Why be so Critical? Nineteenth Century Mathematics and the Origins of Analysis

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One striking feature of nineteenth century mathematics, as contrasted with that of previous eras, is the higher degree of rigor and precision demanded by its practitioners. This tendency was especially noticeable in *analysis*, a field of mathematics that essentially began with the "invention" of calculus by Leibniz and Newton in the mid-17th century. Unlike the calculus studied in an undergraduate course today, however, the calculus of Newton, Leibniz and their immediate followers focused entirely on the study of geometric *curves*, using algebra (or 'analysis') as an aid in their work. This situation changed dramatically in the 18th century when the focus of calculus shifted instead to the study of *functions*, a change due largely to the influence of the Swiss mathematician and physicist Leonhard Euler (1707–1783). In the hands of Euler and his contemporaries, functions became a powerful problem solving and modelling tool in physics, astronomy, and related mathematical fields such as differential equations and the calculus of variations. Why then, after nearly 200 years of success in the development and application of calculus techniques, did 19th-century mathematicians feel the need to bring a more critical perspective to the study of calculus? This project explores this question through selected excerpts from the writings of the 19th century mathematicians who led the initiative to raise the level of rigor in the field of analysis.

1 The Problem with Analysis: Bolzano, Cauchy and Dedekind

To begin to get a feel for what mathematicians felt was wrong with the state of analysis at the start of the 19th century, we will read excerpts from three well-known analysts of the time: Bernard Bolzano (1781–1848), Augustin-Louis Cauchy (1789–1857) and Richard Dedekind (1831–1916). In these excerpts, these mathematicians expressed their concerns about the relation of calculus (analysis) to geometry, and also about the state of calculus (analysis) in general. As you read what they each had to say, consider how their concerns seem to be the same or different. The project questions that follow these excerpts will then ask you about these comparisons, and also direct your attention towards certain specific aspects of the excerpts.[1]

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[1]To set them apart from the project narrative, all original source excerpts are set in sans serif font and bracketed by the following symbol at their beginning and end: ∞∞∞

Bernard Bolzano, 1817, Rein analytischer Beweis des Lehrsatzes, dass zwischen je zwey Werthen, die ein entgegengesetzes Resultat gewähren, wenigstens eine reele Wurzel der Gleichung liege (Purely analytic proof of the theorem that between any two values which give results of opposite sign there lies at least one real root of the equation)[2]

There are two propositions in the theory of equations of which it could still be said, until recently, that a completely correct proof was unknown. One is the proposition: *that between any two values of the unknown quantity which give results of opposite signs there must always lie at least one real root of the equation*. The other is: *that every algebraic rational integral function of one variable quantity can be divided into real factors of first or second degree*. After several unsuccessful attempts by d'Alembert, Euler, de Foncenex, Lagrange, Laplace, Klügel, and others at proving the latter proposition Gauss finally supplied, last year, two proofs which leave very little to be desired. Indeed, this outstanding scholar had already presented us with a proof of this proposition in 1799, but it had, as he admitted, the defect that it proved a purely analytic truth on the basis of a geometrical consideration. But his two most recent proofs are quite free of this defect; the trigonometric functions which occur in them can, and must, be understood in a purely analytic sense.

The other proposition mentioned above is not one which so far has concerned scholars to any great extent. Nevertheless, we do find mathematicians of great repute concerned with the proposition, and already different kinds of proof have been attempted. To be convinced of this one need only compare the various treatments of the proposition which have been given by, for example, Kästner, Clairaut, Lacroix, Metternich, Klügel, Lagrange, Rösling, and several others.

However, a more careful examination very soon shows that none of these proofs can be viewed as adequate. The most common kind of proof depends on a truth borrowed from *geometry*, namely, *that every continuous line of simple curvature of which the ordinates are first positive and then negative (or conversely) must necessarily intersect the x-axis somewhere at a point that lies in between those ordinates*. There is certainly no questions concerning the correctness, nor the indeed the obviousness, of this geometrical proposition. But it is clear that it is an intolerable offense against correct method to derive truths of pure (or general) mathematics (i.e., arithmetic[3], algebra, analysis) from considerations which belong to a merely applied (or special) part, namely, *geometry*. [...]

[2]The translation of Bolzano's paper used in this project is taken from [8].

[3]As was not uncommon in the nineteenth century, Bolzano's use of the word 'arithmetic' here referred to the mathematical discipline that is today called 'number theory.'

Augustin Cauchy, 1821, Cours d'Analyse (Course on Analysis)[4]

> As for the methods [in this text], I have sought to give them all the rigour that is demanded in geometry, in such a way as never to refer to reasons drawn from the generality of algebra. . . . . . One should also note that [reasons drawn from the generality of algebra] tend to cause an indefinite validity to be attributed to the algebraic formulae, even though, in reality, the majority of these formulae hold only under certain conditions, and for certain values of the variables which they contain. By determining these conditions and values, and by fixing precisely the meaning of the notations of which I make use, I remove any uncertainty; . . .

Augustin Cauchy, 1823, Résumé des leçons sur le calcul infinitésimal (Summary of lessons on the infinitesimal calculus)

> My principal aim has been to reconcile rigor, which I took as a law in my *Cours d'Analyse*, with the simplicity that results from the direct consideration of infinitesimals. For this reason, I believed I should reject the expansion of functions by infinite series whenever the series obtained was divergent; and I found myself forced to defer Taylor's formula until the integral calculus, [since] this formula can not be accepted as general except when the series it represents is reduced to a finite number of terms, and completed with [a remainder given by] a definite integral. I am aware that [Lagrange] used the formula in question as the basis of his theory of derivative functions. However, despite the respect commanded by such a high authority, most geometers[5] now recognize the uncertainty of results to which one can be led by the use of divergent series; and we add further that, in some cases, Taylor's theorem seems to furnish the expansion of a function by a convergent series, even though the sum of that series is essentially different from the given function.

[4] The English translation of the two Cauchy excerpts used in this project are due to the project author.

[5] The meaning of the word 'geometer' also changed over time; in Cauchy's time, this word referred to any mathematician (and not just someone who worked in geometry.)

Richard Dedekind, 1872, Stetigkeit und irrationale Zahlen (Continuity of irrational numbers)[6]

> My attention was first directed toward the considerations which form the subject of this pamphlet in the autumn of 1858. As professor in the Polytechnic School in Zürich I found myself for the first time obliged to lecture upon the elements of the differential calculus and felt more keenly than ever before the lack of a really scientific foundation for arithmetic[7]. In discussing the notion of the approach of a variable magnitude to a fixed limiting value, and especially in proving the theorem that every magnitude which grows continually but not beyond all limits, must certainly approach a limiting value, I had recourse to geometric evidences. Even now such resort to geometric intuition in a first presentation of the differential calculus, I regard as exceedingly useful, from the didactic standpoint, and indeed indispensable, if one does not wish to lose too much time. But that this form of introduction into the differential calculus can make no claim to being scientific, no one will deny. For myself this feeling of dissatisfaction was so overpowering that I made the fixed resolve to keep meditating on the question until I should find a purely arithmetic and perfectly rigorous foundation for the principles of infinitesimal analysis.

Question 1

In what way do the concerns of these three mathematicians about the relation of calculus (analysis) to geometry, and about the state of calculus (analysis) in general, seem to be the same/different?

[6] The translation of Dedekind's text used in this project is taken from [7].

[7] Unlike Bolzano's use of the word 'arithmetic' to mean 'number theory', Dedekind's use of the expression 'scientific foundation for arithmetic' was related to the set of real numbers and its underlying structure.

Question 2

This question looks at some of the mathematical results mentioned by Bolzano, Cauchy and Dedekind.

(a) Note that:

- Bolzano discussed two specific theorems — identify or write these theorems here:

- Dedekind discussed one specific theorem — identify or write that theorem here:

- Cauchy made reference to the Taylor formula and related results — look back to see what he has to say, and briefly describe his concerns.

(b) Which of the results in part (a) are familiar to you?
For each that is, try to state it in "modern" terms, or give its "modern name".

(c) Which of the results in part (a), if any, do you believe to be true (and why)?

2 Niels Abel: *Hold your laughter, friends!*

In this section, we will examine an excerpt from a letter written by young Norwegian mathematician Niels Abel (1802–1829) to his high school teacher, Bernt Michael Holmboe, on January 26, 1826. Abel is often remembered for his celebrated impossibility proof in the theory of equations in which he proved that a 'quintic formula' for the general fifth degree polynomial equation does not exist — a proof that marked an important step in the mathematical quest for algebraic solutions to polynomial equations which began with the development of Babylonian procedures for solving quadratic equations in 1700 BCE. Abel is equally well known for his work in analysis, and especially the theory of elliptic functions. In his letter to Holmboe, written during a study-abroad trip to Paris and Berlin, Abel described some of his concerns about the state of analysis in general, and particularly about the use of infinite series. The **letter itself (in English translation) appears on pages 8 – 9** of this project; after reading it, complete your responses to questions 3 – 6 below.

Question 3
Find at least two references in Abel's letter to infinite series as an important concept or issue in mathematics.

To what degree do the concerns that Cauchy expressed about series agree with Abel's view of series?

Question 4

What was it that Abel thought was “exceedingly surprising” about the “current” state of mathematics? Be specific here!

Do you agree with his reaction to this state of affairs? Explain.

Question 5

Towards the end of this excerpt, Abel remarked that a series of the following form can be convergent for ‘x less than 1’, but divergent for $x = 1$:

$$\phi(x) = a_0 + a_1 x + a_2 x^2 + \dots$$

(a) Provide an example in which this occurs, specifying both the series (by giving values for the coefficients $a_0, a_1, \dots$) and the function $\phi(x)$ to which that series converges for ‘x less than 1’. (Note: You don’t really need to work too hard to do this.)

(b) Notice that Abel went on to speculate that an even worse situation might occur. Namely, he proposed the possibility that a series $\phi(x) = a_0 + a_1 x + a_2 x^2 + \dots$ might be convergent for ‘x less than 1’ *and* convergent for $x = 1$, but in such a way that $\lim_{x \to 1} \phi(x)$ is not equal to $\phi(1)$.

What mathematical concept is involved here? That is, if such a function ϕ does in fact exist, what function property is ϕ lacking?

Question 6

Consider the following series discussed by Abel at the end of this extract:

$$\frac{x}{2} = \sin x - \frac{1}{2}\sin 2x + \frac{1}{3}\sin 3x - \text{etc.}$$

(a) Describe how this series is different from a power series.

(b) Now complete Abel's arguments concerning the numerical aspects of this series by determining what is absurd about this formula for $x = \pi$.

(c) Next complete Abel's comments about the differential aspects of this series by differentiating the formula term-by-term in order to show what can go wrong when one "applies all operations to infinite series as if they were finite". [Be sure to say what is wrong with the differentiation results!]

Heinrik Abel, 1826, Letter to Holmboe[8]

Another problem with which I have occupied myself a lot is the summation of the series

$$\cos mx + m\cos(m-2)x + \frac{m(m-1)}{2}\cos(m-4)x + \ldots$$

When m is a positive integer, the sum of this series as you know, is $(2\cos x)^m$, but when m is not an integer, this is no longer the case, except when x is less than $\pi/2$.

There is no other problem which has occupied mathematicians in recent times as much as this one. Poisson, Poinsot, Plana, Crelle and a large number of others have tried to solve it, and Poinsot is the first to have found the correct sum, but his reasoning is totally false. To this time no one has been able to get to the end with this [problem]. I am happy that I quite rigorously have arrived at this [end]. A memoir about this will appear in the Journal, and another I will soon send to France to appear in Gergonne's *Annales de Mathematiques*.

[There follows a discussion, omitted here, of some results concerning the above series which Abel has found.]

Divergent series are on the whole devilish, and it is a shame that one dares to base any demonstration on them. One can obtain whatever one wants, when one uses them. It is they which have created so much disaster and so many paradoxes. Can one imagine anything more appalling than to say

$$0 = 1 - 2^n + 3^n - 4^n + etc.$$

where n is a positive integer? *Risum teneatis amici!*[9]

I have in general got my eyes opened in a most astonishing manner: Because when one excludes the most simple cases, for ex. the geometric series, then in the whole of mathematics there is almost no infinite series whose sum is determined in a strict way. In other words, the most important part of mathematics stands there without foundation. Most of it is correct, that is true, which is exceedingly surprising. I am working hard to search for the reason behind this.

A very interesting task. I do not think you will be able to propose to me many theorems in which there are infinite series, against whose proof I shall not provide reasoned objections. Do it, and I will answer you.

[There follows a discussion, omitted here, about the Binomial Series, about which Abel had derived certain results.]

[8] The English translation of Abel's letter used in this project is taken from [2].

[9] Latin for "Hold your laughter, friends!"

To show by a general example how poorly one is reasoning and how careful one ought to be, I will choose the following example: Let

$$a_0 + a_1 + a_2 + a_3 + a_4 + \text{etc.}$$

be any infinite series. Then you know that a very useful way to sum this series is to search for the sum of the following:

$$a_0 + a_1 x + a_2 x^2 + a_3 x^3 + a_4 x^4 + etc.$$

and after that to put $x = 1$ in the result. This may be correct, but to me it seems one cannot assume it without proof, because even if one proves that

$$\phi(x) = a_0 + a_1 x + a_2 x^2 + \dots$$

for all values of x less than 1, it is not because of this certain that the same thing happens for $x = 1$. It could very well be possible that the series $a_0 + a_1 x + a_2 x^2 + \dots$ approaches a different quantity than $a_0 + a_1 + a_2 + \dots$ when x approaches more and more to 1. This is clear in the general case when the series $a_0 + a_1 + a_2 + \dots$ is divergent, because then it has no sum. I have proved that it is correct when the series is convergent.

The following example shows how one can cheat oneself. It can be strictly proved for all values of x less than π that

$$\frac{x}{2} = \sin x - \frac{1}{2}\sin 2x + \frac{1}{3}\sin 3x - etc.$$

From this it seems to follow that the same formula should hold for $x = \pi$, but then we would obtain . . . [an absurdity].

. . . .

One applies all operations to infinite series as if they were finite, but is this allowed? Hardly! — Where is it proved that one gets the differential of an infinite series by differentiating each term?

It is easy to give an example where this is not correct, for example:

$$\frac{x}{2} = \sin x - \frac{1}{2}\sin 2x + \frac{1}{3}\sin 3x - etc.$$

. . . .

3 Concluding Questions and Comments

The concerns expressed by Abel, Bolzano, Cauchy and Dedekind in the excerpts we have read in this project were emblematic of the state of analysis at the turn of the nineteenth century. Ultimately, mathematicians of the nineteenth century responded to this set of concerns by moving to the requirement of *formal proof* as a way to certify knowledge via the *rigorous use of inequalities* intended to capture the notion of two real numbers 'being close' that underlies the limit concept. Other factors that influenced this direction included new teaching and research situations, such as the École Polytechnique in Paris, that required mathematicians to think carefully about their ideas in order to explain them to others. Today, this nineteenth century response remains at the core of the study and practice of real analysis. The final question in this project takes another look back at the motivations of those who led the way in formulating this response, as they expressed it in their own words.

Question 7
Look back at the excerpts from the works of Abel, Bolzano, Cauchy and Dedekind that we have read in this project. What questions or comments would you address to these mathematicians about aspects of their concerns that are not addressed in the earlier questions? (Write at least one question and at least one comment, please!)

References

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[3] Bolzano, B., *Rein analytischer Beweis des Lehrsatzes, dass zwischen je zwey Werthen, die ein entgegengesetzes Resultat gewähren, wenigstens eine reele Wurzel der Gleichung liege*, Leipzig: W. Engelmann, 1817.

[4] Cauchy, A., *Cours d'Analyse de L'Ecole Royale Polytechnique*, Paris: Debure, 1821.

[5] Cauchy, A., *Résumé des Leçons sur le calcul infinitésimal*, Paris: Debure, 1823.

[6] Dedekind, R.,*Stetigkeit und irrationale Zahlen*, Braunschweig: F. Vieweg und Sohn, 1872.

[7] Dedekind, R., *Essays on the Theory of Numbers*, translator W. W. Beman, Chicago: The Open Court Publishing Company, 1901.

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[9] Sørensen, H. K., Exceptions and counterexamples: Understanding Abel's comment on *Cauchy's Theorem*, *Historia Mathematica* 32 (2005), 453 – 480.