's need to teach. Near the end of the eighteenth century, a major social change occurred. Before the last decades of the century, mathematicians were often attached to royal courts; their job was to do mathematics and thus add to the glory, or edification, of their patron. But almost all mathematicians since the French Revolution have made their living by teaching [**31**, p. 140] [**2**, p. 95,108].

This change in the economic circumstances of mathematicians had other causes than the decline of particular royal courts. In the eighteenth century, science was expanding. This was the "age of Newton" and the success of Newtonian science. Governments and businessmen felt that science was important and could be useful; scientists encouraged them in these beliefs. So governments founded educational institutions to promote science. Military schools were founded to provide prospective officers with knowledge of applied science. New scientific chairs were endowed in existing universities. By far the most important new institution for scientific instruction, one which served as a model to several nations in the nineteenth century, was the *École polytechnique* in Paris, founded in 1795 by the revolutionary government in France.

Why might the new economic circumstances of mathematicians — the need to teach — have helped promote rigor? Teaching always makes the teacher think carefully about the basis for the subject. A mathematician could understand enough about a concept to use it, and could rely on the insight he had gained through his experience. But this does not work with freshmen, even in the eighteenth century. Beginners will not accept being told, "After you have worked with this concept for three years, you'll understand it."

What is the evidence that teaching helped motivate eighteenth and nineteenth century mathematicians to make analysis rigorous? First, until the end of the eighteenth century, most work on foundations did not appear in scientific journals, apparently because foundations were not considered to pose major mathematical (as opposed to philosophical) questions. Instead, such work appeared in courses of lectures, in textbooks, or in popularizations. Even in the nineteenth century, when foundations had been established as essential to mathematics, their origin was often in teaching. The work on foundations of analysis of Lagrange [**23, 26**], of Cauchy [**10, 11**], of Weierstrass [**21**, pp. 283–4] [**7**, pp. 284–7], and of Dedekind [**14**, p. 1], all originated in courses of lectures.

Each of the points we have made so far helps explain what motivated mathematicians to shift from the result-oriented view of the eighteenth century to the more rigorous standards of the nineteenth. One more catalyst of the change should be identified: Joseph-Louis Lagrange. Lagrange's own interest in the problem of foundations was first engaged by having to teach the calculus at the military school in Turin [**24**]. In 1784, by proposing the foundations of the calculus as a prize problem for the Berlin Academy of Sciences, he stimulated the first major book-length contributions to foundations of the calculus written on the Continent. (see [**27**] [**9**] [**7**, p. 254–255] and [**18**, pp. 149–150]). Above all, Lagrange's lectures at the *École polytechnique*, published in two widely influential books, attempted to give a general and algebraic framework for the calculus [**26**] [**23**]. Lagrange did not correctly solve the problem of foundations — we can no longer accept his *definition* of $f'(x)$ as the coefficient of h in the Taylor series expansion of $f(x + h)$. Nevertheless, his vision of reducing the calculus to algebra decisively influenced the work of Bolzano [**5**] and — as we shall see — of Cauchy.

The change in attitude we have been discussing was not enough in itself to establish rigor in the calculus — as the example of Lagrange shows. Having decided that we want to make a subject rigorous, what else do we need? Two more things are required: the right definitions, and techniques of proof to derive the known results from the definitions. We must now answer another question: where did the required definitions and proofs come from?

Eighteenth-century mathematicians themselves had developed many of the techniques, and isolated many of the basic defining properties — even though they did not know that this is what they were doing. It is amazing that so many of the techniques used by Cauchy in rigorous arguments had been around for so long. This fact shows that a real change in point of view was required for the rigorization of analysis; it was not an automatic development out of eighteenth-century mathematics.

4. The eighteenth-century origins of nineteenth-century rigor. We shall illustrate the eighteenth-century origins of nineteenth-century rigor by giving several examples of eighteenth-century work which was transformed into nineteenth-century definitions and proofs. The principal area of eighteenth-century mathematics we shall investigate is the study of approximations. Eighteenth-century mathematicians, whether solving algebraic equations or differential equations, developed many useful approximation methods. When the goal is results, an approximate result is better than nothing. Paradoxically, eighteenth-century mathematicians were most exact when they were being approximate; their work with inequalities in approximations later became the basis for rigorous analysis.

We shall discuss two classes of eighteenth-century approximation work: the actual working out of approximation procedures, and the computation of error estimates. Let us see what use nineteenth-century analysts made of these.

One new way in which nineteenth-century mathematicians looked at eighteenth-century approximations was to see the approximate solution as a construction of that solution, and therefore as a proof of its existence. For instance, Cauchy did this in developing what is now called the Cauchy-Lipschitz method of proving the existence of the solution to a differential equation; the proof is based on an approximation method developed by Euler [**15**, pp. 424–5] [**12**, p. 399 ff]. Similarly Cauchy's elegant proof of the intermediate-value theorem for continuous functions was based on an eighteenth-century approximation method [**22**, pp. 260–1] [**25**, sections 2,6] [**10**, pp. 378–80]. For a continuous function $f(x)$, Cauchy took $f(a)$ and $f(b)$ of opposite sign, divided the interval $[a, b]$ into n parts, and concluded that there were at least two values of x on $[a, b]$, differing by $(b - a)/n$, which yielded opposite sign for $f(x)$. He then repeated the procedure on the interval between these two new values, on an interval of length $(b - a)/n$, which gives two more values, differing by $(b - a)/n^2$, and so on. Where Lagrange had used this technique to approximate to the root ξ of a polynomial included between $x = a$ and $x = b$, Cauchy used it to argue for the existence of the number ξ as the common limit of the sequences of values of x which gave positive sign for f, and negative sign for f. The origin of Cauchy's proof in algebraic approximations is further demonstrated by the context in which he gave it: a "*Note*" devoted to discussing the approximate solution of algebraic equations [**10**, p. 378 ff].

Another example of the conversion of approximations into existence proofs is given by Cauchy's theory of the definite integral. In the eighteenth century, it was customary to define the integral as the inverse of the derivative. It was known, however, that the value of the integral could be approximated by a sum. Cauchy took Euler's work on approximating the values of definite integrals by sums [**15**, pp. 184–7], and looked at it from an entirely new point of view. Cauchy *defined* the definite integral as the limit of a sum, proved the existence of the definite integral of a continuous (actually, uniformly continuous) function, and then used his definition to prove the Fundamental Theorem of Calculus [**11**, pp. 122–5, 151–2].

Now let us consider another type of result in eighteenth-century approximations: approximations given along with an error estimate. These results took a form like this: given some n, the mathematician could compute an upper bound on the error made in taking the nth approximation for the true value. Near the end of the eighteenth century, the algebra of inequalities was exploited with great skill in computing such error estimates [**13**, pp. 171–183] and [**25**, pp. 46–7, p. 163]. Cauchy, Abel, and their followers turned the approximating process around. Instead of being given n and finding the greatest possible error, we are *given* what is in effect the "error"—epsilon—and, provided that the process converges, we can always find n such that the error of the nth approximation is less than epsilon. (This seems to be the reason for the use of the letter "epsilon" in its usual modern sense by Cauchy [**10**, pp. 64–5 *et passim*].) [**1**] [**10**, pp. 400–415]. Cauchy's definition of convergence—which is essentially ours—is based on this principle [**10**, Chapter VI].

Another way in which nineteenth-century mathematicians changed eighteenth-century views of results using inequalities was to take facts known to eighteenth-century mathematicians in special cases and to make them legitimate in general. For instance, D'Alembert and others had shown that some particular series converged by showing that they were, term-by-term, less than a convergent geometric progression [**13**]. Gauss in 1813 used this criterion to investigate, in a rigorous manner, the convergence of the hypergeometric series [**17**]. Cauchy used the comparison of a given series with a geometric one to derive and to prove some general tests for the convergence of any series; the ratio test, the logarithm test, and the root test [**10**, pp. 121–127].

Let us look at one last example — a very important one — of an eighteenth-century result which became something different in the nineteenth century: the property of the derivative expressed by

$$f(x+h) = f(x) + hf'(x) + hV, \tag{4.1}$$

where V goes to zero with h. As we have remarked, Lagrange had defined $f'(x)$ as the coefficient of h in the Taylor expansion of $f(x+h)$. He then "derived" (4.1) from that Taylor series expansion, considering V to be a convergent infinite series in h. Lagrange used (4.1) to investigate many properties of the derivative. To do this, he interpreted "V goes to zero with h" to mean that, for any given quantity D, we can find h sufficiently small so that $f(x+h) - f(x)$ "will be included between" $h[f'(x) - D]$ and $h[f'(x) + D]$ [**23**, p. 87]. First Cauchy, and then Bolzano and Weierstrass, made (4.1) and its associated inequalities into the *definition* of $f'(x)$. (Cauchy's definition was actually verbal, but he translated it into the language of inequalities in proofs.) [**11**, pp. 44–5; 122–3], [**4**, Chapter 2] and [**7**, pp. 285–7]. This definition made legitimate the results about $f'(x)$ that Lagrange had derived from (4.1) — for instance, the mean-value theorem for derivatives. (Except, we must note, for a few errors, especially the confusion between convergence and uniform convergence, which was not cleared up until the 1840's.)

Of course, we do not mean to imply that Gauss, Cauchy, Bolzano, Abel, and Weierstrass were not original, creative mathematicians. They were. To show that major changes in point of view occur in mathematics, we have concentrated in this section on what these men owed to eighteenth-century techniques. But, besides transforming what they borrowed, they contributed much of their own that was new. Cauchy, in particular, devised beautiful proofs about convergent power series in real and complex variables, about real and complex integrals, and, of course, contributed to a variety of subjects besides analysis. Nevertheless, for our present purposes, we need the biassed sample we have chosen — things accomplished either by taking what the eighteenth century knew for particular cases and making it general, or by taking what the eighteenth century had derived for one purpose and putting it to a more profound use.

Much effort was needed to transform eighteenth-century techniques in the ways we have discussed. But it was more than just a matter of effort. It took asking the right questions *first*; and then using — and expanding — the already existing techniques to answer them. It took — and was — a major change in point of view. The reawakening of interest in rigor was just as necessary as the availability of techniques to produce the point of view of Bolzano and Cauchy—the point of view which has been with us ever since. Mathematics requires not only results, but clear definitions and rigorous proofs. Individual mathematicians may still concentrate on the creation of fruitful methods and ideas to be exploited, but the mathematical community as a whole can no longer be indifferent to rigor.

5. Conclusion. We began by asking whether mathematical truth was time-dependent. Perhaps mathematical truth is eternal, but our knowledge of it is not. We have now seen an example of how attitudes toward mathematical truth have changed in time. After such a revolution in thought, earlier work is re-evaluated. Some is considered worth more; some, worth less.

What should a mathematician do, knowing that such re-evaluations occur?

Three courses of action suggest themselves. First, we can adopt a sort of relativism which has been expressed in the phrase "Sufficient unto the day is the rigor thereof." Mathematical truth is just what the editors of the *Transactions* say it is. This is a useful view at times. But this view, if universally adopted, would mean that Cauchy and Weierstrass would never have come along. Unless there were the prior appearance of major errors, standards could never improve in any important way. So the attitude of relativism, which would have counselled Cauchy to leave foundations alone, will not suffice for us.

Second, we can attempt to set the highest conceivable standard: never use an argument in which we do not completely understand what is going on, dotting all the *i*'s and crossing all the *t*'s. But this is even worse. Euler, after all, knew that there were problems in dealing with infinitely large and infinitely small quantities. According to this high standard, which textbooks sometimes urge on students, Euler would never have written a line. There would have been no mathematical structure for Cauchy and Weierstrass to make rigorous.

So I suggest a third possibility: a recognition that the problem I have raised is just the existential situation mathematicians find themselves in. Mathematics grows in two ways: not only by successive increments, but also by occasional revolutions. Only if we accept the possibility of present error can we hope that the future will bring a fundamental improvement in our knowledge. We can be consoled that most of the old bricks will find places somewhere in the new structure. Mathematics is *not* the unique science without revolutions. Rather, mathematics is that area of human activity which has at once the least destructive and still the most fundamental revolutions.

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