ELSEVIER
Available online at www.sciencedirect.com
SCIENCE DIRECT
Historia Mathematica 32 (2005) 453–480
HISTORIA MATHEMATICA
www.elsevier.com/locate/yhmat

Exceptions and counterexamples: Understanding Abel's comment on *Cauchy's Theorem*

Henrik Kragh Sørensen *

Department of Mathematics, Agder University College, Kristiansand, Norway

Available online 18 March 2005

Abstract

It may seem odd that Abel, a protagonist of Cauchy's new rigor, spoke of "exceptions" when he criticized Cauchy's theorem on the continuity of sums of continuous functions. However, when interpreted contextually, exceptions appear as both valid and viable entities in the early 19th century. First, Abel's use of the term "exception" and the role of the exception in his binomial paper is documented and analyzed. Second, it is suggested how Abel may have acquainted himself with the exception and his use of it in a process denoted *critical revision* is discussed. Finally, an interpretation of Abel's exception is given that identifies it as a representative example of a more general transition in the understanding of mathematical objects that took place during the period. With this interpretation, exceptions find their place in a fundamental transition during the early 19th century from a *formal* approach to analysis toward a more *conceptual* one.

Zusammenfassung

Es könnte merkwürdig aussehen, daß Abel, ein Protagonist der neuen Strenge von Cauchy, von "Ausnahmen" sprach, als er den Lehrsatz von Cauchy über die Stetigkeit der Reihen von stetigen Funktionen kritisierte. Aber kontextbezogen interpretiert werden Ausnahmen sowohl akzeptable als auch sinnvolle Objekte der Analysis des frühen 19. Jahrhunderts. Zuerst werden Abels Gebrauch des Ausdruckes "Ausnahmen" und die Rolle, die Ausnahme in seinem Binomial-Arbeit spielt, dokumentiert und analysiert. Danach wird angedeutet, wie Abel sich mit den Ausnahmen vertraut gemacht haben könnte, und es wird sein Gebrauch von Ausnahmen in einem Prozess, der "kritischen Revision", diskutiert. Schliesslich wird eine Interpretation von Abels Ausnahme als Zeichen einer Umwandlung mathematischer Objekte vorgeschlagen. Diese Auslegung zeigt, wie die Ausnahmen ihren Platz in

* Fax: 47 3814 1071.
E-mail address: mail@henrikkragh.dk.

0315-0860/$ – see front matter
doi:10.1016/j.hm.2004.11.010

einem fundamentalen Wandel des frühen 19. Jahrhunderts finden, von einem formellen Zugang in Richtung auf eine begrifflichere Auffassung der Mathematik.

MSC: 01A55; 40-03

Keywords: Abel; Degen; Exception; Counterexample; Formula-centered; Concept-centered; Critical revision; Cauchy's theorem

1. Introduction

From the mid-18th century to the mid-19th century, the style of mathematical research in analysis underwent changes from a *formula-centered* approach epitomized by L. Euler (1707–1783) to a *concept-centered* style presented in works of G. P. L. Dirichlet (1805–1859) and G. F. B. Riemann (1826–1866). The transition manifested itself in multiple aspects of the mathematical enterprise including notations, questions, results, methods, and techniques. It was felt by the active and creative mathematicians of the nineteenth century who spotted a difference between the computational machinery associated with the *formula-centered* approach and the decidedly mental analysis belonging to the *concept-centered* approach. We find this distinction seized upon for instance in Dirichlet's obituary of C. G. J. Jacobi (1804–1851) where Dirichlet noticed[1]

> [...] the constantly increasing tendency of the new analysis to put thoughts in the place of calculations, [...],[2]

in the methodological principle attributed by D. Hilbert (1862–1943) to Riemann near the end of the century,

> I have tried to avoid the large computational apparatus of Kummer such that also here Riemann's principle should be observed, according to which one should conquer proofs not by computations but solely through thoughts,[3]

or in what H. Minkowski (1864–1909) called the "second Dirichlet principle" heralding the modern times in mathematics, according to which problems should be conquered

[1] All translations into English are made by the author. The original language quotations are included in the footnotes.

[2] "Wenn es die immer mehr hervortretende Tendenz der neueren Analysis ist Gedanken an die Stelle der Rechnung zu setzen, so giebt es doch gewisse Gebiete, in denen die Rechnung ihr Recht behält. *Jacobi*, der jene Tendenz so wesentlich gefördert hat, leistete vermöge seiner Meisterschaft in der Technik auch in diesen Gebiete Bewundernswürdiges." [Dirichlet, 1852, 21]

[3] "Ich habe versucht, den großen rechnerischen Apparat von Kummer zu vermeiden, damit auch hier den Grundsatz von Riemann verwirklicht würde, demzufolge man die Beweise nicht durch Rechnung, sonder lediglich durch Gedanken zwingen soll." [Hilbert, 1897, 67]

with a minimum of blind calculations and a maximum of enlightening thoughts.[4]

In the midst of this transition, the Norwegian N. H. Abel (1802–1829) briefly entered onto the international mathematical scene in the 1820s to raise a number of new questions and produce breathtaking new results. Although his mathematical corpus mainly dealt with algebraic questions, particularly pertaining to elliptic and higher transcendental functions, Abel also produced a new proof of the binomial theorem—a theorem central to attempts by Euler, J. L. Lagrange (1736–1813), and A.-L. Cauchy (1789–1857) to construct firmer foundations for analysis.

Abel's interest in the binomial theorem was awakened by Cauchy's *Cours d'analyse* of 1821,[5] in which Cauchy constructed a theory of infinite series based on a new standard of rigor. Aspiring to generalize Cauchy's proof of the binomial theorem to include complex exponents, Abel set forth on his path of inquiry adopted from his reading of Cauchy. In the process, however, Abel spotted that one of Cauchy's central theorems (on the continuity of any convergent sum of continuous functions) "admitted exceptions"—and it is this realization and the role it played in the transition between two different mathematical styles that is discussed in the present paper.

In T. S. Kuhn's philosophy of science,[6] the accumulation of evidence (observations and experiments) contradicting the prevailing paradigm plays the role of provoking crises ultimately resolved through revolutions. To some, mathematics differs from the sciences because the statements of mathematics are thought to be either true or false according to a time-independent correctness of their proofs, and no real revolutions occur in mathematics.[7] More recently, e.g., with I. Lakatos,[8] mathematics is seen as created by humans and developed through a dialectic that allows theorems to be falsified (refuted by counterexamples). Without adhering strictly to any of the theories associated with Kuhn or Lakatos, the present paper offers a diachronical reading of an important primary source from the early 19th century. The paper contextualizes this source within the transition from one style (a paradigm) that I call *formula-centered* mathematics to a new one, here termed *concept-centered*.[9]

One of the main problems facing the historian trying to make use of philosophical frameworks such as Kuhn or Lakatos is their reconstructed mechanism of development. Instead of claiming that the present framework presents a universal scheme applicable to all other fields, mathematicians, or periods of time, I am content to explore its explanatory power in understanding particular aspects of Abel's mathematical works. Here, I will use the transformation from a *formula-centered* to a *concept-centered* approach to investigate and analyze the role of Abel's exception. This interpretation will help us understand how theorems can allow exceptions during periods of transition. The development of exceptions into counterexamples is consequently explained through the evolution of *formula-centered* mathematics into *concept-centered* mathematics, an evolution that can be traced in the objects of mathematical studies and the methods for manipulating them.

4 "[...] von dem anderen *Dirichlet* schen Prinzipe, mit einem Minimum an blinder Rechnung, einem Maximum an sehenden Gedanken die Probleme zu zwingen, datiert die Neuzeit in der Geschichte der Mathematics." [Minkowski, 1905, 163]

5 [Cauchy, 1821].

6 [Kuhn, 1962].

7 On revolutions in mathematics, see [Gillies, 1992] and in particular [Gray, 1992].

8 [Lakatos, 1976].

9 This conceptual framework of *formula-centered* and *concept-centered* styles in mathematics has recently appeared in the literature, in particular in the works of H. Jahnke and D. Laugwitz.

2. Abel's *exception*

In his paper on the binomial theorem,[10] Abel used the series

$$\sum_{n=1}^{\infty} \frac{(-1)^{n-1}\sin nx}{n} \tag{1}$$

to question an important step in Cauchy's proof of the binomial theorem, a proof that Abel otherwise considered the most rigorous one available. Below, I reproduce Abel's objection in full from an enigmatic footnote attached to one of the theorems in Abel's paper:

> Remark. In the above-mentioned work of Mr. *Cauchy* (page 131) [the *Cours d'analyse* [Cauchy, 1821]], the following theorem can be found:
> "Whenever the various terms of the series
>
> $$u_0 + u_1 + u_2 + u_3 + \cdots$$
>
> are functions of a single variable quantity, and furthermore continuous functions with respect to this variable in the vicinity of a particular value for which the series converges, then the sum s of the series is also a continuous function of x in the vicinity of that particular value."
> However, it appears to me that this theorem admits exceptions. Thus, for instance, the series
>
> $$\sin\phi - \frac{1}{2}\sin 2\phi + \frac{1}{3}\sin 3\phi - \cdots$$
>
> is discontinuous for every value $(2m+1)\pi$ of x where m is an integer. As is known, a multitude of series with similar properties exists.[11]

[10] [Abel, 1826]. Most historical accounts of the rise of rigorization in analysis describe Abel's paper and the series (1); see, e.g., [Grattan-Guinness, 1972, 79–85] or [Bottazzini, 1986, 113–117].

[11] "Anmerkung. In der oben angeführten Schrift des Herrn *Cauchy* (Seite 131) findet man folgende Lehrsatz:
"Wenn die verschiedenen Glieder der Reihe

$$u_0 + u_1 + u_2 + u_3 + \cdots \quad \text{u.s.w.}$$

Functionen einer und derselben veränderlichen Größe sind, und zwar stetige Functionen, in Beziehung auf diese Veränderliche, in der Nähe eines besonderen Werthes, für welchen die Reihe convergirt, so ist auch die Summe s der Reihe, in der Nähe jenes besonderen Werthes, eine stetige Function von x."
Es scheint mir aber, daß dieser Lehrsatz Ausnahmen leidet. So ist z. B. die Reihe

$$\sin\phi - \frac{1}{2}\sin 2\phi + \frac{1}{3}\sin 3\phi - \cdots \quad \text{u.s.w.}$$

unstetig für jeden Werth $(2m+1)\pi$ von x, wo m eine ganze Zahl ist. Bekanntlich giebt es eine Menge von Reihen mit ähnlichen Eigenschaften." [Abel, 1826, 316, footnote]

So we notice that it “appeared” to the young Norwegian that Cauchy’s “theorem admitted exceptions.” Today, exceptions to mathematical theorems are not tolerated, so this remark needs to be seen in its historical context to make sense of it—understanding this context is the objective of the present paper.

At this point, it should be noted that A. L. Crelle (1780–1855)—the editor of the *Journal für die Reine und Angewandte Mathematik*—translated Abel’s paper from French into German for publication in the first volume of the *Journal*.[12] Unfortunately, Abel’s original manuscript is no longer extant. Therefore, we cannot be sure that Abel wrote the footnote himself nor how he chose his original words in French. Crelle’s version was adopted by B. M. Holmboe (1795–1850) and retranslated into French in the first edition of Abel’s *Œuvres* and later reproduced in the second edition.[13] Neither edition contains any essential comments concerning the phrasing of the footnote. One could be led to credit Crelle with the footnote,[14] but as the argument in this paper will show, there is nothing odd in assuming that Abel himself wrote the footnote and chose the words.

In order to understand how radically Cauchy (and Abel) broke with the established tradition in the theory of series in the 1820s,[15] it has been fruitful to focus on the movement called *algebraic analysis* that goes back to Lagrange.[16] By analyzing this tradition in its German context, H. N. Jahnke has demonstrated that Cauchy’s choice of a new foundation for analysis was quite contingent: Another approach that threw away fewer of the established methods but strived for an equally well founded analysis was attempted by, e.g., M. Ohm (1792–1872).[17]

As Jahnke has also pointed out,[18] a major inspiration for reconsidering the foundations of the theory of series in the early 19th century came from a phenomenon sometimes called the *Poisson Paradox*. For particular choices of m and x, the series

$$\sum_{n=0}^{\infty} \frac{\prod_{k=0}^{n-1}(m-k)}{n!} \cos\bigl((m-2n)x\bigr), \tag{2}$$

which is a formal expansion of $(2\cos x)^m$, produced one of the first sophisticated examples in which the series (2) was convergent but converged to a “false” sum—i.e., to a value, different from $(2\cos x)^m$. This fact, first observed by S.-D. Poisson (1781–1840) in 1811,[19] contested the common practice of inserting numerical values into a formal equality between a series and a closed expression. In particular, the impermissibility of such an operation was not easily identified from the properties of the series alone.

12 [Abel, 1881, II, 302].

13 [Abel, 1839, I, 66–92] and [Abel, 1881, I, 219–250].

14 Crelle was associated with the combinatorial school (see below), and, therefore, he was used to theorems (formulae) having exceptions.

15 Abel has been described as “more Cauchian than Cauchy” [Grattan-Guinness, 1970, 80], and Jahnke—presumably from the perspective of his 19th-century protagonist M. Ohm—largely equates their positions, even sometimes referring to it as “Cauchy/Abel” [Jahnke, 1987, 148]. However, the framework of *formula-centered* and *concept-centered* mathematics lets me view Abel as a much more complex figure in interpreting Cauchy: Abel was responsible for fixating the pointwise interpretation of Cauchy’s *concept-centered* definitions, and at the same time, Abel also worked extensively within the *formula-centered* approach to other branches of mathematics; more on this later.

16 See, e.g., [Fraser, 1987, 1988–1989; Jahnke, 1999].

17 [Jahnke, 1987, 1990].

18 [Jahnke, 1987, 105–117].

19 [Jahnke, 1987, 104].

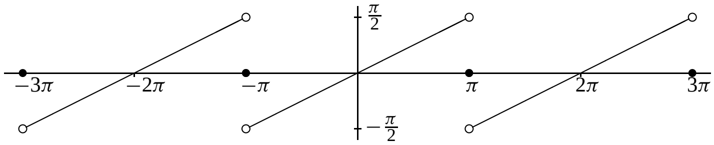

Fig. 1. Abel's *exception* (Eq. (1)) plotted graphically.

Explaining this problem played a prominent role in both French and German discussions of a proper foundation for the theory of series. Abel probably came across the example in 1825 during his discussions with Crelle, who had published on the topic. From his letters to Holmboe and from the binomial paper, itself,[20] it is quite obvious that the proper resolution of this problem was a key inspiration for Abel's binomial paper.

Considering the importance of the *Poisson Paradox* for Abel's research, one could be led to suggest that Ohm had exerted a direct influence on Abel during his time in Berlin. However, we have no indication of any direct interaction between Abel and Ohm—in fact, we have only one short mention in Abel's first letter from Berlin:

> Previously, Crelle used to house a weekly gathering of mathematicians, but he had to discontinue this because of a man named Ohm, who nobody got on with due to his horrible arrogance.[21]

When Abel listed the mathematicians who had worked on the *Poisson Paradox*, he explicitly included Crelle but not Ohm, although the latter had worked extensively on the problem.[22] Judging from this evidence, it is likely that Abel never met Ohm and—more speculatively—that he was discouraged from reading any of Ohm's works. Had Abel done so, he would have found in them many discussions of formulae with exceptions.[23] However, I see the occurrence of exceptions in Ohm's works more as an indication of the general state of analysis (the *formula-centered* approach) in Abel's time than as a source of direct inspiration. Instead, I find it equally likely that Abel drew his inspiration from another source, with which he was familiar even before his arrival in Germany.

The series chosen by Abel in his binomial paper to question Cauchy's statement was the Fourier series expansion of the function $f(x) = \frac{x}{2}$ on the open interval $(-\pi, \pi)$[24]; the graph of the series is depicted in Fig. 1. Abel's remark that "as is well known, a multitude of series with similar properties exists" has been understood as a reference to the work of J. B. J. Fourier (1768–1830).[25] However, as I will argue below, the series (1) (and similar ones) was familiar to Abel through other, more likely, sources. The series, itself, occurred thrice in Abel's mathematical corpus, always serving the purpose of criticism. The first instance—the only public one—from Abel's paper on the binomial theorem has already been presented and its context will be discussed. Besides, Abel used the series in a letter to Holmboe discussing term-

20 E.g., [Abel to Holmboe, 1826/01/16; Abel, 1902, 15–16] or [Abel to Holmboe, 1826/12; Abel, 1902, 52].

21 "Hos Crelle var før ogsaa een Gang om Ugen en Samling af Mathematikere men han var nødt til at ophøre dermed da der var een ved Navn Ohm, som ingen kunde komme ud af det med formedelst hans skrækkelige Arrongance." [Abel to Hansteen, 1826/12/05; Abel, 1902, 11]

22 [Abel to Holmboe, 1826/01/16; Abel, 1902, 15].

23 [Jahnke, 1987, e.g., 126].

24 The series expansion is periodic with a period of 2π and is periodically repeated on the real axis.

25 E.g., [Fourier, 1822]; see, e.g., [Grattan-Guinness, 1972, 85].

wise differentiation of series and in a notebook on the theorem that he put in place of *Cauchy's Theorem* (*Theorem C*; throughout this paper, I denote by *Theorem C* the statement that "any convergent sum of continuous functions is, itself, a continuous function"). These two instances will be discussed further below.

In his paper on the binomial series, Abel extended Cauchy's traditional way of proving the binomial theorem to allow for complex exponents. Furthermore, Abel emphasized that establishing the convergence of a series and finding its sum need to be separated into two steps—thereby stressing a point not foreign to Cauchy.

The basic structure of Cauchy's proof originated with Euler,[26] and it can be described as follows. First,[27] Cauchy devoted a problem to studying the functional equation

$$\phi(x+y)=\phi(x)\phi(y). \tag{3}$$

He found that $\phi(nx)=\phi(x)^n$ for integral values of n and he extended this to rational $\frac{p}{q}$ by letting $y=\frac{p}{q}x$ and writing $\phi(x)^p=\phi(px)=\phi(qy)=\phi(y)^q$. By extracting roots, Cauchy found that $\phi(\frac{p}{q}x)=\phi(x)^{\frac{p}{q}}$, and therefore, that $\phi(\mu)=\phi(1)^\mu$ for "any number μ."[28] For this last step, Cauchy implicitly—but consciously—assumed the continuity of ϕ. As a result of these steps, Cauchy obtained that the solutions to (3) were necessarily the exponential functions.

The proof of the binomial theorem, itself, came in another problem, some 60 pages later. There, Cauchy first recalled the binomial *formula* (the "formula of Newton"):

$$(1+x)^m=\sum_{n=0}^{m}\binom{m}{n}x^n \quad \text{for } m \text{ a positive integer.}$$

Cauchy then replaced m by any number μ and observed that the finite sum changed into the infinite series

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

He then combined three facts concerning this ϕ: (a) the series is convergent for $-1<x<1$, (b) the function ϕ is continuous, and (c) by the multiplication of series, ϕ satisfies the functional equation (3). Thus, the above-mentioned problem applies, and

$$\phi(\mu)=\phi(1)^\mu=(1+x)^\mu,$$

because $\phi(1)=1+x$ by direct inspection. *Theorem C* provided the continuity of ϕ as a function of μ and was thus a central step in the proof of Cauchy's binomial theorem.

[26] Euler gave two proofs of the binomial theorem; the one closer to Cauchy's adaptation is [Euler, 1775].
[27] [Cauchy, 1821, 106–108].
[28] [Cauchy, 1821, 108].

2.1. Abel's criticism of Cauchy's proof

At the center of Abel's critical attention was a theorem—*Theorem C*—in Cauchy's textbook *Cours d'analyse* describing the behavior of the sum of an infinite number of continuous functions. Cauchy argued the correctness of his theorem along the following lines[29]: First, he considered the series $s = \sum_{m=1}^{\infty} u_m(x)$ and introduced the notation $s = s_n + r_n$ where s_n denoted the sum of the first n terms of the series, and r_n denoted the corresponding tail of the series. He then argued that the increase of $s_n(x+\alpha)$ over $s_n(x)$ was infinitely small for infinitely small α (by the continuity of the polynomial s_n), and that $r_n(x+\alpha)$ and $r_n(x)$ vanished together (as tails of the series). Therefore, the sum was a continuous function, Cauchy claimed.

Formulated in modern terms, Cauchy's argument can be summarized as follows:

$$\begin{aligned} s &= s_n + r_n, \\ r_n(x+\alpha) &\to 0 \quad \text{when } n \to \infty, \\ r_n(x) &\to 0 \quad \text{when } n \to \infty, \text{ and} \\ s_n(x+\alpha) - s_n(x) &\to 0 \quad \text{when } \alpha \to 0. \end{aligned}$$

Therefore,

$$s(x+\alpha) - s(x) \to 0 \quad \text{when } \alpha \to 0.$$

In modern terms, this argument is not valid because the two limit processes ($\alpha \to 0$ and $n \to \infty$) are not independent. However, Cauchy had no means of symbolically separating the limit processes, and his argument suffered accordingly.[30] In fact, with the standard interpretation (which was partly settled by Abel's reading of Cauchy), these processes were indeed interrelated, but neither Cauchy nor Abel devised theories capable of dealing with such double limits. Later, double limits became a center of much attention, and efforts were made to devise new concepts, including *uniform convergence* that would clarify the situation.[31]

It was against this background of Cauchy's proof of the binomial theorem and *Theorem C* that Abel reacted. Abel's criticism consisted of three parts. First, Cauchy had banned divergent series from analysis, but Abel insisted on a complete separation of the two processes of finding a sum: (a) convergence of the series and (b) determination of the sum of the series. Second, Abel wanted to extend Cauchy's proof of the binomial theorem to include complex exponents—this extension made the separation of convergence and sum even more relevant because some cases of complex exponents led to divergent series (or even series with a "false sum," as had been the case with the *Poisson Paradox*). Finally, but to Abel not even worth

[29] [Cauchy, 1821, 131–132].

[30] With the advent of *nonstandard analysis* in the 20th century, some mathematicians, historians, and philosophers have reconstructed Cauchy's argument in terms of the nonstandard model of the real numbers, in which it can be interpreted to be correct; see, e.g., [Fischer, 1978; Giusti, 1984; Lakatos, 1978; Laugwitz, 1987, 1994; Robinson, 1966].

[31] Cauchy, himself, played a part in this in 1853, but only after various concepts highlighting modes of convergence had been expressed independently during the 1840s by K. T. W. Weierstrass, P. L. von Seidel, and G. G. Stokes; see, e.g., [Bottazzini, 1986, 202–208]. See also [Grattan-Guinness, 1986].

explicitly mentioning, Abel wanted to recast Cauchy's theory of series in a slightly different manner optimal for the proof of the binomial theorem. In this process, he replaced *Theorem C* with another result because he found the former to be "admitting exceptions." Thus, the exception was only part of a broader criticism, and Abel's motivation for this criticism was rooted in a more general unease with the state of affairs in the theory of series. In a letter to his mentor, C. Hansteen (1784–1873), Abel wrote of his plans for future research:

> Pure mathematics in its purest form must be my exclusive study for the future. I will devote all my powers to bringing some light to the vast darkness that incontestably now exists in *analysis*. It [analysis] completely lacks plan and coherence and it is truly remarkable that it can be studied by so many—and worst of all that it is not rigorously treated. Very few theorems exist in higher analysis that are demonstrated with convincing rigor. Everywhere one finds the unfortunate way of deducing from the special to the general, and it is highly remarkable that after such procedures so few of the so-called paradoxes entail. It is really very interesting to search for the reason for this.[32]

Abel's statement that the "unfortunate way" of reasoning "from the special to the general" could lead to "paradoxes" is a direct continuation of Cauchy's argument against the "generality of algebra" from his introduction to the *Cours d'analyse*.[33] There, Cauchy described how he had been forced to abandon certain types of arguments, in particular arguments based on the "generality of algebra," i.e., the formal interpretation of equality between expressions, to achieve his desired "geometrical" standard of rigor. Cauchy replaced this Eulerian idea of formal equality by a numerical conception of equality in which two expressions were equal if they gave exactly the same results for equal values of the variables. Prompted by this transition in conceptions of equality, mathematicians revisited, revised, and reformulated old and important results to bridge the gap between the two styles—I term this process *critical revision*—and many of Abel's actions can best be understood in this context. Abel's ambition to investigate the relative infrequency of paradoxes in analysis is one of the core components of his critical revision in the theory of series. Besides pointing to the problem, Abel also suggested as a partial explanation for the relatively few problems that had been encountered that until recently analysts had mainly worked with power series. These, Abel thought, apparently behave nicely and in accord with intuition and the questioned procedures. He continued:

> As soon as others [series that are not power series] enter, which does not happen often, one is mostly led astray, and a set of interrelated false theorems emerges from false conclusions.—I have worked through many of these and have had the fortune of seeing it clearly. Whenever one proceeds in the ordinary fashion, it is probably all right; but I have had to be very cautious because the theorems that have been accepted

32 "Den rene Mathematik i sin reneste Betydning maa blive ganske mit Studium for Fremtiden. Alle mine Kræfter vil jeg anvende paa at bringe noget mere Lys i det uhyre Mørke som der uimodsigelig nu findes i *Analysen*. Den mangler saa ganske al Plan og System, saaat det virkelig er høist forunderlig at den kan studeres af saa mange og nu det værste at den aldeles ikke er stræng behandlet. Der gives yderst faae Sætninger i den høiere Analyse som ere bevisede med overbevisende Strænghed. Overalt finder man den ulykkelige Maade at slutte fra det Specielle til det Almindelige, og yderst mærkværdigt er det at der efter en saadan Fremgangsmaade dog kuns findes faae af de saakaldte Paradoxer. Det er virkelig meget interessant at eftersøge Grunden hertil." [Abel to Hansteen, 1826/03/29; Abel, 1902, 22]

33 [Cauchy, 1821, ii–iii].

> without rigorous proof (i.e., without proof) have struck such deep roots with me that I constantly run the risk of using them without further testing.[34]

Abel was quite explicit about the need for a critical revision of the theory of series, and he knew that previously accepted truths had been so well established that mathematicians could have trouble questioning them and investigating the reasons behind their validity. Furthermore, he observed that nonpower series (e.g., trigonometric series) might provide the key to producing streams of "interrelated, false theorems." These two observations provide the essence of Abel's critical revision: (1) the prevailing theory needs critical revision, and (2) trigonometric series could be used to test the accepted truths and principles.

The *exception* and its context in terms of *Theorem C* have now been presented. Before I discuss why Abel would choose to call it an "exception," it is time to present and analyze Abel's reactions to it.

2.2. *How did Abel respond to the* exception*?*

In response to the problematic status of *Theorem C* that "admitted exceptions," Abel devised a new theorem—here called *Theorem A1*—tailored to the specific demands raised by the binomial theorem. Whereas *Theorem C* dealt with any convergent series of continuous functions, Abel stated and proved a theorem only dealing with a certain type of series yet powerful enough for (and specifically intended to be used in) the required step in the proof of the binomial theorem. Abel considered series of the form

$$\sum_{n=0}^{\infty} v_n(x)\alpha^n \tag{4}$$

and assumed that the functions v_n were continuous in an interval $x \in [a, b]$. He then argued that if a value $\delta > 0$ existed such that the series $\sum_{n=0}^{\infty} v_n(x)\delta^n$ converged,[35] and if $0 \leqslant \alpha < \delta$,[36] then the sum function (4) would also be continuous on the same interval.[37]

Faced with a situation in which a theorem admitted exceptions, Abel took refuge in a narrower theorem only pertaining to a subconcept of infinite series—in Abel's case defined by what could appear to be a somewhat arbitrary form (4). This approach has been viewed by Lakatos as one of the prototypical responses to the emergence of "monsters" or "exceptions," and Lakatos termed it "exception-barring."[38] However, three different arguments can be made on why Abel saw power series as a safe haven protecting against the "emergence of false theorems" opened by trigonometric series. First, *Theorem A1* dealt with

---

34 "Saasnart der komme andre imellem hvilket rigtig nok ikke ofte er Tilfældet saa gaaer det gjerne ikke godt og af falske Slutninger opstaae da en Mængde med hinanden forbundne urigtige Sætninger.—Jeg har gjennemgaaet flere af disse og har været saa heldig at komme paa det Rene dermed. Naar man blot gaaer almindelig tilværks saa gaaer det nok; men jeg har maattet være særdeles forsigtig, thi de engang uden strængt Beviis (ɔ: uden Beviis) antagne Sætninger have slaaet saa dybe Rødder hos mig at jeg hvert Øjeblik staaer Fare for at bruge dem uden nøiere Prøvelse." [Abel to Hansteen, 1826/03/29; Abel, 1902, 22–23]

35 The convergence at δ was not explicitly mentioned by Abel but appears to be a tacit assumption that was made explicit in the *Œuvres* [Abel, 1881, I, 223]; see also [Spalt, 2002, 291].

36 Abel was not explicit about the precise nature of α and δ.

37 [Abel, 1826, 315].

38 [Lakatos, 1976, 133–136].

a class of series large enough to include the ones needed for the binomial theorem—it was sufficiently strong; this is a pragmatic reason. Second, *Theorem A1* bore resemblances to another of Abel's theorems guaranteeing the continuity of a power series on its border of convergence under conditions similar to those assumed in *Theorem A1*[39]; this would be a structural reason. And finally, Abel saw aesthetic values in power series which, formally, were the simplest specialized class of series. Thus, three intrinsic reasons for Abel's response can be reconstructed—the first two merit additional attention.

Abel's proof of *Theorem A1* was closely modeled on his proof of the theorem preceding it in the binomial paper—here referred to as *Theorem A2*—that stated the continuity of a power series when its variable approaches the boundary of convergence; i.e., in modern terms, if $\delta > 0$ is such that the series $\sum_{n=0}^{\infty} a_n \delta^n$ is convergent and if $0 \leqslant \alpha < \delta$, then

$$\lim_{\alpha \to \delta_-} \sum_{n=0}^{\infty} a_n \alpha^n = \sum_{n=0}^{\infty} a_n \delta^n. \tag{5}$$

The close interrelation between *Theorem A2* and *Theorem A1* led Abel to model his proof of the latter on the proof of the former. By analyzing the two proofs, their relationship will become apparent and it will be found that *Theorem A1* lacks the same kind of uniformity requirements as *Theorem C* and is therefore equally dubious—as it stands, it is actually false.

In the process of proving *Theorem A2*, Abel assumed the convergence of the series $\sum_{n=0}^{\infty} v_n \delta^n$, broke off the series after m terms, and let p denote a quantity that was larger than any of the sections of the tail:

$$p \geqslant \sum_{n=m}^{m+M} v_n \delta^n \quad \text{for } M = 0, 1, 2, \ldots. \tag{6}$$

Because the series was assumed to be convergent, such a p indeed exists; however, its existence was not a question to Abel and was never mentioned. Believing to have obtained such a p (6), Abel wrote the tail of the series (4) as

$$\psi(x) = \sum_{n=m}^{\infty} v_n \alpha^n < \sum_{n=m}^{\infty} v_n \delta^n. \tag{7}$$

A previous result of the binomial paper[40] stated and proved that for any decreasing sequence $\{\varepsilon_n\}$ and for any sequence $\{t_n\}$ whose partial sums were bounded $\sum_{k=0}^{m} t_k < \nu$ (for all m), the following inequality holds:

$$\sum_{k=0}^{m} \varepsilon_k t_k < \nu \varepsilon_0.$$

39 [Abel, 1826, 314].

40 [Abel, 1826, 314].

This rather technical lemma now allowed Abel to conclude from (7) that

$$\psi(\alpha) < \left(\frac{\alpha}{\delta}\right)^m p, \tag{8}$$

and from this, *Theorem A2* followed quickly by letting m increase, since the first m terms of the series were always a finite (and thus continuous) polynomial.

When Abel applied the same procedure to the situation in which the coefficients were functions of the variable x (*Theorem A1*), he introduced the function $\theta(x)$ such that in analogy with (6),

$$\theta(x) \geqslant \sum_{n=m}^{m+M} v_n(x)\delta^n \quad \text{for } M = 0, 1, 2, \ldots \text{ and for all } x \in [a, b]. \tag{9}$$

However, Abel treated the function $\theta(x)$ as a constant, in complete analogy with p of the preceding proof. Consequently, Abel claimed that m could be chosen such that $(\frac{\alpha}{\delta})^m\theta(x)$ was infinitesimal. He did not realize that such an m could depend on x—and his notation gave no possibility of making the interrelation explicit. Thus, when Abel tacitly applied the same argument to $\theta(x + \omega)$ and—still tacitly—thought that a single, definite m could be found independent of x, it amounted to a hidden uniformity requirement. Later, another version based on *absolute convergence* was demonstrated,[41] but a counterexample to *Theorem A1* can actually be constructed.[42]

Once Abel had set up the general theoretical results to be used in his proof of the binomial theorem, he returned to the procedure already advocated at the beginning of his paper: to consider the convergence of the binomial series and to determine the conditions under which its sum equaled the binomial. In doing so, Abel combined his six preliminary theorems (which constitute an elaborate conceptual analysis of convergence and continuity) with much more explicit manipulations of the particular case at hand, the binomial series. Although the many details of these manipulations take up most of Abel's paper, they are of little direct impact for the present discussion.

Apart from the binomial paper, the primary traces of Abel's investigations into the permissible operations on infinite series are found in his notebooks. They date from the year following the publication of the binomial paper (1827).[43] Holmboe made selections from these notebooks and published them in the first edition of Abel's *Œuvres*.[44] Most of the selection concerns interesting criteria of convergence, but the last part of it is devoted to the continuity of series of the form

$$f(y) = \sum_{n=0}^{\infty} \phi_n(y)x^n, \tag{10}$$

which constitute a class similar to the one that Abel treated in *Theorem A1* in the binomial paper. Thus, Abel continued to work on new proofs of *Theorem A1*, and in the notebook, we find two new deductions of it. Following the deductions, Abel turned to testing the limits of the theorem when he observed:

41 [du Bois-Reymond, 1871].
42 See the suggestion in [Walter, 1985, 138].
43 [Abel, 1881, II, 326].
44 [Abel, 1827/1881].

> For example, [...]
>
> $$f(y) = \sin y \cdot x + \frac{1}{2}\sin 2y \cdot x^2 + \frac{1}{3}\sin 3y \cdot x^3 + \cdots$$
>
> is a continuous function of y if $x < 1$. If $x = 1$, the series is still convergent but in this case $f(y)$ is discontinuous for certain values of y.[45]

Here, we see Abel using a series very similar to the *exception*—indeed, it is the same function translated horizontally by π—for (self-)critical purposes. The fact that Abel returned to the *Theorem A1* within the first year after his binomial paper was published prompted M. S. Lie (1842–1899), one of the editors of the second edition of Abel's *Œuvres*, to "conclude beyond doubt" that Abel had become dissatisfied with the published proof.[46] For the present context, it is more interesting to notice how properties of the *exception* were once again utilized to illustrate the boundaries of results. On the same occasion, Abel also considered a small number of other examples in order to illustrate similar points.

In summary, Abel began his research on the theory of series inspired by Cauchy's new attitudes toward rigor and a great sense of the need for critical revision. In a footnote, he criticized *Theorem C* through the use of an "exception," and he went on to prove a limited version of *Theorem C* that was sufficiently strong and that he thought was more rigorous. We now turn to the *exception* as such and Abel's use of it in yet another, related context.

3. Abel's acquaintance with the *exception*

Abel's exceptional series (1) played an important role not only in his binomial paper but also in another context. In this section, I discuss how Abel may have come to know of the remarkable properties of this series and how he used these. In this respect, some important clues can be found in Abel's training and apprenticeship as a mathematician.

3.1. Encounters with Degen

The first mathematician outside Norway with whom Abel had contact was C. F. Degen (1766–1825), a professor of mathematics at the University of Copenhagen. Their communication began in 1821, when Abel thought he had solved the general quintic equation and his presumed solution was forwarded to Degen for evaluation.[47] In 1823, Abel visited Copenhagen and met Degen. Because their communication

45 "Par exemple, [...]

$$f(y) = \sin y \cdot x + \frac{1}{2}\sin 2y \cdot x^2 + \frac{1}{3}\sin 3y \cdot x^3 + \cdots$$

est fonction continue de y, si $x < 1$. Si $x = 1$, la série est encore convergente, mais dans ce cas $f(y)$ est discontinue pour certaine valeurs de y." [Abel, 1827/1881, 202]

46 [Abel, 1881, II, 326].

47 See, e.g., [Stubhaug, 2000, 239–240] or [Sørensen, 2002a, 2002b].

was only oral, sources documenting their interactions in this period are sparse. However, we know that Abel held Degen in high regard and that he thought of him as his intellectual mentor.[48] We also know from Abel's letters that he read some of Degen's works with interest.[49]

To Degen, the object that Abel later utilized as an "exception" to *Theorem C* was familiar; indeed, he had used it in an interesting way in a paper from 1802.[50] Degen's essay—published in Danish in the journal of the Royal Danish Academy of Sciences and Letters and entitled "Contributions to the critique of studies in mathematics"—discussed the mental prerequisites for the successful cultivation of mathematics. It was construed as an argument against an exclusively utilitarian rationale or legitimization for mathematics and took the form of a defense of the intrinsic value of the discipline. However, it was also Degen's purpose to discuss the "abuse that one could fear from the generalization of theorems"—a topic of direct interest to our argument.[51] To the latter, critical end—explicitly informed by the critical philosophy of I. Kant (1724–1804)—Degen wrote:

> For instance, one would conclude from the fact that
>
> $$\sin\phi + \frac{1}{2}\sin 2\phi + \frac{1}{3}\sin 3\phi + \cdots \text{ in inf.} = \frac{\pi - \phi}{2}$$
>
> that $0 + 0 + 0 + \cdots = \frac{\pi}{2}$ and conclude wrongly.[52]

In a footnote, Degen continued:

> This is not the only case in which the assumption that the variable quantity $= 0$ can lead one astray when one employs the general form without caution. [...] The author does not believe that this remark is superfluous with respect to whoever want to educate themselves as mathematicians. For the masters of the science it [the remark] is of course not necessary.[53]

Two observations should be made here concerning Degen's paper. First, Degen used a function strikingly similar to Abel's *exception* in pointing out that inserting particular numerical values into "general forms" was highly problematic.[54] Explicitly, Degen drew the same conclusion that Abel would later draw,

48 In the period 1821–1824, Abel's main influences were Hansteen and Degen, see [Holst, 1902, 22].

49 [Abel to Holmboe, 1826/08/04; Abel, 1902, 5].

50 [Degen, 1802].

51 [Degen, 1802, 77].

52 "Man vilde f.Ex. deraf, at

$$\sin\phi + \frac{1}{2}\sin 2\phi + \frac{1}{3}\sin 3\phi + +\text{in inf.} = \frac{\pi - \phi}{2}$$

slutte, at $0 + 0 + 0 + 0 + \cdots = \frac{\pi}{2}$, og slutte *urigtigen*." [Degen, 1802, 91, original emphasis]

53 "Dette er ikke det eneste *Tilfælde, hvor den Forudsætning, at den foranderlige Störrelse* $= 0$, *kan före i Vildfarelse,* naar man uden Forsigtighed anvender den almindelige Form. [...] Forf. troer ikke, at denne Anmærkning er overflödig i Henseende til den, der vilde danne sig til Mathematiker. For Mestere i Videnskaben behöves den naturligviis ikke." [Degen, 1802, 91–92, footnote, original emphasis]

54 What Degen called a "general form" is the *formal equality* so central to the Eulerian, *formula-centered* approach to functions.

namely that inserting $x = 0$ into the (general) equality

$$\sum_{n=1}^{\infty} \frac{\sin nx}{n} = \frac{\pi - x}{2} \tag{11}$$

would give absurd results. Precisely this difference between the *formal* equality of (11) and the numerical equality that would result if $x = 0$ were inserted into (11) is a central feature of the rigorization of analysis associated with the name of Cauchy. It hinges on the point that formulae are not themselves the truths of Cauchy's analysis—instead, the numerical correspondences, which they entail, lie at the heart of the discipline. Second, Degen reserved his remarks for the novices of mathematics who had not yet developed an intuition for the proper use of "general forms"—to the masters of the discipline, such remarks would be superfluous. I will return to this below, when the role of intuitions in mathematics is briefly touched upon.

To sum up, it is probable that Abel learned of the properties and problems of the series that would later serve as his *exception* from Degen's 1802 paper. We even have a good indication that Abel read this paper: Abel borrowed the relevant volume of the transactions of the Danish academy on April 30, 1822.[55] As there are no other papers with mathematical content in this volume of the transactions, I hold it for certain that Abel borrowed the volume to read Degen's paper. However, Degen was by no means the first to treat this series; it had also occurred in the works of Euler and J. F. Pfaff (1765–1825),[56] whom Abel could also have read. However, I believe that Degen's sound epistemological and methodological intuitions—the expression of which was the purpose of his paper—made Abel aware of the potential use of the example to illustrate how even simple objects could display nonintuitive behavior.

3.2. The exception *in Abel's letter to Holmboe*

Abel's use of the *exception* (1) was not restricted to the criticism of *Theorem C* that has been discussed above. Indeed, he also used the same series to discuss the permissibility of termwise differentiation of series. In a long and important letter from Abel to Holmboe—written while Abel was contemplating the binomial paper—the *exception* served as a critical tool. First, Abel related to Holmboe the same use of it that he would later repeat in the famous footnote of the binomial paper discussed already:

> The following example shows how one can be deceived. It can be proved rigorously that for all values of x less than π one has
>
> $$\frac{1}{2}x = \sin x - \frac{1}{2}\sin 2x + \frac{1}{3}\sin 3x - \text{etc.}$$
>
> From this it appears that the same formula should hold for $x = \pi$, but in that case one would have
>
> $$\frac{1}{2}\pi = \sin \pi - \frac{1}{2}\sin 2\pi + \frac{1}{3}\sin 3\pi - \text{etc.} = 0 \quad \text{(absurd).}$$

[55] I am grateful to B. Granrud, who found this information in the loan protocols located in the Manuscript Collection, the National Library, Oslo.

[56] The series occurred, e.g., in [Euler, 1754, 584] and was used by Pfaff in 1788; see [Reich, 1989, 233–234]. Both Euler and Pfaff used it to solve problems, and not as a vehicle for critique.

One can find indefinitely many such examples.[57]

In this letter, we find Abel presenting the same example as in the footnote but interpreting it in a slightly different way. Notably, in the letter to Holmboe, Abel spoke of "paradoxes," not of "exceptions,"[58] and he observed that "indefinitely many such examples can be found"—a point that he also stressed in the binomial paper.[59] This emphasis on the nonsingular nature of the exception is a common theme in most of the uses of exceptions in the early 19th century—and one to which I shall return below.

Later in that same letter, Abel discussed the limits to the permissible operations on infinite series[60]:

> One applies all operations to infinite series as if they were finite, but is this permissible? I think not.—Where is it proved that one gets the differential of an infinite series by differentiating each term? It is easy to give an example for which this is not true; e.g.,
>
> $$\frac{1}{2}x = \sin x - \frac{1}{2}\sin 2x + \frac{1}{3}\sin 3x - \cdots.$$
>
> Differentiation gives
>
> $$\frac{1}{2} = \cos x - \cos 2x + \cos 3x - \text{etc.}$$
>
> a result which is quite false because this series is divergent.[61]

57 "Følgende Exempel viser hvor man kan bedrage sig. Det kan strængt bevises og man har for alle Værdier af x som ere mindre end π

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

Deraf synes at følge at den samme Formel skulde finde Sted for $x = \pi$; men da vilde man faae ud

$$\left[\frac{1}{2}\right]\pi = \sin \pi - \frac{1}{2}\sin 2\pi + \frac{1}{3}\sin 3\pi - \text{etc.} = 0 \quad \text{(absurd)}.$$

Man kan finde utallige saadanne Exempler." [Abel to Holmboe, 1826/01/16; Abel, 1902, 17–18]

58 [Abel to Holmboe, 1826/01/16; Abel, 1902, 16].

59 [Abel, 1826, 316, footnote].

60 [Abel to Holmboe, 1826/01/16; Abel, 1902, 13–19].

61 "Man anvender alle Operationer paa uendelige Rækker som om de vare endelige, men er dette tilladt. Vel neppe.—Hvor staaer det beviist at man faar Differentialet af en uendelig Række ved at differentiere hvert Led? Det er let at anføre Exempler hvor dette ikke er rigtigt, f.Ex.:

$$\left[\frac{1}{2}\right]x = \sin x - \frac{1}{2}\sin 2x + \frac{1}{3}\sin[3]x - \cdots$$

Differentieres saa faaer man

$$\left[\frac{1}{2}\right] = \cos x - \cos 2x + \cos 3x - \text{etc.}$$

Abel further remarked that several other operations were just as problematic as the termwise differentiation of series, and that he found great satisfaction in scrutinizing the established rules:

> I have begun to work through the most important rules that are now valid in this respect and demonstrate in which cases they are true or not.—I make good progress, and it is immensely interesting.[62]

As can be seen, Abel made good use of the exceptional series that he had come across. Not only did it illustrate that the convergence of a series of continuous functions did not necessarily imply the continuity of the sum, it could also be used to illustrate that the common procedure of termwise differentiation was not unproblematic. Furthermore, the excerpt illustrates how Abel acted as a *concept-centered* mathematician faced with a critical amount of counterevidence and began researching the limits of his concepts (continuity, convergence, etc.) and reformulating statements of theorems to take the exceptions into account. However, Abel never published much on this critical revision, and we are left with only sparse evidence for his investigations.

The present discussion has suggested how Abel used the exception series (1) as a sound critical tool inspired by Degen's essay. It has also illustrated how the footnote in the binomial paper was not Abel's only encounter with the exception series and that he made important use of it in other contexts of critical examination. Now that we know about Abel's familiarity with the "exception," we must inquire again why he would choose to describe it as such.

4. "Exception" or "counterexample": what's in a word?

At first sight, one might be tempted to interpret Abel's choice of words as an indication of the veneration he had for Cauchy. Plausible as this might be to those tempted by psychologisms, the fact—to be demonstrated below—that other mathematicians also spoke seriously about "exceptions" seems to refute this as being much too simplistic a way out. Below, based on diachronical and contextual readings, I suggest and insist that what Abel termed an "exception" in 1826 needs to be distinguished from the highly technical meaning given to counterexamples today, and that Abel's *exception* must be interpreted within the changing attitudes toward analysis in the early 19th century.

4.1. "Exceptions" in early 19th century mathematics

Even outside the school of algebraic analysis, a number of examples can be given, in which mathematicians—contemporaries of Abel—also spoke of "exceptions." For instance, as highlighted by Lakatos,[63] in the discussion of Euler's theorem on polyhedra, S. A. J. l'Huillier (1750–1840) and J. F. C. Hessel (1796–1872) spoke of "theorems suffering exceptions," using the same phrases as Abel did.[64]

Resultat, ganske falsk, thi denne Række er divergent." [Abel to Holmboe, 1826/01/16; Abel, 1902, 18]

62 "Jeg har begyndt at gjennemgaae de vigtigste Regler som ere gældende (nu) i denne Henseende, og vise i hvilke Tilfælde de ere rigtige eller ikke.—Det gaaer ganske godt og interesserer mig umaadelig." [Abel to Holmboe, 1826/01/16; Abel, 1902, 18]

63 [Lakatos, 1976].

64 "[...] que le théorème d'Euler souffre des exceptions numbreuses" [Lhuilier, 1812/1813, 172] and "Indessen leidet derselbe [Satz] Ausnahmen." [Hessel, 1832, 13].

Similarly, in the context of finding the number of imaginary roots of an equation, J. B. Bérard quoted J. le R. d'Alembert (1717–1783) to the effect that "exceptions confirm the rule."[65] And, in a discussion of a proof by F. D. de Foncenex (1734–1799), Lagrange also linked the word "exception" to the concept of "general validity"[66]:

> This principle is generally valid, but I have remarked that it can be subject to exceptions that can make the preceding demonstration defective.[67]

Below, two further examples will support my claim, that an "exception" was an independent object in mathematics that we must try to understand. The first example arose from a discussion between two of Abel's best friends, while the second one takes us back to the master, himself, Cauchy.

Six years after Abel's early death, the professors of mathematics at the University of Christiania (now Oslo, Norway) engaged in a heated debate concerning the nature of mathematics and its proper teaching. In a textbook on planar geometry from 1835,[68] Hansteen suggested an extension of ordinary planar geometry to include curved lines. In the process, he sought to widen the concept of parallel lines to include nonintersecting, curved ones. Holmboe responded emphatically to this extended definition in a notice in a Christiania newspaper.[69] Holmboe's means of refuting Hansteen's definition consisted of three different components worthy of comment. First, Holmboe illustrated in some detail how there were "exceptions to [some of Hansteen's] theorems."[70] Second, Holmboe argued that indefinitely many exceptions (exhibiting quite odd behavior) could be constructed. And finally, Holmboe was not satisfied with presenting "exceptions" but went on to give other points of criticism in support for his views—exceptions were not sufficient reason to overthrow results. In Denmark, C. Jürgensen (1805–1860) also reviewed the textbook and commented upon the theorems that had exceptions.[71] According to Jürgensen, Hansteen's extension was quite natural, and Holmboe's exceptions concerned theorems that were presented as being *general*, and therefore the criticism by exceptions did not apply. Thus, Jürgensen concluded that the mere lack of lucid presentational style was not sufficient reason to dismiss the extended concept of parallel lines.[72]

From this example, three important observations of direct relevance to our discussion can be drawn. First, Holmboe used the term "exception" in a way comparable to Abel's use of it. Although there is no direct reference from Holmboe to Abel's use of the word, two scenarios force themselves upon us: It is plausible that the term "exception" was not at all exceptional among mathematicians of the 1820s and 1830s,[73] or Holmboe may have picked the term out of Abel's paper. Second, Holmboe's exceptions were not *definitive*—they did not close the debate: Holmboe had to observe that indefinitely many such

65 [Bérard, 1819, 349].

66 For more on Lagrange's concept of "the general" and his belief in it as a principle, see [Grabiner, 1981a, 317] and [Grabiner, 1981b, 39].

67 "Ce principe est généralement vrai; mais j'ai remarqué depuis qu'il était sujet à de exceptions qui pouvaient mettre la démonstration précédente en défaut." [Lagrange, 1826, 182; the same phrase can be found in the previous editions of 1797/98 (an VI) and 1808]

68 [Hansteen, 1835].

69 [Holmboe, 1835].

70 [Holmboe, 1835, 2].

71 [Jürgensen, 1836].

72 [Jürgensen, 1836, 468].

73 This seems plausible given the examples presented above.

exceptions existed, and he even had to go beyond the use of exceptions and voice other criticisms against Hansteen's concepts. And third, Jürgensen and Holmboe disagreed over the severity of the exceptions—according to Jürgensen, theorems that were *general* might allow for some exceptions without being seriously impaired.

After briefly discussing the Hansteen–Holmboe dispute that took place on the periphery of mathematical Europe, we will immediately return to its center. After he announced *Theorem C* in the *Cours d'analyse* (1821), Cauchy did not return specifically to the subject in print until 1853. Then, prompted by a communication to the *Académie des sciences* by C. A. Briot (1817–1882) and J.-C. Bouquet (1819–1885),[74] Cauchy eventually faced the problems associated with *Theorem C*.[75] Interestingly, in 1853, Cauchy introduced *Theorem C* in the following way:

> In establishing, in my *Analyse algébrique*, the general rules relating to the convergence of series, I have also announced the following theorem.[76]

Here, we see *Theorem C* described as both a "theorem" and a "rule"—a fact, to which I shall return soon. Then, acknowledging the work of Briot and Bouquet, Cauchy described how *Theorem C* could not be "admitted without restrictions" and he presented the series $\sum \frac{1}{n} \sin nx$ as an example with a discontinuous sum function. Cauchy then proceeded to amend the theorem:

> By the way, it is easy to see how one should modify the statement of the theorem so that it no longer gives way to any exception.[77]

Cauchy's revision consisted of adding an explicit requirement of uniform convergence. Interesting as this concept is, our main interest here lies with Cauchy's choice of words and with his response.

From the quotation, we see how Cauchy had been brought to realize that *Theorem C* admitted exceptions—in the brief, printed version, Briot and Bouquet did not speak of "exceptions," but the word might have been used in the discussions in the *Académie des sciences*, and Cauchy certainly used it. Also worthy of notice is the fact that Cauchy explicitly considered the family of exceptions that included Abel's *exception*. Cauchy's example—which was also used by Degen and Abel—is a horizontal translation of Abel's example (1) and, in a sense, these are the "natural" exceptions to *Theorem C*. By comparing their responses, we realize that Abel and Cauchy reacted differently to the fact that *Theorem C* admitted exceptions: Abel limited the class of objects under consideration by requiring a special *form* of the functions u_m, whereas Cauchy made extra requirements concerning the nature of the convergence.[78] Cauchy may thus be seen as reacting in a *concept-centered* way compared to Abel's restriction of the domain of the theorem by an essentially *formula-centered* criterion. Finally, we learn that Cauchy thought of the

74 [Briot and Bouquet, 1853].

75 [Cauchy, 1853].

76 "En établissant, dans mon *Analyse algébrique*, les règles générales relatives à la convergence des séries, j'ai, de plus, énoncé le théorème suivant." [Cauchy, 1853, 30–31]

77 "Au reste, il est facile de voir comment on doit modifier l'énoncé du théorème, pour qu'il n'y ait plus lieu à aucune exception." [Cauchy, 1853, 31–32]

78 This difference has been emphasized by [Lakatos, 1976, 127–141]; see also [Koetsier, 1991, 73–92].

contents of the *Cours d'analyse* as a set of "general rules" governing the convergence of series—this will be elaborated upon in the following argument.

4.2. Rules and exceptions or theorems and counterexamples

In a certain sense, the crucial part of the present argument can be phrased in terms of the two word-pairs: rule–exception and theorem–counterexample. In the 19th century, I suggest, a "rule" would be perceived as a *general statement* for which "exceptions" were possible, i.e., *a result that holds generally true*.[79] We have seen how both Cauchy and Abel spoke of "rules" in the theory of series, and we have observed the curious fact that some such rules admitted "exceptions." On the other hand, a (mathematical) "theorem" is now perceived as an absolutely true statement; any "counterexample" will render the theorem false and therefore turn it into a nontheorem. However, when Abel spoke of "exceptions," they were exceptions to *theorems*, and this complication of the word-pairs has to be disentangled.

In the present context, there is an interesting difference between the theorems of geometry in Euclid and the results in analysis presented by Euler: Euler's results in analysis were often devised as *rules*, i.e., formal statements that hold generally good. The value of rules lies in their applicability: skilled or intuitively gifted mathematicians—such as Euler, himself—knew when to apply which rules and when these rules might possibly lead to "exceptions." This kind of operational knowledge is opposed to mathematical *theorems* that are held not to be only *generally* good but to be precise, i.e., true for all (meaning each and everyone of) the terms falling under them. In the modern model of mathematical knowledge, one might argue, a theorem with an exception—which we prefer to call a *counterexample*—is *refuted* and is thus *false*: its domain of validity has been misstated. Furthermore, with the modern view of theorems, counterexamples are extremely useful in the very important procedure of precisely establishing the extension of domains of validity or of concepts—be it heuristically or more informed as suggested in [Lakatos, 1976].

Thus, I argue, there is an essential difference between the two word-pairs in the truth-values that they encompassed in the 19th century. A rule with an exception was nevertheless *a rule*—that just could not be applied to the exception. A theorem with a counterexample was *false* and hence was no longer a theorem!

Almost as importantly, mathematicians reacted differently to exceptions and to counterexamples—indeed, this may come closer to a true definition of the terms as I interpret and use them. A mathematician of the 18th century faced with an exception would accept it, perhaps ignore it, or perhaps try to understand it a bit better. On the other hand, a mathematician of the late 19th century who discovered or presented a counterexample would be expected to immediately realize that some result had been refuted. He or others would then set to work repairing it by carefully analyzing the counterexample and the restrictions it imposed on the domain of the theorem in question. Thus, we may be helped in distinguishing exceptions from counterexamples by analyzing how (and if) mathematicians reacted to their presence.

We have noticed repeatedly that Abel and his contemporaries were concerned with the *number of exceptions* to a given result. At first, this aspect may appear curious and of less importance, but provided the present interpretation, more sense can be made of it. Above, I have suggested that in the *formula-centered* approach to analysis, general results (rules) might survive the presentation of a few exceptions. Thus, Abel's concern for the infinite number of exceptions was a relevant way of pointing to the severity of the problem and to caution mathematicians to apply the questioned result with care.

79 I take this as my working definition of "rules" and have pointed to some similar 19th century uses of the term.

In summary, it appears plausible that the word "exception" could and should be taken very seriously as a counterpart to the rule-like nature of general statements. However, as must also be clear in the present context, Abel put forward an *exception* to something that Cauchy had called a *theorem*, not to an overly general rule but to an apparently precise mathematical theorem. I claim that the juxtaposition of *Theorem C* and Abel's *exception* is actually a confusion of the word-pairs that occurred in a time when a precise notion of theorem was just taking over from the previous emphasis on general rules. Thus, I suggest that in the interim period, a result (a rule) would be claimed to be a theorem even if it admitted some exceptions. K. Volkert has provocatively described such theorems as "statistically using the ∀-quantifier"[80]—i.e., they seem to claim to apply "for all" but actually (and with contemporary knowledge and acceptance) only apply to "almost all" or to "all but some few, uninteresting exceptions."

4.3. General arguments and exceptions

The mode of mathematics prevalent in the 18th century relied on a heavy apparatus of formal and explicit manipulations—therefore, I termed it the *formula-centered* style of mathematics. Because of the often cumbersome machinery, applicable results were not easily overthrown—not even when faced with a few exceptions. This was manifested—as will be illustrated below—in the fact that the paradigm included arguments "by generality" and results that were known to be true only "in general."

In the early 19th century, Scandinavian mathematicians—including Abel and Degen—spoke of "general" or "ordinary" arguments, principles, or rules.[81] Often, these referred to practices which Cauchy in 1821 labeled "the generality of algebra" and banned from his new rigorization of analysis.[82] In particular, these arguments would work with formal and general relationships between analytical expressions without regard for the numerical behavior when numerical values were inserted for the variables.

Examples of this mode of general reasoning can be found in Abel's works on elliptic and higher transcendental functions. One easily accessible example will have to serve as an introduction to this—by modern standards—strange way of reasoning. In 1826, Abel presented a paper to the *Académie des sciences*, in which he applied a very general, algebraic approach to the question of integrating algebraically related differentials, his highly celebrated, so-called *Paris mémoire*.[83] Despite its general approach and a large number of general, *concept-centered* results, this paper worked well within the *formula-centered*-style with pages of long manipulations following each other. One specific operation frequently used in the paper is particularly noteworthy in the present context.

Abel's *Paris mémoire* dealt with integrals—later called *abelian*—of the form $\int f(x, y)\,\mathrm{d}x$, where y was related to x by an algebraic equation $\chi(x, y) = 0$, and f was a rational function. At a crucial point of the argument, where Abel sought to determine the number μ of independent abelian integrals,[84] he employed a generalized degree operator that he denoted h. Just as is the case for the ordinary degree operator of polynomials, deg P (which Abel also used), the degree of a sum may fail to be the maximum

80 [Volkert, 1986, 144–145].

81 The Danish word "almindelige" can mean both "general" and "ordinary."

82 [Cauchy, 1821, ii–iii].

83 [Abel, 1841].

84 Abel found that for given f and χ, a certain number, μ, existed such that *any* sum of abelian integrals (with f, χ) could be reduced to μ such integrals.

of the two degrees,

$$\deg(P_1 + P_2) \stackrel{?}{=} \max\{\deg P_1, \deg P_2\}, \tag{12}$$

if $\deg P_1 = \deg P_2$. However, in the *Paris mémoire*, Abel was not interested in such peculiarities and he simply argued that the equality equivalent of (12) was true "in general," i.e., with the exception of some particular cases of little interest:

> Thus, one has in general, except in certain particular cases which I refrain from considering [...][85]

Later, the precise determination of μ (then termed the "genus") became a focal point of much research, e.g., in the works of Riemann.[86]

Other examples of a similar approach where peculiar special cases were ignored can be found in Abel's works. For instance, in the *Recherches*,[87] Abel studied the inversion of elliptic integrals (of the first kind) into *elliptic functions*. These new functions were known only through an abstract, formal inversion, and thus, for them to become well-established mathematical entities, Abel deduced various representations including infinite series and infinite products. All these representations were deduced formally, i.e., both (1) using formulae and long manipulations of such and (2) with no concern for the convergence of the involved infinite expressions. Only in his final paper on elliptic functions, the *Précis*, did Abel remark rather laconically on the convergence of the deduced expressions when he simply stated without further ado that "[the series] are always convergent."[88]

Also in the *Précis*, Abel used another notion related to exceptions—that of *restrictions*—when he stated that:

> The formulae presented above hold with certain restrictions if the modulus c is arbitrary, real or imaginary.[89]

These examples serve to illustrate that the concern for convergence and rigor that was so prevalent in Abel's paper on the binomial series was much less outspoken in his research on elliptic functions. They also illustrate that Abel—when facing useful *formula-centered* results—was prepared to accept certain restrictions in their domain of validity with just a side-remark that they were generally valid.

It is interesting to further investigate the backgrounds for this mode of general reasoning. One possibility is that it may be related to the relative immaturity of the field. When Abel entered into the field of elliptic and higher transcendental functions, these were studied only by a rather small group of mathematicians led by A.-M. Legendre (1752–1833). Legendre's presentational style was well within the *formula-centered* approach, involving and boasting long formal manipulations and various representations for different purposes. Without aiming to reduce the problem of general arguments to differences

85 "Alors on aura, en général, excepté quelques cas particuliers que je me dispense de considérer: [...]." [Abel, 1841, 162]

86 [Houzel, 1986, 310–313].

87 [Abel, 1827, 1828b].

88 [Abel, 1829, 244].

89 "Les formules présentées dans ce qui précède ont lieu avec quelques restrictions, si le module c est quelconque, réel ou imaginaire." [Abel, 1829, 245]

between fields of research, I suggest that Abel used two different modes of mathematical argument in different contexts, one *formula-centered* and one *concept-centered*. One indication of this can, I argue, be found in the references to "certain restrictions" that his "generally valid" results might suffer.

In summary, Abel often followed his predecessors in arguing "generally" and (consciously) neglecting certain cases of little interest where the deductions could be imprecise. In the period dominated by the *formula-centered* approach to analysis, such deductions were extremely valuable and the presupposition of intuition on behalf of the reader was not a general problem. However, with the advent of a new approach with much greater emphasis on *precisely formulated theorems*, such exceptions were no longer tolerated. Therefore, we now turn to one other particular example from Abel's mathematical production that will help us appreciate this observation.

4.4. Counterexamples in Abel's criticism of Olivier

As already mentioned, Abel did not publish much on the implementation of Cauchy's new analysis. However, on one other occasion—prompted by a paper by a certain L. Olivier in Crelle's *Journal für die Reine und Angewandte Mathematik*—Abel published on possible tests of convergence for infinite series—tests that were central to Cauchy's approach to the subject. In 1827, Olivier presented a very simple criterion of convergence that can be expressed as

$$\sum_{n=0}^{\infty} a_n \text{ convergent} \quad \Longleftrightarrow \quad na_n \to 0 \quad \text{when } n \to \infty.^{90} \tag{13}$$

Thus, Olivier's claim can be interpreted as stating that the harmonic series precisely determines the border between convengence and divergence.

In a very short and concise argument, published in the same journal the following year, Abel criticized Olivier's result. First, he showed directly that the series $\sum_{n=2}^{\infty} \frac{1}{n \log n}$ would contradict Olivier's criterion. And following this counterexample, he demonstrated that it is impossible to construct a test function $\phi(n)$ determining completely the convergence of any series in the way Olivier had sought to in his criterion (13): That is, no function $\phi(n)$ exists such that for all series $\sum a_n$,

$$\sum_{n=0}^{\infty} a_n \text{ convergent} \quad \Longleftrightarrow \quad \phi(n)a_n \to 0 \quad \text{when } n \to \infty.^{91} \tag{14}$$

Abel based his proof on a process by which he could derive a divergent series from a convergent one and vice versa. More precisely, Abel showed that if the existence of a test function $\phi(n)$ was assumed, the series $\sum_{n=1}^{\infty} \frac{1}{\phi(n)}$ provided a counterexample to (14).[92]

---

[90] [Olivier, 1827].

[91] [Abel, 1828a]; Olivier then responded by a brief remark revealing some of his geometrical arguments but essentially accepting Abel's observations [Olivier, 1828].

[92] It may be noted that in this argument, Abel actually demonstrated that sound reasoning about divergent series may lead to interesting and important results—somewhat contrary to his assertion that "it is shameful to base any argument on divergent series" [Abel to Holmboe, 1826/01/16; Abel, 1902, 16]. The main difference, of course, lies in the completely different attitudes

In the present context, Abel's evaluation of his opponent's argument is noteworthy, as is his use of examples to make his point. In the published version, Abel was very cautious stating only that one of the implications in the asserted "theorem is very true but the [other one, i.e. the implication toward the left in (13)] seems not to be so" and that "the theorem announced in the above quotation is thus defective in this case."[93] In his notebooks, however, we find Abel expressing a stronger position when he stated that "[t]hus, Mr. Olivier has seriously misled himself."[94] We see Abel expressing himself carefully about the status of the theorem that he had completely shattered by presenting not only a specific counterexample but a very general argument showing the invalidity of any such criterion. This might suggest an interpretation of Abel's words in terms of diplomacy—but the suggested interpretation contrasting exceptions and counterexamples appears to explain more, in particular how Abel reacted to these. In the binomial paper, Abel simply noted the *exception* to *Theorem C* and went on to present a different version of the theorem that he must have believed (1) was sufficient, and (2) did not suffer from the same problems. This is the type of reasoning that I associate with the *formula-centered* approach. In his criticism of Olivier, Abel made quite a point out of the counterexample and went one step further into analyzing the possibility of such criteria, thereby beginning to describe the class of convergence tests. Thus, in this context, Abel used the counterexample in a more modern (*concept-centered*) sense and began to do proof analysis.[95] Thus, I interpret Abel's responses to Cauchy and Olivier as being quite different—one being an *exception* within the *formula-centered* approach, the other being a *counterexample* in the *concept-centered* style.

5. Conclusions

In the previous sections, we have seen how Abel's *exception* was not an isolated incidence in the early 19th century. We have seen how mathematicians used the same word—"exception"—to designate a particular kind of example sometimes used in a special kind of critical argument. And we have seen how the preference for general arguments prevailed into the 19th century. It is now time to return to the framework of *formula-centered* and *concept-centered* mathematics to arrive at some overall conclusions from these observations.

D. Laugwitz captured the essence of the transition in analysis in the period by stating that "[b]efore Cauchy, the calculus had dealt with expressions."[96] I mean to elaborate on this, claiming that analysis in the 18th century essentially dealt with formulae: arguments consisted of manipulations of formulae, questions concerned formulae, and formulae were obtained as the results. This preoccupation with formulae—which I term *formula-centered* mathematics—was gradually replaced by a "most elevated viewpoint" in the course of the 19th century.[97] This new outlook on mathematical analysis had *concepts*

toward the property of being "divergent." The illegitimate use of divergent series of which Abel spoke was tied to their manipulations and therefore to the *formula-centered* paradigm. However, Abel's present argument made use of divergence as a property (the complement of convergence) and by investigating the delineation of these concepts, it was basically *concept-centered*.

93 [Abel, 1828a, 79].

94 "Donc M. *Olivier* s'est trompé sérieusement." [Abel, 1827/1881, II, 199]

95 Thus, with Lakatos, we may see Abel in the binomial paper as an "exception barrer," whilst in debating with Olivier, the "counterexample opened up a new field of research." [Lakatos, 1976, 133–136, 128]

96 [Laugwitz, 1994, 319].

97 Weierstrass praised Abel's "most elevated point of view," see excerpts of quotations in [Biermann, 1966, 218].

for its basic objects and is therefore described as *concept-centered* mathematics.[98] In the *concept-centered* approach, new types of questions arose and were answered by analyses of the extensions of concepts and of relations between concepts. This transition from the *formula-centered* to the *concept-centered* outlook was deep and affected both ontological and epistemological levels of mathematics. Here, I restrict myself to applying this scheme to offer a perspective on the exceptions, rules (understood as general statements), theorems, and proofs of the early 19th century.

The great concern for formulae in the *formula-centered* approach meant that mathematicians working within this framework were aware that their results (formulae) might not apply in certain cases. We have seen that from time to time, their results were described as "rules," and we perceive these as generalized, formal statements. The "exceptions" that these results, theorems, or rules might admit were intuitively excluded when applications had to be made, because being the masters of their discipline, mathematicians knew how to apply their intuitions. However, to younger people entering the field of mathematics in the 19th century, results were thought of as *theorems* and, in a period of transition, even theorems were admitting exceptions. When the status of various mathematical results were firmly settled within the *concept-centered* approach, such exceptions became counterexamples that were skillfully employed to test the limits of concepts and proofs—and to refute theorems.

With this background in mind, it made sense in the early 19th century to speak of "exceptions"—and they did not just reduce to counterexamples. A theorem—just like a formula—might in this interim period be extremely useful, even if it failed to apply to a few examples that nominally fell within its domain. It was only with the new critical approach—and the associated critical revision—that the exceptions became untenable. By modifying statements so that they no longer admitted exceptions, mathematicians advanced their science on the way to a wholely *concept-centered* mathematics.

As outlined here, this transition helps us understand the stress on the number of exceptions and the variation in the attitudes and reactions toward results known to admit exceptions. It suggests that in the *formula-centered* approach, a singular exception was insufficient reason to overturn an otherwise applicable and useful result, and that mathematicians—when faced with a result allowing for exceptions—would attempt to provide a different, restricted result having sufficient strength but avoiding the problems discovered in the previous result. On the other hand, after the transition toward *concept-centered* analysis, when emphasis was put on the precise statement of theorems, the critical revision of previous results into precise theorems was inevitable and required. Then, it became important to precisely determine and characterize the exceptions and their numbers and to include this information into the theorems by a process comparable to Lakatos' schematized proof-analysis.

Similarly, the framework adds a perspective on the choice of words for the outstanding examples. When Abel called attention to the "exception" that *Theorem C* suffered, his choice of words reflected more than merely respect for Cauchy. Abel was using a vocabulary that when applied in the *formula-centered* approach let mathematicians use their intuitions to avoid absurd cases. Similarly, when Abel on other occasions spoke of "restrictions" without specifying them, he was referring to the same general validity of results. Later, in the *concept-centered* paradigm, such terms were transformed into specified and explicit "conditions," and mathematicians used "counterexamples" to differentiate between concepts and precisely determine the domains of validity of theorems. Abel's use of the word "paradox" is an

[98] Versions of this transition-framework have appeared in the literature during the past decades; see, e.g., [Jahnke, 1987; Gray, 1992; Laugwitz, 1999].

intermediary one which resulted from combining the statements of the *formula-centered* approach with the criticism of the *concept-centered* approach.

By now, it should also be evident that the transition from a *formula-centered* mathematics to a *concept-centered* outlook was not immediate. The transition was an extended one that lasted for at least a few decades of the early 19th century. To some mathematicians working during the transition, the change was felt, but as the example of Abel illustrates, they were sometimes too deeply immersed in the mathematical style to deliberately and explicitly choose sides: Abel is an interesting, intermediary, and transitional figure in this respect.

Acknowledgments

This paper was prepared with financial support from the History of Science Department (University of Aarhus), the Dutch Research Organisation (NWO), and the Danish Natural Science Research Council (SNF). In preparing it, I have profited immensely from the critical and constructive comments of Professors Kirsti Andersen and Henk Bos, as well as comments from two anonymous referees and the editor. For this, I am very grateful.

References

Abel, N.H., 1826. Untersuchungen über die Reihe $1 + \frac{m}{1}x + \frac{m\cdot(m-1)}{2}x^2 + \frac{m\cdot(m-1)(m-2)}{2\cdot 3}x^3 \ldots$ u.s.w. Journal für die Reine und Angewandte Mathematik 1 (4), 311–339. Reproduced in: [Abel, 1881, I, 219–250].

Abel, N.H., 1827/1881. Sur les séries. In: [Abel, 1881], pp. 197–205, vol. 2. Undated manuscript, probably written in the second half of 1827.

Abel, N.H., 1827. Recherches sur les fonctions elliptiques. Journal für die Reine und Angewandte Mathematik 2 (2), 101–181. Reproduced in: [Abel, 1881, I, 263–388].

Abel, N.H., 1828a. Note sur le mémoire de Mr. L. Olivier No. 4. du second tome de ce journal, ayant pour titre “Remarques sur les séries infinies et leur convergence”. Journal für die Reine und Angewandte Mathematik 3 (1), 79–82. Reproduced in: [Abel, 1881, I, 399–402].

Abel, N.H., 1828b. Recherches sur les fonctions elliptiques. Journal für die Reine und Angewandte Mathematik 3 (2), 160–190. Reproduced in: [Abel, 1881, I, 263–388].

Abel, N.H., 1829. Précis d’une théorie des fonctions elliptiques. Journal für die Reine und Angewandte Mathematik 4 (4), 236–277, 309–348. Reproduced in: [Abel, 1881, I, 518–617].

Abel, N.H., 1839. Oeuvres Complètes de N.H. Abel, mathématicien, avec des notes et développements, edited by B.M. Holmboe, first ed. Chr. Gröndahl, Christiania.

Abel, N.H., 1841. Mémoire sur une propriété générale d’une classe très étendue de fonctions transcendantes. In: [Abel, 1881, pp. 145–211, volume 1]. First published Mémoires présentés par divers savants, t. VII, 1841. Delivered to l’Académie des sciences on October 26, 1826.

Abel, N.H., 1881. Oeuvres Complètes de Niels Henrik Abel, edited by L. Sylow and S. Lie, second ed. Grøndahl, Christiania.

Abel, N.H., 1902. Breve fra og til Abel. In: [Holst et al., 1902a]; also published with French translations in: [Holst et al., 1902b].

Bérard, 1819. Sur le nombre des racines imaginaires des équations; en réponse aux articles de MM. Tédenat et Servois, insérés aux pages 215 et 223 de ce volume. Annales de Mathématiques Pures et Appliquées 9 (11), 345–372.

Biermann, K.-R., 1966. Karl Weierstraß. Ausgewählte Aspekte seiner Biographie. Journal für die Reine und Angewandte Mathematik 223, 191–220.

Bottazzini, U., 1986. The Higher Calculus: A History of Real and Complex Analysis from Euler to Weierstrass. Springer-Verlag, New York.

Briot, Bouquet, 1853. Note sur le développement des fonctions en séries convergentes ordonnées suivant les puissances croissantes de la variable. Comptes rendus hebdomadaires des séances de l'Académie des Sciences 36, 334–335. Dated 21 February.

Cauchy, A.-L., 1821. Cours d'analyse de l'École Royale Polytechnique. Premier partie. Analyse algébrique. l'Imprimerie Royale, Paris.

Cauchy, A.-L., 1853. Note sur les séries convergentes dont les divers termes sont des fonctions continues d'une variable réelle ou imaginaire, entre des limites donné. In: Œuvres Complètes d'Augustin Cauchy. vol. I.12 of 1st series. Gauthier–Villars, Paris, pp. 30–36. Read to the Académie des Sciences on March 14, 1853 and originally published in Comptes Rendus, vol. 36, p. 454.

Degen, C.F., 1802. Bidrag til det mathematiske Studiums Kritik, eller: Betragtninger over hvad der udfordres til Mathematikens heldige Dyrkelse. Første Afdeling. Det kongelige danske Videnskabers-Selskabs Skrivter 2 (2), 75–153.

Dirichlet, L., 1852. Gedächtnisrede auf Carl Gustav Jacob Jacobi. In: C.G.J. Jacobi's Gesammelte Werke, edited by C.W. Borchardt, vol. 1. Reimer, Berlin, pp. 3–28. Reprinted 1969, Chelsea Publishing Company, New York.

du Bois-Reymond, P., 1871. Notiz über einen Cauchy'schen Satz, die Stetigkeit von Summen unendlicher Reihen betreffend. Mathematische Annalen 4, 135–137.

Euler, L., 1754. Subsidium calculi sinuum. In: Leonhardi Euleri *Opera Omnia*. vol. I:14 of 1st series. Teubner, Leipzig/Bern, pp. 542–584. First published Novi commentarii academiae scientiarum Petropolitanae 5 (1754/1755) 1760, pp. 164–204.

Euler, L., 1775. Demonstratio theorematis Neutoniani de evolutione potestatum binomii pro casibus quibus exponentes non sunt numeri integri. In: Leonhardi Euleri *Opera Omnia*. vol. I:15 of 1st series. Teubner, Leipzig/Bern, pp. 207–216. First published Novi commentarii academiae scientiarum Petropolitanae 19 (1774/1775) 103–111.

Fischer, G.M., 1978. Cauchy and the infinitely small. Historia Math. 5 (3), 313–331.

Fourier, J., 1822. Théorie analytique de la chaleur. In: Œuvres de Fourier, vol. 1. Gauthier–Villars et fils, Paris. First published Firmin Didot, Père et fils, Paris.

Fraser, C.G., 1987. Joseph Louis Lagrange's algebraic vision of the calculus. Historia Math. 14 (1), 38–53.

Fraser, C.G., 1988–1989. The Calculus as algebraic analysis: Some observations on mathematical analysis in the 18th century. Archive for History of Exact Sciences 39, 317–335.

Gillies, D. (Ed.), 1992. Revolutions in Mathematics. Clarendon, Oxford.

Giusti, E., 1984. Gli "errori" di Cauchy e i fondamenti dell'analisi. Bollettino di Storia delle Scienze Matematiche 4 (2), 24–54.

Grabiner, J.V., 1981a. Changing attitudes toward mathematical rigor: Lagrange and analysis in the eighteenth and nineteenth centuries. In: Jahnke, H.N., Otte, M. (Eds.), Epistemological and Social Problems in the Sciences in the Early Nineteenth Century. Reidel, Dordrecht, pp. 311–330.

Grabiner, J.V., 1981b. The Origins of Cauchy's Rigorous Calculus. MIT Press, Cambridge, MA.

Grattan-Guinness, I., 1970. The Development of the Foundations of Mathematical Analysis from Euler to Riemann. MIT Press, Cambridge, MA.

Grattan-Guinness, I., 1972. Joseph Fourier 1768–1830. A Survey of His Life and Work, Based on a Critical Edition of His Monograph on the Propagation of Heat, Presented to the Institut de France in 1807. MIT Press, Cambridge, MA.

Grattan-Guinness, I., 1986. The Cauchy–Stokes–Seidel story on uniform convergence again: Was there a fourth man? Bulletin de la Société Mathématique de Belgique (Serie A) 38, 225–235.

Gray, J., 1992. The nineteenth-century revolution in mathematical ontology. In: [Gillies, 1992], Ch. 12, pp. 226–248.

Hansteen, C., 1835. Lærebog i Plangeometrie. Johan Dahl, Christiania.

Hessel, 1832. Nachtrag zu dem Eulerschen Lehrsatze von Polyëdern. Journal für die Reine und Angewandte Mathematik 8 (1), 13–20.

Hilbert, D., 1897. Die Theorie der algebraischen Zahlkörper. In: David Hilbert. Gesammelte Abhandlungen, second ed. vol. 1. Springer-Verlag, Berlin/Heidelberg/New York, pp. 63–361. First published Jahresbericht der Deutschen Mathematiker-Vereinigung 4 (1897) 175–546.

Holmboe, B., Dec. 1835. Om Professor Hansteens nye Parallellære (i hans nylig udkomne Lærebog i Plangeometrie, forlagt af Johan Dahl, Christiania 1835). Morgenbladet—En daglig Avis af alle Slags Indhold 17 (339).

Holst, E., 1902. Historisk Indledning. In: Holst et al. [1902a].

Holst, E., Størmer, C., Sylow, L. (Eds.), 1902a. Festskrift ved Hundredeaarsjubilæet for Niels Henrik Abels Fødsel. Jacob Dybwad, Kristiania.

Holst, E., Størmer, C., Sylow, L. (Eds.), 1902b. Niels Henrik Abel. Memorial publie a l'occasion du centenaire de sa naissance. Jacob Dybwad, Kristiania.

Houzel, C., 1986. Fonctions elliptiques et intégrales abéliennes. In: Dieudonné, J. (Ed.), Abrégé d'histoire des mathématiques. Hermann, Paris. Ch. 7, pp. 293–314.

Jahnke, H.N., 1987. Motive und Probleme der Arithmetisierung der Mathematik in der ersten Hälfte des 19. Jahrhunderts—Cauchys Analysis in der Sicht des Mathematikers Martin Ohm. Archive for History of Exact Sciences 37, 101–182.

Jahnke, H.N., 1990. Mathematik und Bildung in der Humboldtschen Reform, Studien sur Wissenschafts-, Social- und Bildungsgeschichte der Mathematik, No. 8. Vandenhoeck & Ruprecht, Göttingen.

Jahnke, H.N., 1999. Die algebraische Analysis des 18. Jahrhunderts. In: Jahnke, H.N. (Ed.), Geschichte der Analysis. Texte zur Didaktik der Mathematik. Spektrum Akademischer Verlag, Heidelberg/Berlin. Ch. 4, pp. 131–170.

Jürgensen, C., 1836. Lærebog i Plangeometrie af Chr. Hansteen. Christiania 1835. XX. og 280 S. 8vo. Maanedsskrift for Litteratur, Udgivet af et Selskab 15, 446–472.

Koetsier, T., 1991. Lakatos' Philosophy of Mathematics. A Historical Approach, Studies in the History and Philosophy of Mathematics, No. 3. North-Holland, Amsterdam.

Kuhn, T.S., 1962. The Structure of Scientific Revolutions. The University of Chicago Press, Chicago/London.

Lagrange, J.-L., 1826. Traité de la résolution des équations numériques de tous les degrés, avec des notes sur plusieurs points de la théorie des équations algébriques, third ed. Bachelier, Paris.

Lakatos, I., 1976. Proofs and Refutations. The Logic of Mathematical Discovery. Cambridge Univ. Press, Cambridge.

Lakatos, I., 1978. Cauchy and the continuum: the significance of non-standard analysis for the history and philosophy of mathematics. In: Mathematics, Science and Epistemology, edited by J.P. Cleave, Philosophical Papers. vol. 2. Cambridge Univ. Press, Cambridge/London/New York/Melbourne. Ch. 3, pp. 43–60.

Laugwitz, D., 1987. Infinitely small quantities in Cauchy's textbooks. Historia Math. 14, 258–274.

Laugwitz, D., 1994. Real-variable analysis from Cauchy to non-standard analysis. In: Grattan-Guinness, I. (Ed.), Companion Encyclopedia of the History and Philosophy of the Mathematical Sciences. Routledge, London. Ch. 3.3, pp. 318–330.

Laugwitz, D., 1999. Bernhard Riemann 1826–1866. Turning Points in the Conception of Mathematics, translated by A. Shenitzer from the 1996 German original. Birkhäuser, Boston/Basel/Berlin.

Lhuilier, 1812/1813. Mémoire sur la polyèdrométrie; contenant une démonstration directe du Théorème d'Euler sur les polyèdres et un examen des diverses exceptions auxquelles ce théorème est assujetti. Annales de mathématiques pures et appliquées 3, 168–189.

Minkowski, H., 1905. Peter Gustav Lejeune Dirichlet und seine Bedeutung für die heutige Mathematik. Jahresbericht der Deutschen Mathematiker-Vereinigung 14, 149–163.

Olivier, L., 1827. Remarques sur les séries infinies et leur convergence. Journal für die Reine und Angewandte Mathematik 2 (1), 31–44.

Olivier, L., 1828. Remarque de Mr. L. Olivier. Journal für die Reine und Angewandte Mathematik 3 (1), 82.

Reich, K., 1989. Lehrbücher. In: Folkerts, M., et al. (Eds.), Maß, Zahl und Gewicht: Mathematik als Schlüssel zu Weltverständnis und Weltbeherrschung. Acta Humaniora, VCH, Weinheim. Ch. 9, pp. 216–240.

Robinson, A., 1966. Non-standard Analysis. Studies in Logic and the Foundations of Mathematics. North-Holland, Amsterdam.

Spalt, D.D., 2002. Cauchys Kontinuum. Eine historiografische Annäherung via Cauchys Summensatz. Archive for History of Exact Sciences 56, 285–338.

Stubhaug, A., 2000. Niels Henrik Abel and His Times. Called too soon by Flames Afar, translated by R.H. Daly. Springer-Verlag, Berlin.

Sørensen, H.K., 2002a. Abel and his mathematics in contexts. NTM: International Journal of History and Ethics of Natural Sciences, Technology and Medicine 10, 137–155.

Sørensen, H.K., 2002b. The Mathematics of Niels Henrik Abel—Continuation and New Approaches in Mathematics during the 1820s. Ph.D. thesis, History of Science Department, University of Aarhus, Aarhus, Denmark.

Volkert, K.T., 1986. Die Krise der Anschauung. Eine Studie zu formalen und heuristischen Verfahren in der Mathematik seit 1850. Studien zur Wissenschafts-, Sozial- und Bildungsgeschichte der Mathematik, No. 3. Vandenhoeck & Ruprecht, Göttingen.

Walter, W., 1985. Analysis I. Springer-Verlag, Berlin.