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The Mathematics of NIELS HENRIK ABEL

Continuation and New Approaches in Mathematics During the 1820s

HENRIK KRAGH SØRENSEN

For Mom and Dad

who were always there for me
when I abandoned
all good manners,
good friends,
and common sense
to pursue my dreams.

The Mathematics of NIELS HENRIK ABEL

Continuation and New Approaches in Mathematics During the 1820s

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PhD dissertation
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This dissertation was submitted to the Faculty of Science, University of Aarhus in March 2002 for the purpose of obtaining the scientific PhD degree. It was defended in a public PhD defense on May 3, 2002. A second, only slightly revised edition was printed October, 2004.
The PhD program was supervised by associate professor KIRSTI ANDERSEN, History of Science Department, University of Aarhus.
Professors UMBERTO BOTTAZZINI (University of Palermo, Italy), JEREMY J. GRAY (Open University, UK), and OLE KNUDSEN (History of Science Department, Aarhus) served on the committee for the defense.

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For further information, additions, corrections, and contact to the author, please refer to the website `http://www.henrikkragh.dk/phd/`.

The picture on the front page is a painting of NIELS HENRIK ABEL performed by the Norwegian painter JOHAN GØRBITZ during ABEL's time in Paris 1826. It is the only authentic depiction of ABEL and is reproduced from (Ore, 1957).
The picture on the reverse shows a curlicue frequently used by ABEL in his notebooks to mark the end of manuscripts. It is reproduced from (Stubhaug, 1996).

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Summary

The present PhD dissertation uses the mathematics of the Norwegian N. H. ABEL (1802–1829) as a framework for describing and analyzing trends in the development of mathematics during the first half of the nineteenth century. ABEL'S mathematics is read and interpreted in its context and used to describe a fundamental change in mathematics in the early nineteenth century in which concepts replaced formulae as the basic objects of mathematics.

The dissertation is structured into five parts: 1) an introductory part consisting of biographical and other historical framework, 2) three descriptive parts each devoted to a particular theme analyzed from a particular discipline in ABEL'S mathematical production, and 3) a part comprising the syntheses of a general transition in mathematics in the early nineteenth century as seen from the perspective of ABEL'S works.

Introduction. In the introductory part, ABEL'S biography is described to point out some of the formative instances in the creation of one of the important mathematicians of the first half of the nineteenth century. Because ABEL'S biography has been written repeatedly — and recently in an excellent cultural biography — the biography is only intended to locate ABEL'S production in its contexts of his life and the mathematics of his time.

New questions: Algebraic solubility. The first of the three studies of ABEL'S mathematics deals with his contributions to the theory of equations. It is illustrated how ABEL was led to ask a new kind of question of the solubility of equations which would have seemed both counter intuitive and futile to mathematicians a few generations before. With the foundation in works of J. L. LAGRANGE (1736–1813) and A.-L. CAUCHY (1789–1857), ABEL was able to prove that the algebraic solution of general quintic equations was impossible. This result restricted the class of solvable equations and separated it from the class of all polynomial equations. In another line of research, ABEL proved that an extensive class of equations — later called *Abelian equations* — were algebraically solvable. Compared with the previous result, the solubility of the *Abelian* equations showed that the extension of the concept of solvable equations did not collapse. Subsequently, this branch of the theory of equations became a question of delineating the extension of solvable equations, i.e. of drawing

the border between solvable and unsolvable equations by some other criterion. ABEL commenced research on this issue but had to leave it incomplete. In part II, ABEL'S research on all these issues is carefully analyzed based on the works of his main predecessors and contemporaries. The reception of ABEL'S research and the subsequent development of the theory is also addressed.

New epistemic standards: Rigorization. ABEL'S devotion to and adaption of the rigorization movement spearheaded by CAUCHY is the topic of part III. By describing ABEL'S critical attitude towards the existing practices of rigor and his publications on the binomial theorem and a certain type of criteria of convergence, it is illustrated how the new epistemic standards were manifesting themselves in a period of rapid transition in analysis. Starting with a description of the Eulerian focus on algebraic equality, it is described how CAUCHY'S new emphasis on arithmetical equality effected the central concepts of continuous functions and converget series. Furthermore, the so-called *exception* which ABEL presented to a theorem by CAUCHY is treated in some detail because it will later prove to be an important example in the description of the change from formula based to concept based mathematics.

New objects: Elliptic and higher transcendentals. The third and final pillar of ABEL'S mathematics which is treated in the present dissertation concerns his works on elliptic and higher transcendentals. Selected aspects of his work are again presented and discussed from a diachronical viewpoint. In this case, special emphasis is given to the way ABEL was led to his formal inversion of elliptic integrals into elliptic functions. Furthermore, ABEL'S means of obtaining workable representations of the formally defined object is described. Thereafter, special attention is paid to the techniques which he employed in studying transcendentals and it is illustrated how *algebraic* methods figured prominently in his toolbox. In the process, it is also described how his style of argument often relied essentially on manipulations of formulae in ways which could sometimes lead to results which were true "in general". Finally, the changing internal relationships between definitions and results are illustrated by describing various ways towards a general theory of elliptic functions.

Syntheses. In the ultimate part, the preceding descriptions and discussions of ABEL'S mathematics are thrown into perspective by arguing that the development of mathematics in the early nineteenth century can be understood as a change of paradigms: An Eulerian, formula based paradigm is contrasted with a concept based paradigm. Various aspects of ABEL'S works — including delineation problems, ABEL'S *exception*, and the nature of arguments which are only true "in general" — are then all interpreted based on this transition in paradigms.

Preface to the 2004 edition

For this second edition, some minor changes have been made to the initial version, which was handed in on March 27, 2002 and defended on May 3 the same year. The changes include a number of corrections of misprints, a revision of the layout and the figures, and the addition of a name index, a subject index, and a list of boxes.[1] Furthermore, I have included a new preface below, which elaborates on the general theme of dissertation, namely the analysis of NIELS HENRIK ABEL'S (1802–1829) mathematics within a transition from formula based to concept based mathematics. This new "introduction" is an updated and distilled version of the lecture which I gave at the defence in May 2002.

Funded by grants from the Faculty of Science (Aarhus) and from the Netherlands' Organization for Scientific Research (NWO), I have continued to elaborate on this transitional framework, producing two papers focusing on particular aspects such as critical revision, exceptions, and habituation — processes which are discussed in the present work. One of these papers is currently accepted for publication in *Historia Mathematica*, (Sørensen, 2005); the other remains in the pipeline and should be submitted soon. In the future, I will continue to elaborate on the general analytical framework and its impact on analysing ABEL'S mathematics.

Abel's mathematics in the context of traditions and changes

The aim of the dissertation was twofold: to describe and analyse ABEL'S mathematics within its historical context and to draw perspectives on the general development of mathematics in the early nineteenth century from the Abelian corpus of mathematics.

The analyses which are drawn from ABEL'S mathematics are — obviously — related to it, and it has been my main ambition to point to general developments which shed light on ABEL'S mathematics. That is to say, I do not claim that these trends are independent of ABEL'S mathematics and could (or should) apply to other periods of time, other topics of mathematics, or other mathematicians. However, it is my convic-

[1] I am grateful for the corrections which were pointed out to me by OLE HALD and by many other friendly people. However, if some misprints have endured, I would not be too surprised and therefore beg the forgiveness of the reader.

tion that they are genuine themes, and I have sought to develop and document this position in this dissertation and in my subsequent research.

The main theme, which I have idenfied and used to structure and analyse ABEL'S mathematics is one of a transition from a predominantly formula based approach to mathematics towards a more concept based one.

The formula based approach to mathematics can be found most clearly in the works of such mathematicians of the eighteenth century as L. EULER (1707–1783) or A.-M. LEGENDRE (1752–1833). These mathematicians approached analysis (including the theory of equations) in a formula based way, in which *formulae* were the central objects of mathematics. Their mathematics — to a large extend — consisted of manipulations of formulae which produced new formulae as results.

As a counterpart to the formula based approach, I introduce concept based approach of the nineteenth century. This way of thinking about and doing mathematics was championed by such mathematicians as G. P. L. DIRICHLET (1805–1859) and G. F. B. RIEMANN (1826–1866). They thought mainly in terms of *concepts*, and their mathematics consisted of researching the relations between concepts, including representations of concepts, extensions of concepts, and the precise delineation of concepts.

This framework of formula based and concept based mathematics has helped me to organise my study of ABEL'S mathematics because it explains several particular aspects and provides an organising scheme. Thus, in the second part — on the theory of algebraic solubility — the framework provides an explanation and structuring of what I term the *delineation problem*: finding the precise extension of the quality of solvable equations. In the third part (rigorisation of analysis), the framework suggests that mathematicians were struggling with changing conceptions about *their objects*, not just the ways of manipulating them. This led to the necessity for *critical revision* which is also better understood within a framework of transitions. And in the fourth part (elliptic and higher transcendentals), the framework finally suggests that *habituation* — coming to know new objects — was a prominent problem which played a part in the shaping of concept based mathematics.

This extremely brief outline of the suggestive and explanatory powers of the framework is meant to justify and explain the organising principle of the dissertation. In my subsequent research, I have worked to expand more on the framework and its analytical powers.

Recent literature

Since the dissertation was submitted in 2002, new literature on ABEL and his mathematics has emerged. In particular, ABEL'S *Paris memoir* was located — first partially, then completely; see (Del Centina, 2002; Del Centina, 2003). I am grateful to NILS VOJE JOHANSEN for providing me with a xerox copy of the famous treatise. Later, in connection with the Abel centennial in Oslo 2002, the proceedings were published (Laudal and Piene, 2004). These contain a mathematical introduction by CHRISTIAN HOUZEL which elaborates on the previous publications of this scholar. Besides these, a number of other publications deal with ABEL'S mathematics and his topics; these include (Radloff, 2002) which sheds interesting light on ABEL'S theory of solubility and (Pesic, 2003). In my future research and publishing on ABEL'S mathematics, these will provide good possibilities for discussions. Although this list is not complete and no use of these publications has been made in the present work, they are cited here for the convenience of the reader.

Preface to the 2002 edition

The present PhD dissertation grew out of long held curiosity towards the multifaceted transformation which mathematics underwent during the nineteenth century. In this respect, the project is about how mathematics came to have a form recognizable to us as modern mathematics. As a pragmatic and useful tool, KIRSTI ANDERSEN — who supervised the project — suggested studying the mathematics of ABEL in order to get a firmer hold of the transition which was thereby also restricted to a shorter period in the first half of the nineteenth century. Based on detailed and often cumbersome studies of ABEL'S mathematics, some aspects of the transition stand out which are here described and analyzed from the perspective of concept based mathematics.

Layout

Quotations. Quotations are used extensively to convey the authentic arguments and thoughts. All quotations are presented in English in the text with the original included in a footnote. This method has been chosen because it increases the flow of the main text. The translations are sometimes based on existing translations; in such cases references are given. Otherwise, the translations have been made by the author. Throughout, small-caps have been reserved for names mentioned in the text. For this reason, small-caps found in quotations have been replaced by bold-face.

References and footnotes. References to the bibliography are given in the footnotes and consist of the name(s) of the author(s) and the year of (original) publication. Some items are referred to through collected works or other compilations; in such cases, the bibliography contains both the publication in the collection (primary) and the original means of publication (secondary). For papers published in A. L. CRELLE'S *Journal für die reine und angewandte Mathematik*, the original publication is always the primary one. In all cases, page references relate to the primary method of publication mentioned in the bibliography. Thus, for instance, (N. H. Abel, 1826f, 311) refers to the first page of ABEL'S binomial paper as published in CRELLE'S *Journal für die reine und angewandte Mathematik* whereas (L. Euler, 1760, 585) refers to the first page of EULER'S paper as printed in the *Opera*. In the bibliography, authors are listed alphabetically ordered by their last name. Items by the same author are listed chronologically and potential items published in the same year are separated by letters. There are a few exceptions to these rules — mainly concerning Abel's publications. All collected works and a few other items have been given more illustrative names, e.g. (N. H. Abel, 1839; N. H. Abel, 1902e) which denote the first edition of Abel's *Œuvres* and the Norwegian *Festschrift* of 1902, respectively. Some of Abel's manuscripts have been dated and published posthumously; for these, the year they were written is included in brackets as in (N. H. Abel, [1828] 1839). References to Abel's manuscripts and notebooks are of the form (Abel, MS:351:A). Letters are refered to as (Abel→Holmboe, Kjøbenhavn, 1823/06/15. In N. H. Abel, 1902a, 3–4), which denotes the letter from Abel to B. M. Holmboe (1795–1850) sent from Copenhagen on June 15, 1823. I have only used published letters, and the references are given.

Mathematics and notation. It has been my general ambition to unwrap and disentangle the mathematics presented in the dissertation to such a degree that the reader who holds no particular knowledge of the topics discussed but is familiar with mathematical reasoning and mathematical notation should be able to benefit from the arguments and analyses. At the same time, it has been a high priority of mine to present the mathematics produced in the early nineteenth century in a way which respects and represents the way its creators thought about it. However, I have introduced a minimum of notational advances, in particular combining sums into the modern notation using the summation sign. I have also occasionally renumbered indices or replaced symbols to ease the notation. Throughout, I write Σ_n for the symmetric group on n symbols, which is elsewhere frequently referred to as S_n. Slightly off-topic mathematical themes have been placed in boxes shaded gray.

Names and portraits. Upon first mention, historical actors are listed with their full Christian names and years of birth and death according to the *Dictionary of Scientific Biography*.[2] In situations where the person is not included in the *Dictionary of Scientific Biography*, other sources are employed and explicitly referred to.[3] Full names and dates of important persons are sometimes repeated in various parts. Unless otherwise noticed, all pictures stem from the history of mathematics internet archives at St. Andrews, Scotland.[4]

Acknowledgments

My utmost gratitude extends towards my supervisor, Kirsti Andersen. For more than four years, Kirsti has always kindly guided me and put up with my changing moods. Kirsti has read most of the chapters of the present dissertation while

2 (Gillispie, 1970–80).
3 Mostly (Biermann, 1988; Poggendorff, 1965; Stubhaug, 1996).
4 `http://www-history.mcs.st-and.ac.uk/history/BiogIndex.html`.

they were under production. The endless effort which she put into her constructive criticism has made my presentation more easily understandable and sharpened my arguments considerably. For this, I am infinitely grateful.

Benefitting from my supervisor's elaborate network, I have had the opportunity to discuss aspects of my project with experts who have all been extremely forthcoming. During my visit with professor H. J. M. BOS in Utrecht in November and December 1998, the foundation for the overall structure of the present work was laid and I am greatly indebted to professor BOS for his always timely suggestions and continued kind interest. In May and June of 2000, I had the opportunity to visit professor UMBERTO BOTTAZZINI in Palermo. Discussing my progress with one of the greatest experts in the field was an unforgettable experience and I owe a lot of inspiration to the kind counselling of professor BOTTAZZINI. Closer to home, professor JESPER LÜTZEN has critically and constructively sharpened my research through a number of discussions. In particular, LÜTZEN'S insightful and inspiring examination of my progress report (1999) helped me improve its contents and made me aware of interesting parallels also in need of consideration.

In Norway, historians of mathematics have taken a kind interest in my work. In particular, I wish to thank professor REINHARD SIEGMUND-SCHULTZE for his comments on a paper of mine;[5] the present dissertation has also benefitted from these comments. The unsurpassable biography of ABEL had been written by ARILD STUBHAUG less than two years before I embarked on my project.[6] I deeply admire STUBHAUG'S cultural biography and enjoyed meeting with him to discuss our common obsession. However, the greatest of my debts to him was that his work made it easier for me to focus on *my* project: the (almost) exclusive study of ABEL'S *mathematics*.

For many years, I have enjoyed being part of the History of Science Department. I have had the chance to discuss my project, mathematics, science, and life in general with some very inspiring and kind people. I wish to thank the entire staff and various generations of students of the Department for their openness and support. I particular, I wish to thank ANITA KILDEBÆK NIELSEN, LOUIS KLOSTERGAARD, TERESE M. O. NIELSEN, and BJARNE AAGAARD for interesting discussions from which this dissertation has benefitted, directly or indirectly. I am also indebted to THOMAS BRITZ for comments and corrections on part II and to my mother, KIRSTEN STENTOFT, for proof reading and help in translating quotations into English.

My thanks also go to SIGBJØRN GRINDHEIM at the manuscript collection of the university library in Oslo for providing me with copies of ABEL'S manuscripts, and to HANS ERIK JENSEN, Statsbiblioteket Aarhus University for tracing the Vienna review of ABEL'S impossibility proof.[7] I also wish to thank KLAUS FROVIN JØRGENSEN for kindly taking the time to provide me with a list of the ABEL-manuscripts held in

5 (Sørensen, 2002).
6 (Stubhaug, 1996), translated into English (Stubhaug, 2000).
7 Used in section 6.7.

the Mittag-Leffler archives in Djursholm, Sweden,[8] to UDAI VENEDEM for providing me with a list of items reviewed in BARON DE FERRUSAC'S *Bulletin*, and to OTTO B. BEKKEN for his interest in my work and for providing me references to the announcements by BRIOT and BOUQUET.[9]

It goes almost without saying that despite the generous help which I have received, any remaining mistakes, misprints, or misunderstandings are solely my responsibility.

The material presented in part II constitutes the reworking of a 140-page chapter appended to my progress report (Sørensen, 1999). Aspects of the research leading up to the dissertation have been presented in various colloquia, courses and meetings.

8 (I. Grattan-Guinness, 1971, 372–373).

9 Touched upon in chapter 14.

Part I

Introduction

Chapter 1

Introduction

In the aftermath of the French Revolution of 1789, the political and scientific scenes in Paris and throughout Europe underwent radical changes. Social and educational reforms introduced the first massive instruction in mathematics at the newly established *École Polytechnique* in Paris; and mathematics, itself, changed and developed into a form recognizable to modern mathematicians. In the first decades of the nineteenth century, the neo-humanist movement greatly influenced Prussian academia and as an effect, mathematics was promoted into a very prominent position in the curriculum of secondary schools. At the university level, mathematics gained a certain autonomy and started to evolve along a distinctly theoretical line with less focus on applications and mathematical physics.

The present work centers on one of the main innovative figures in mathematics in the 1820s, the Norwegian NIELS HENRIK ABEL (1802–1829), and describes his contribution to and influence on the fermentation of the mathematical discipline in the early nineteenth century. Born at the periphery of the mathematical world and with a life-span of less than 27 years, ABEL nevertheless contributed importantly to the disciplines which he studied. The overall outline of this presentation is recapitulated in the following three sections which introduce ABEL'S professional background and training, the mathematics of his works, and the treated themes of development in mathematics in the first half of the nineteenth century. Throughout, ABEL'S mathematics is seen in its mathematical context, and the influences of mathematicians such as A.-L. CAUCHY (1789–1857), C. F. GAUSS (1777–1855), J. L. LAGRANGE (1736–1813), and A.-M. LEGENDRE (1752–1833) is traced and described. This approach provides a background for discussing aspects of continuity and transformation in mathematics as can be envisioned from ABEL'S works.

1.1 The historical and geographical setting of ABEL's life

ABEL lived in a politically turbulent time during which his birthplace, Finnøy, belonged to three different monarchies. When ABEL was born in 1802, Finnøy belonged to the Danish-Norwegian twin monarchy but in the wake of the Napoleonic Wars, the province of Norway was ceded to Sweden after a short spell of independence. Education in the twin monarchy was centered in Copenhagen, and only in 1813 was the university in Christiania (now Oslo) opened. The scientific climate was beginning to ripe, but mathematics was not studied at a high level.

As was common practice for the sons of a minister, ABEL attended cathedral school in Christiania and soon got the young B. M. HOLMBOE (1795–1850) as a mathematics teacher. HOLMBOE was the first to notice ABEL'S affinity for and skills in mathematics and they began to study the works of the masters in special private lessons. In 1821, after graduating from the cathedral school, ABEL enrolled at the university but continued his private studies of the masters of mathematics. In 1824, he applied for a travel grant to go to the Continent and he embarked on his European tour in 1825. It brought him to Berlin and Paris where he had the opportunities to meet some of the most prominent mathematicians of the time and frequent the well equipped continental libraries. More importantly, ABEL came into contact with A. L. CRELLE (1780–1855) in Berlin. CRELLE became ABEL'S friend and published most of ABEL'S works in the *Journal für die reine und angewandte Mathematik*, which he founded in 1826. When ABEL returned to Norway in 1827 he found himself without a permanent job and with no family fortune to cover his expenses, he took up tutoring in mathematics. He had suffered from a lung infection during his tour, and in 1829 he succumbed to tuberculosis.

ABEL'S geographical background thus dictated his approach to mathematics; it forced him to study the masters and advance in isolation to do original work. In his short life span he carefully studied works of the previous generation and went beyond those. During the months abroad, he came into contact with the newest trends in mathematics, and was immediately engaged in new research. Almost all his publications were written during or after the tour. The presentation of ABEL'S historical and biographical background serves to provide a framework for tracing ideas, influences, and connections in his work.

1.2 The mathematical topics involved

ABEL'S mathematical production span a wide range of topics and theories which were important in the early nineteenth century. His primary contributions are universally considered to be in the theory of algebraic solubility of equations, in the rigorization of the theory of series, and in the study of elliptic functions and higher transcendentals. However, some of ABEL'S other works (published or unpublished) also have their place in the contexts of other disciplines, e.g. in the solution of particular types of differential equations, in the prehistory of fractional calculus, in the theory of integral equations, or in the study of generating functions. However, to keep the focus of the present dissertation, these "minor" topics have not been included and emphasis is put on equations, series, and elliptic and higher transcendentals.

Theory of equations. The essentials of mathematics in the eighteenth century come down to the work of a single brilliant mind, L. EULER (1707–1783). Through a lifelong devotion to mathematics which spanned most of the century preceding the French Revolution, EULER reformulated the core of mathematics in profound ways. Inspired by his attempts to demonstrate that any polynomial of degree n had n roots (the so-called Fundamental Theorem of Algebra), EULER introduced another important mathematical question: Can any root of a polynomial be expressed in the coefficients by radicals, i.e. by using only basic arithmetic and the extraction of roots? This question concerned the algebraic solubility of equations and to EULER it was almost self-evident. However, mathematicians strove to supply even the evident with proof, and LAGRANGE developed an elaborated theory of equations based on permutations to answer the question. Though a believer in generality in mathematics, LAGRANGE came to recognize that the effort required to solve just the general fifth degree equation might exceed the humanly possible. In LAGRANGE'S native country, Italy, an even more radical perception of the problem had emerged; around the turn of the century, P. RUFFINI (1765–1822) had made public his conviction that the general quintic equation could *not* be solved by radicals and provided his claim with lengthy proofs.

ABEL'S first and lasting romance with mathematics was with this topic, the theory of equations; his first independent steps out of the shadows of the masters were unsuccessful ones when in 1821 he believed to have obtained a general solution formula for the quintic equation. Provoked by a request to elaborate his argument, he realized that it was in err, and by 1824 he gave a proof that no such solution formula could exist. The proof, which was based on a detailed theory of permutations and a classification of possible solutions, reached world (i.e. European) publicity in 1826 when it appeared in the first volume of CRELLE'S *Journal für die reine und angewandte Mathematik*. But as so often happens, solving one question only leads to posing another. Realizing that the general fifth degree equation could not be solved by radicals, ABEL set out on a mission to investigate which equations could and which equations could not be solved algebraically. Despite his efforts — which were soon distracted to another subject — ABEL had to leave it to the younger French mathematician E. GALOIS (1811–1832) to describe the criteria for algebraic solubility.

Elliptic functions. Since the emergence of the calculus toward the end of the seventeenth century, the mathematical discipline of analysis had been able to treat an increasing number of curves. In his textbook *Introductio in analysin infinitorum* of 1748, EULER elevated the concept of function to the central object of analysis. Concrete functions were studied through their power series expansions and the brilliant calculator EULER obtained series expansions for all known functions including the trigonometric and exponential ones. However, EULER did not stop there but ventured into the territory of unknown functions of which he tried to get hold. One important type of function which analysis had struggled to treat on a par with the rest was the so-called elliptic integrals which can measure the length of an arc of an ellipse.

Mathematicians such as EULER and LEGENDRE felt and spoke of an unsatisfactory restriction of analysis because it was only able to treat a limited set of elementary transcendental functions. Admitting new functions into analysis meant obtaining the kind of knowledge about these functions that would allow them to be given as *answers*. If a function today is nothing more than a mapping of one set into another, the knowledge of a function then included tabulation of values, series expansions and other representations, differential and integral relations, functional relations, and much more.

When ABEL made elliptic integrals his main research topic, much knowledge concerning these objects had already been established. An algebraic approach which had profound influence on ABEL was GAUSS' study of the division problem for the circle (construction of regular n-gons) in the *Disquisitiones arithmeticae*.[1] GAUSS had hinted that his approach could be applied to the lemniscate integral, a particularly simple case of elliptic integrals, and ABEL took it upon himself to provide the claim with a proof. By a new idea, soon to be praised as one of the greatest in analysis, ABEL inverted the study of elliptic integrals into the study of elliptic functions: Instead of considering the value of an integral to be a function of its upper limit, he considered the upper limit to be a function of the value of the integral (compare arcsin and sin). Through formal substitutions and certain addition formulae, ABEL obtained elliptic functions of a complex variable. By this inversion of focus, ABEL managed to place the entire theory of elliptic integrals on a new and much more fertile footing. Fueled by a fierce competition between ABEL and the German mathematician C. G. J. JACOBI (1804–1851), the new theory gained almost immediate momentum and became one of the central pillars of and main motivations for nineteenth century advances in mathematics.

Although ABEL had presented the crucial idea of inverting elliptic integrals into elliptic functions, his impact on the further development of the theory stemmed as much from a vast generalization of the addition formulae presented in a paper which he handed in to the Parisian *Académie des Sciences* in 1826 (not published until 1841). In this paper, ABEL treated an even broader class of integrals generalizing the elliptic

1 (C. F. Gauss, 1801).

ones and — again using primarily algebraic methods — proved more general versions of the addition theorems. The quest of later mathematicians to reapply ABEL'S daring inversion of elliptic integrals to this broader class of integrals led to much of the important development in complex analysis and topology in the nineteenth century.

Rigor. Although the theory of equations was closest to ABEL'S heart, and the theory of elliptic functions brought him fame in the nineteenth century, his mathematical legacy remembered in the twentieth century is just as much about his intense reception of CAUCHY'S new rigor. Picking up from LAGRANGE'S theory of functions, CAUCHY had placed concepts such as continuity and convergence in the foreground and founded these concepts on a new interpretation of *limits*. Equally importantly, CAUCHY had shown a way of working with these concepts to deduce properties of *classes* of objects (e.g.. continuous functions or convergent series) rather than explicit, often lengthy, studies of specific objects.

In a memorable and often quoted letter dated 1826 (first published 1839), ABEL expressed his conversion to *Cauchy-ism* and gave the new rigor its dogmatic manifesto. Apparently more radical than CAUCHY himself, ABEL helped determine the formulation of the new rigor through his interpretative readings of CAUCHY. In the process of re-founding analysis on rigorous grounds, central concepts were specified and *changed* (stretched) to an extent where they included elements whose behavior was deemed abnormal. The encounter and resolution of these abnormalities, *exceptions* as they were often called, was an integrated part of the rigorization process; such exceptions — which a modern reader would consider *counter examples* — shed interesting light on the role and use of concepts in mathematics in the early nineteenth century.

1.3 Themes from early nineteenth-century mathematics

The early nineteenth century marks a period of transition and fermentation in mathematics which involves most layers of the discipline, external as well as internal. With the boundaries fixed, say, between 1790 and 1840, a definite change in the way mathematics was performed and presented is evident; research mathematicians began working in institutions set up for instruction in mathematics and started presenting their results in professional periodicals with substantial circulation. However, the change even effected the internal core of the discipline: how mathematics was done, what mathematics was, and which mathematical questions were interesting. Gradually, *concepts* and relations between concepts took an increasingly central position in mathematics research; although the concern for concrete objects never ceased completely.

Concept based mathematics. Concepts such as *function*, *continuity* of functions, *irreducibility* of equations, and *convergence* of series attained central importance in mathematical research in the transitional period. CAUCHY'S contribution to the rigorization of the calculus laid as much in *applying* technical definitions of concepts to prove theorems as with providing the definitions, themselves. Generalization in the 1820s turned the attention from specific objects to *classes* of objects, which were then investigated. This shift of attention toward collections of individual objects had a very direct influence on the style of presenting mathematical research. In the 'old' tradition, mathematical papers could easily be concerned with explicit derivations (calculations) pertaining to single mathematical objects. Although this presentational style far from ceased to fill periodicals, a less explicit style gained impetus in the first half of the nineteenth century. By deriving properties of classes instead of individual objects, the arguments became more abstract and often more comprehensible by lowering the load of calculations and simplifying the mathematical notation. The transition is evident in ABEL'S works which show deep traces of the calculation based approach to doing mathematics as well as being markedly conceptual at times; his 1826 paper on the binomial theorem is a fascinating mixture of both approaches.

Abstract definitions and coming to know mathematical objects. In many developing fields of mathematics in the early nineteenth century, new concepts were specified by the use of abstract definitions based on previous proofs, intentions, and intuitions. In the approach which I term *concept based mathematics*, the concepts were *defined* in the modern sense that there is nothing more to a concept than its definition. However, when abstract definitions determine the extent of a concept, representations and demarcation criteria are required in order to get hold of properties of objects, and this quest for understanding, *coming to know*, the objects is an important aspect of early nineteenth century mathematics. In many ways, analogies may be drawn to the effort of coming to know geometrical objects, e.g. *curves*, in the seventeenth century. To mathematicians of the seventeenth century, a curve meant more than any single given piece of information. In particular, an equation (or a method of constructing any number of points on the curve) was not considered sufficient to accept the curve as *known*. Similarly, in the nineteenth century, knowledge of an elliptic function meant more than just a formal definition and included various representations, basic properties, and even tabulation of values.

The question of coming to know a mathematical object relates to the problem of accepting the object as solutions to problems. The reduction of properties of curves to questions pertaining those *basic* curves which were considered well known was important in the seventeenth century. However, certain properties were not expressible in basic curves (or functions) but required higher transcendentals such as elliptic integrals. Thus, much of EULER'S research on elliptic integrals in the eighteenth century can be seen as an effort to make these integrals *basic* in the sense of acceptable solutions to problems. This research program was continued and reformulated in the nineteenth century during which the foundations, definitions, and framework of elliptic functions underwent repeated revolutions.

Critical revision. The critical mode of thought, rooted in the Enlightenment, had a profound impact on mathematics. Together with the demand for wider instruction in mathematics, the critical attitude brought about a deeply sceptical reading of the masters which focused on the foundations. In geometry, some mathematicians began to believe in the possibility of a non-Euclidean version, and in analysis, the long-standing problem of the foundation of the calculus was made an important *mathematical* research topic.[2]

CAUCHY'S definition of the central concept of limits was itself a novelty, but of equal importance was the outlook for a concept based version of the calculus. CAUCHY'S new foundation for the calculus was *arithmetical* and introduced the arithmetical concept of equality. In the wake of the change of foundations of the calculus, certain objects and methods could no longer be allowed into analysis, and it became a quest to prop up parts of the mathematical complex recently made insecure. In particular, CAUCHY had to abolish from analysis all divergent series which had formerly been interpreted by a *formal* concept of equality. However, divergent series had provided new insights to mathematicians which they were reluctant to abandon and it became a legitimate, albeit difficult, mathematical problem to investigate how problematic or outright unjust procedures had led to correct results. Resolution of this problem laid in further specification of concepts involved and a heightened awareness of the procedures employed in arguments. For instance, by the mid-nineteenth century, the unreflective interchange of orders of limit processes had been identified as problematic and concepts such as absolute and uniform convergence had been introduced and put to use in theorems and proofs.

1.4 Reflections on methodology

The present study aims at illustrating important conceptual developments in mathematics which took place in the first decades of the nineteenth century. In order to introduce a focus on the many-faceted aspects of these developments, the mathematical production of ABEL has been taken as a starting point. In the present section, some of the methodological choices and considerations involved in the project are briefly discussed. It is not my ambition to present a coherent theoretical framework for historical enquiries but rather to make some considerations explicit and open for discussion.

2 See e.g. (Grabiner, 1981b).

1.4.1 Diachronic descriptions

It is within the framework of mathematics developed by EULER and cultivated in the eighteenth century that ABEL'S production is rightfully seen. Although his mathematics has been perceived as a very important step *forward* in a linear development, ABEL'S mathematical ideas were rooted in the previous attitude and style; and many of the famed new trends are only barely recognizable in his work. Therefore, anachronisms and teleological conceptions have to be dismissed in favor of a diachronic, more hermeneutic approach. In the process of tracing and describing this historical evolution of mathematical content, it is of the utmost importance that ABEL'S works be studied within their contemporary framework — their mathematical context.

Each of the main theories outlined above with which ABEL was involved is reviewed with the purpose of illustrating how they were effected by currents of mathematical change in the early nineteenth century. To do so, ABEL'S mathematics is presented and discussed based on a contextualized reading which emphasizes ABEL'S own methods and tools. To place these in their proper historical contexts, the theories and results will be traced back into the eighteenth century in search of the inspirations and their progressions in the nineteenth century will be followed. In the theory of equations, for instance, the works of EULER on the fundamental theorem of algebra will be briefly introduced; more emphasis will be given to the works of LAGRANGE and GAUSS which served as the direct inspirations for ABEL and most of his contemporaries in the field. Then, in the nineteenth century, special emphasis is given to those works which share their inspirations with the works of ABEL, in this case the works of RUFFINI and GALOIS. For each disciplinary theme, a sketch of the further development after the initial decades of the nineteenth century is then given in order to illustrate how the ideas and currents which were barely discernible in the first decades came to play very important roles in the conceptions of mathematicians. The descriptions of the ensuing histories also serve to illustrate how ABEL'S works were valued and received by the following generations.

Besides, it has been a secondary aim of the present study to make ABEL'S authentic mathematical thought available to the mathematically trained reader who is not familiar with early nineteenth century technicalities. In order to understand and evaluate ABEL'S role in the formation of modern mathematics, this presentation will always favor the original source over any modern approach.

The setting of ABEL'S mathematics within the general view of mathematics expressed by EULER will also be manifest in another way as a chronological mark. It has been necessary to trace many of the ideas and methods of ABEL'S mathematics back to the middle of the eighteenth century, but they might be even older. However, as this is a work on ABEL'S mathematics, such hypothesis will rarely be made explicit and EULER will be attributed things, which he did do — perhaps not as the first.

1.4.2 Philosophical theories and their applicability

Philosophical theories enter the framework of the present study only rather implicitly. Written with the utopian goal of being an "account of things which happened", the outlines of a certain perception of concepts such as *change* and *transformation* is nevertheless discernible. The internal (and external) structure of scientific change has been subjected to many philosophical investigations over the past decades. In the present context, however, two of the founding theories have served as inspirations; those of *Kuhnian paradigms and revolutions* developed for the sciences in general and those of *Lakatosian dialectics* which were developed explicitly for mathematics and illustrated by examples from the nineteenth century. These two philosophical positions are so well established within the history of science community that only a brief presentation is given with an emphasis on their applicability to the present study.

T. S. KUHN (1922–1996), paradigms, crises, and revolutions. In his very influential monograph *The structure of scientific revolutions* of 1962,[3] KUHN advocated an explanation of the dynamics of scientific change. The total mental and physical entourage of a science at a given time was encompassed in the notion of *paradigms*. Paradigms are abruptly replaced through *revolutions* which are the responses to *crises* brought about by a compilation of *anomalies* inexplicable within the ruling paradigm. Once a revolution has taken place, a new paradigm is introduced and communication between two distinct paradigms (e.g. over which paradigm to prefer) becomes irrational or extra-scientific. KUHN'S original model — although deeper than the present description — was a simplistic one which was amended and extended by numerous studies following its publication. Here, on the other hand, it will not be taken to serve as a complete model but rather as inspiration and terminological framework used to capture important aspects.

As a model of mathematical change in the early nineteenth century, the Kuhnian system offers some obvious advantages; the important position given to anomalies in bringing about crises and revolutions was further extended in the works of I. LAKATOS (1922–1974) (see below). However, as has been emphasized by many philosophers and historians of mathematics, no overthrow of knowledge seems to occur in mathematics; thus no truly Kuhnian revolutions seem possible in the mathematical realm.[4] The remaining notions of the Kuhnian conceptual framework such as paradigms, anomalies and crises are, however, applicable and useful in the description and analysis of mathematical change, even if KUHN'S dynamics are not always appropriate. In the first sections of part II, for instance, it is illustrated how a mathematical theory came into being by a change of focus (a paradigmatic change) which shifted emphasis to questions of solubility. The theme will recur even more distinctly in part III which

3 (Kuhn, 1962).
4 See (Gillies, 1992).

documents ABEL'S role and position in the most Kuhnian of changes in mathematics during the early nineteenth century: the complete reformulation of analysis according to CAUCHY'S new program of arithmetical rigor.

LAKATOS and the extension of concepts. Further philosophical inspiration is taken from LAKATOS' *Proofs and Refutations* published as a series of articles in 1963–64 and as a book in 1976.[5] LAKATOS described the dynamics of mathematical change in terms of a dialectic between *proofs* and *counter examples* by means of *proof revisions*. In the main part of the *Proofs and Refutations*, LAKATOS explained his theory by exhibiting a rationally reconstructed development of the Eulerian polyhedral formula; in appendices, he further illustrated the theory by exhibiting applications to other concepts including the development of the concept of uniform convergence (see part III).

LAKATOS saw the process of proof as central to the mathematical endeavor. Incorporating into mathematics a version of K. R. POPPER'S (1902–1994) falsificationism, LAKATOS described mathematical change as a continued revision of proofs to reflect objections raised by counter examples. LAKATOS classified counter examples as either local (refuting only part of a proof, but not the overall statement) or global (refuting the overall statement, but not necessarily any identifiable part of the proof).

Counter examples could, in LAKATOS' description, be constructed from existing proofs by a process of *concept stretching* by which a partially defined concept was redefined in an extended version which — although possibly more precise — encompassed instances (objects) not covered by the previous — often more intuitive — version of the concept.

In response to such falsifications (refutations) by counter examples, LAKATOS suggested various strategies for refining the proofs. A naive approach would try to explain the counter examples away, either by arguing that they were too pathological to be taken seriously or (more interestingly) by restricting the theorem to a narrower domain for which it was believed to surely valid; the latter approach was named *exception barring* by LAKATOS. A more fruitful response to the refutation by counter examples — and the one which LAKATOS' philosophy dogmatized — was the method of *proof analysis* which took the counter examples more seriously. By carefully analyzing the counter example and the proof which it refuted, proof analysis produced a new proof in which a refuted lemma was replaced by an unrefuted one which might cause an alteration of the overall statement. Thus, LAKATOS suggested, theorems were produced which had very explicit assumptions and were very hard to refute.

Just as was the case with the Kuhnian model, LAKATOS' model — in all its generality — is often found inadequate to describe the actual historical development of mathematics. On the other hand, LAKATOS' model offers some further concepts which often ease the description and analysis of past events. Most importantly, LAKATOS'

5 (Lakatos, 1976). A good description of LAKATOS' life and philosophy can be found in (Larvor, 1998).

description of counter examples and the role that they play in mathematical change elaborates the role played by anomalies in the Kuhnian model and suggests a more refined view on the status of a mathematical theory in crisis.

The Lakatosian theory of mathematical evolution is present as background throughout; it will surface sporadically in parts II–IV and become important again in the final, more analytical part V.

M. EPPLE'S epistemic configurations. Quite recently, EPPLE has suggested the notion of *epistemic configurations* in order to be able to discuss change in mathematics in another context.[6] In EPPLE'S analysis, epistemic configurations consist of *epistemic objects* and *epistemic techniques* and are manipulated in mathematical *workshops*. The concept of epistemic objects encompasses the immaterial objects with which mathematics deals. These are manipulated and investigated by a number of methods of producing (or obtaining) mathematical knowledge; these methods are the epistemic techniques. The precise applicability and range of EPPLE'S concepts and their usefulness in historical analysis is not the primary objective here. Instead, as with the inspirations of KUHN and LAKATOS, I have taken the liberty of using EPPLE'S terms to ease the analysis and discussion of what I believe to be a fundamental change in mathematics in the early nineteenth century: the change from *formula based* to *concept based* mathematics which is addressed in chapter 21.

1.4.3 Existing literature

Being one of the important mathematicians of the nineteenth century, ABEL'S person and his mathematics have been subjected to study for a multitude of different reasons. A few general trends of the literature on ABEL can profitably be identified at this point.[7] At the relevant places in the subsequent parts, references are given to the secondary literature which is listed in the bibliography.

ABEL in the history of mathematics literature. In the professional literature in the history of mathematics, ABEL is often mentioned in order to illustrate one or more of the following aspects:

1. ABEL'S life story is invoked to illustrate the conditions of young mathematicians two centuries ago. This aspect is closely related to the biographies treated below.
2. ABEL'S letters from Paris are used to illuminate how the confrontation with CAUCHY'S new rigor brought about a radical change. For instance, U. BOTTAZZINI (⋆1947) quotes *in extenso* from these letters in his comprehensive account of the evolution of analysis in the nineteenth century.[8]

6 (Epple, 2000).
7 For a thematic listing of the ABEL literature, see also (Sørensen, 2002).
8 (Bottazzini, 1986).

3. The modern highlights of ABEL'S production, e.g. the binomial theorem or the insolubility of the quintic, are described to shed some light on the evolution of the theories and the involved concepts.

4. ABEL'S mathematics is described *per se* in order to give a presentation of his production. Very good examples include articles by P. L. M. SYLOW (1832–1918) in the ABEL centennial memorial volume and the second edition of the collected works.[9]

The present study incorporates all these approaches to give a comprehensive overview of ABEL'S mathematical production as well as positioning it within a broader frame describing themes of mathematical change in the period.

Two other types of studies treating the life and works of ABEL delineate themselves: biographies and interpretations.

Biographies — scientific or not. As should become clear in the next chapter, ABEL'S biography includes all the components of a truly romantic biography of a misunderstood genius who rose from the dust to become a nobility of mathematics. Such biographies have been written;[10] but more interestingly, biographies have also been written which serve a purpose of their own. The first biographies appeared as obituaries written by ABEL'S friends soon after his death. Of primary importance in describing ABEL'S mathematics are the obituaries written by HOLMBOE and CRELLE which include first hand descriptions of ABEL'S mathematical work.[11] Although a larger number of biographies could be listed, the most widely circulated and very well researched twentieth century biography was written by Ø. ORE (1899–1968);[12] it has been used mainly to help set the chronology straight. The human and cultural aspects of ABEL'S life has most recently been very carefully researched and described by A. STUBHAUG (⋆1948) who meticulously sets the cultural scene of early nineteenth century Norway and Europe.[13] STUBHAUG'S biography has relieved me of any obligation to produce biographical news concerning ABEL'S person; the biography which is provided in chapter 2 serves merely to set the framework of the subsequent chapters. It is my hope that the present study of ABEL'S mathematics will complement STUBHAUG'S book on his life and environment to produce a picture of ABEL'S person *and* his mathematics.

Renderings of ABEL'S work in modern theories. By the very nature of mathematics, mathematical knowledge seems to accumulate and only change its presentational

9 (N. H. Abel, 1881; L. Sylow, 1902).
10 E.g. (Bell, 1953).
11 (A. L. Crelle, 1829b; Holmboe, 1829).
12 (Ore, 1954; Ore, 1957).
13 (Stubhaug, 1996; Stubhaug, 2000).

form or its internal relations within mathematical structures. For this reason, mathematicians often hope to find inspiration in the works of their predecessors. Frequently, this leads to the publication of modernized versions of historical proofs. By itself, this practice is very good as long as the author and the community recognize that it is precisely a revisited proof or theorem and precisely *not* a diachronic description of that proof or theorem within its contemporary structure.

Such revisits to ABEL'S production are most frequently made to his theory of algebraic solubility of equations, more precisely to his proof of the insolubility of the quintic equation.[14] ABEL'S other main contributions attract less attention; the binomial theorem because it has become an integral part of basic mathematical knowledge, and the *Abelian Theorem* (see part IV) because its original form has been surpassed and the result has been recast in a different theory.

As should now be clear, the methodology of the present approach can be summarized thus: A diachronic reading of the original sources of ABEL'S mathematics with the purpose of analyzing themes of mathematical change in the early nineteenth century, in particular the rise of concept based mathematics.

14 See e.g. (R. Ayoub, 1982; Radloff, 1998).

Chapter 2

Biography of Niels Henrik Abel

The life of Niels Henrik Abel (1802–1829) was not always a happy one. Born in a time of national upheaval and into a family with few provisions against the hard times, his actions were always restricted by pecuniary concerns. A melancholic, he preferred to be surrounded by people, but due to his shy and modest nature he felt secure only with a score of friends including his elder brother, his sister, his mathematics teacher B. M. Holmboe (1795–1850), Mrs. C. A. B. Hansteen (1787–1840) — Abel's benefactor and the wife of his university professor, and his mentor in Berlin A. L. Crelle (1780–1855). Abel fell in love with C. Kemp (1804–1862) in 1823 and they were engaged the following year. Unfortunately, Abel's position never became secure enough for them to marry. The last years of Abel's life were spent in uncertainty with hopes of a more stable future either at home or abroad. When he died, Abel's mathematical star was still rising, and years would pass before the world knew exactly how bright it had been.

The short and yet very creative life of Abel has caught the interest of many biographers. Confined within the romantic period and exhibiting distinctly romantic features itself, the biographers have often focused on Abel's poverty and contemporary lack of acknowledgment;[1] both features frequently found in the romanticization of mathematicians and scientists. Another genre of biography has constructed and researched a controversy with C. G. J. Jacobi (1804–1851) over the priority of the inversion of elliptic integrals.[2] The most recent and excellent biography written by A. Stubhaug (⋆1948) has taken a different angle, describing and bringing to life the cultural and political context in which Abel lived and which is so important for Norwegian self-image.[3]

As Stubhaug's book is such a convincing description of the cultural and biographical background of Abel's life, the present biography serves only to provide a self-contained presentation of the temporal framework in which Abel's mathemati-

1 See e.g. (Bell, 1953) or, more soberly, (Ore, 1950; Ore, 1954; Ore, 1957).

2 E.g. (Bjerknes, 1880; Bjerknes, 1885; Bjerknes, 1930). This debate was also the subject of (Koenigsberger, 1879).

3 (Stubhaug, 1996), translated into English in (Stubhaug, 2000).

Figure 2.1: NIELS HENRIK ABEL (1802–1829)

cal production was localized. All facts presented here have been taken from existing literature, primarily HOLMBOE'S obituary, C. A. BJERKNES' (1825–1903) biographies, and STUBHAUG'S contextual biographical study.[4] References to these works will not always be made explicit. I will deliberately desist from giving detailed analyses or speculations concerning the personality and private life of ABEL except where supported by the biographies and ABEL'S correspondence.

2.1 Childhood and education

ABEL was born as the second son into an incumbent's family on 5 August 1802. ABEL'S father, S. G. ABEL (1772–1820), himself the son of a minister, had been educated in Copenhagen and received a call to the rural parish of Finnøy in 1800 (see figure 2.2).

Also in 1800, SØREN GEORG married A. M. SIMONSEN (1781–1846), the daughter of a wealthy merchant, and together they had six children; five boys and a girl. In 1804, SØREN GEORG took over the more lucrative parish of Gjerstad from his father who had died the year before. Nevertheless, due to the family increase, the costs of educating the children, a nationalist sentiment to contribute to the founding of the university, and the troubled times for the nation, the ABEL-family remained without

4 (Holmboe, 1829), (Bjerknes, 1880; Bjerknes, 1885; Bjerknes, 1930), (Stubhaug, 1996).

Figure 2.2: The southern part of Norway with Christiania and Finnøy marked. See also (Stubhaug, 2000, 136).

fortune — a situation which only deteriorated after Søren Georg died in 1820 leaving his widow a thirty year commitment of financial donations to the university in Christiania.

During his years in Copenhagen, Søren Georg had been influenced by the reformist education and he took care of the primary education of his sons himself. Accordingly, the focus was on the catechism and skills in reading, writing, and basic arithmetic. In 1815, when Niels Henrik was 13, he and his elder brother H. M. Abel (1800–1842) were sent to the Cathedral School in Christiania. There, they were taught classical and modern languages, as well as arithmetic and geometry. Abel passed his *examen artium* in 1821 with first grades in the mathematical disciplines, second grade in French, and only third grades in German, Latin, and Greek.[5] In the lower classes, Abel demonstrated no particular affinity for the mathematical disciplines but was a fair student in all subjects. However, this situation dramatically changed to the better due to an unfortunate event which took place in 1817–18.

In November of 1817, H. P. Bader (1790–1819), Abel's mathematics teacher, physically molested one of his pupils who later died from this hands-on approach to mathematics education. Bader, who had a record of hot temper and violent teaching methods, was excused from his teaching obligations, and Holmboe stepped in as

5 (N. H. Abel, 1902d, 3)

Figure 2.3: BERNT MICHAEL HOLMBOE (1795–1850)

a replacement in 1818.[6] HOLMBOE, who was ABEL'S senior by only 7 years, had been educated at the cathedral school himself and had attended mathematics courses under S. RASMUSSEN (1768–1850) at the university. HOLMBOE soon noticed a special talent for mathematics in ABEL and they began reading extra-curricular mathematics together.

2.2 "Study the masters"

Soon, HOLMBOE began to realize what mathematical talent he had at hand and by the autumn of 1818, HOLMBOE urged ABEL to study the important works in mathematics on his own. In one of his notebooks, ABEL pursued calculations inspired by P.-S., MARQUIS DE LAPLACE'S (1749–1827) use of generating functions. Between all the calculations, he noted in the margin:

> "If one wants to know what one should do to obtain a result in more conformity with Nature one should consult the works of the famous Laplace where this theory is exposed with the most clarity and to an extent in accordance with the importance of the subject. It is also easy to see that a theory written by M. Laplace must be much superior to any other written by less bright mathematicians. By the way it seems to me that if one wants to progress in mathematics one should study the masters and not the pupils."[7]

6 For details, see (Stubhaug, 1996, 172–174).

HOLMBOE'S list of the masters whom they studied together sheds interesting light on the mathematical literature of the early nineteenth century as seen from the periphery.[8] In the eyes of the two Norwegians, the two most influential writers were L. EULER (1707–1783) and J. L. LAGRANGE (1736–1813); but the list also included S. F. LACROIX (1765–1843), L. B. FRANCOEUR (1773–1849), S.-D. POISSON (1781–1840), C. F. GAUSS (1777–1855), and J. G. GARNIER (1766–1840). In the following paragraphs, the influences from these authors are outlined.

EULER. EULER and HOLMBOE studied algebra and calculus from EULER'S works; ABEL'S first independent adventures into creative mathematics were greatly inspired by the great Swiss mathematician. Although the sources are not very explicit, it is beyond doubts that ABEL studied EULER'S *Introductio in analysin infinitorum* [Introduction to the infinite analysis] of 1748.[9] To what extent ABEL also knew of EULER'S other publications including his papers in the transactions of the St. Petersburg Academy is left for speculation; we shall return to the question when we see examples of EULER'S — possibly indirect — influence on ABEL in parts II and IV.

The big four. LAGRANGE, LACROIX, POISSON, and GAUSS all belong to the heavyweight division of mathematics in the late eighteenth century with massive and important works on the calculus and algebra. Although a writer of very influential textbooks on the calculus,[10] LAGRANGE mainly inspired ABEL through his work on the theory of equations which redefined the viewpoint from which this theory was to be attacked.[11] LACROIX' effort laid more in organization and presentation than in creative research; his three volume textbook on the calculus, *Traité de calcul différentiel et intégral* [Treatise on differential and integral calculus], ran multiple editions beginning in 1797–1800.[12] In the *Traité*, LACROIX presented an survey of the calculus based on the research of his contemporaries and picking up a variety of approaches and foundations from different authors.

The lesser souls. The two authors in HOLMBOE'S list who today are lesser known, FRANCOEUR and GARNIER, both wrote textbooks on mathematics which found wide circulation toward the end of the eighteenth century. ABEL almost certainly studied

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7 "*Si l'on veut savoir comment on doit faire pour parvenir à un resultat plus conforme à la nature il faut consulter l'ouvrage du celebre Laplace où cette theorie est exposée avec la plus grande clarté et dans une extension convenable à l'importance de la matière. Il est en outre aisé de voir que une theorie ecrite par M. Laplace doit être bien superieure à toute autre donnée des geometres d'une claire inferieure. Au reste il me parait que si l'on veut faire des progres dans les mathématiques il faut étudier les maitres et non pas les écoliers.*" (Abel, MS:351:A, 79, marginal note).

8 (Holmboe, 1829, 335).

9 (L. Euler, 1748).

10 E.g. (Lagrange, 1813).

11 (Lagrange, 1770–1771).

12 (Lacroix, 1797; Lacroix, 1798; Lacroix, 1800).

FRANCOEUR'S *Cours complet de mathematiques pures* [Complete course on pure mathematics],[13] which in two volumes dedicated to the emperor of Russia, introduced arithmetic, geometry, algebra, and differential and integral calculus. The textbook had been translated into German by one of ABEL'S mentors, C. F. DEGEN (1766–1825),[14] but since ABEL only came to master German during his tour 1825–27, he probably studied the French original.

Besides writing textbooks on algebraic analysis (LAGRANGE'S approach to the calculus), in 1807, GARNIER translated EULER'S *Vollständige Einleitung zur Algebra* [Complete introduction to algebra] of 1770 into French.[15] With his limited knowledge of German, it is doubtful whether ABEL read EULER in the original language, but through the translations by LAGRANGE or GARNIER or even through FRANCOEUR'S complete course on pure mathematics, ABEL became acquainted with the elementary parts of contemporary mathematical knowledge, in particular the solution of cubic and bi-quadratic equations.

2.2.1 An alleged solution formula

While still in grammar school, ABEL approached one of the most prestigious problems of contemporary mathematics: the search for an algebraic formula expressing the solution of the quintic equation. Since the Western Renaissance, similar formulae for equations of the first four degrees had been known. In 1821, ABEL believed to have found a closed algebraic expression solving the next case: the general quintic. He wrote down his result and showed it to his teacher HOLMBOE, who took it to C. HANSTEEN (1784–1873), one of the two professors in science at the Christiania University.[16] HANSTEEN, who together with HOLMBOE were among the few people in Norway competent enough to have a chance of understanding ABEL'S tedious argument, took it to the University's *collegium academicum*. The collegium took note of ABEL'S argument and wanted to make it public to a broader mathematical audience. However, as the young Norwegian state was itself without means of such a publication with a wide circulation, ABEL'S paper was sent to professor DEGEN in Copenhagen with the hope that it be published in the transactions of *Royal Danish Academy of Sciences and Letters*. DEGEN'S assessment proved to have a profound influence on ABEL'S career.

Upon reception of the paper, DEGEN scrutinized ABEL'S solution, and DEGEN'S response to HANSTEEN is the only existing written source of ABEL'S adventure. There, DEGEN requested an elaboration, a numerical example, and a rewriting of the manuscript for the other members of *Videnskabernes Selskab* to be able to read it. To ABEL, the refusal to immediately publish his result must have been disappointing. However,

13 (L.-B. Francœur, 1809).

14 (L.-B. Francœur, 1815). In 1839, it was again translated into German by KÜLP.

15 It has been translated into English as (L. Euler, 1972).

16 Very unfortunately, ABEL'S supposed solution has not survived and only speculative reconstructions can be suggested.

as he sat down to provide the details, he must have realized that DEGEN had spared him a humiliating entry onto the mathematical scene. Before 1824, ABEL realized that no algebraic solution formula could be found for the general quintic, and thus that his solution had been flawed and his search in vain. ABEL never sent an elaboration to DEGEN, never published in the *Transactions of the Royal Danish Academy of Science*, and when ABEL and DEGEN eventually met in person two years after their initial correspondence, ABEL had other things on his mind.

2.2.2 A student at the young university

When ABEL enrolled at the university in 1821, the university was still in its constitutional phase. Founded in 1811 and opened in 1813 as only the third university in the twin monarchy (after Copenhagen and Kiel), the Christiania university initially only offered degrees in theology, law, medicine, and philosophy. The study of science and mathematics was subsumed under the philosophical faculty and no course of studies led to any degree in the sciences. Thus, when ABEL enrolled, his determination to study mathematics defied the existing structure of academic qualification. He must have hoped that his extraordinary talents alone would be enough to secure him a future in academia.

During his years at the university, ABEL attended lectures by the two professors in mathematics and astronomy, RASMUSSEN and HANSTEEN. The mathematical lectures were primarily on elementary mathematics, spherical geometry, and applications to astronomy, and ABEL had soon learned all he could from these courses. As a complement, he continued studying the works of the masters of mathematics. In 1823, ABEL came across the *Disquisitiones arithmeticae* of GAUSS,[17] which provided him with a rich source of inspiration and problems for his own research. Itself an immensely important work in the theory of numbers, the *Disquisitiones arithmeticae* influenced ABEL in two other fields: the theory of equations and the rectification of the lemniscate.

During his years as a student, ABEL held a free room and board at the *Regentsen*, a student residence for the most needy students. Until he was given a stipend from the State in 1824, he was financially supported by some of the University professors, including RASMUSSEN and HANSTEEN.[18]

First publications in *Magazin for Naturvidenskaberne*. In 1823, professor HANSTEEN, together with two fellow professors at the university, tried to amend the lack of Norwegian periodicals in natural science with the creation of the *Magazin for Naturvidenskaberne* [Magazine for the natural sciences]. Its aim was to convey Norwegian research in the sciences to the educated lay audience and provide an emerging group of young scientists with a forum for publication.

17 (C. F. Gauss, 1801).
18 (Stubhaug, 1996, 244–245).

In the first issue of the *Magazin*, the 21 year old ABEL had his first publication. Inspired by EULER'S *Institutiones calculi differentialis* and A.-M. LEGENDRE'S (1752–1833) *Exercises de calcul integral*, ABEL solved two problems in the integral calculus.[19] In the same year, a second publication by ABEL dealing with the theory of elimination was published in the *Magazin*.[20] On this occasion, HANSTEEN found it necessary to add an introduction in which he — summoning G. GALILEI (1564–1642), I. NEWTON (1642–1727), and C. HUYGENS (1629–1695) — argued that mathematics even in its purest form was within the scope of the magazine devoted to the natural sciences. Taking into account the limited circulation of the *Magazin* and HANSTEEN'S efforts to make ABEL'S work acceptable to the audience, it is doubtful how much ABEL gained from these publications. But to a young man — still nothing but a *studiosus* — getting his name on printed paper must have been a great satisfaction.

When ABEL'S publications in the *Magazin* were first noticed, it was for all the wrong reasons. In 1824, ABEL published some computations pertaining to the influence of the Moon on the movement of a pendulum.[21] This problem fitted nicely into a research project concerning the magnetic field of the Earth with which HANSTEEN was immensely involved. Upon HANSTEEN'S request, the paper was sent to H. C. SCHUMACHER (1784–1873) in Altona, who edited the journal *Astronomische Nachrichten* [Astronomical intelligencer], for possible republication therein. However, SCHUMACHER realized that ABEL had made a computational error which had led him to estimate the influence of the Moon to be ten times stronger than was rightfully supported. SCHUMACHER refused to publish the paper, and a correction was subsequently inserted in the *Magazin*.[22]

2.2.3 Visiting Copenhagen

In 1823, ABEL for the first time left Norway to go on his first educational tour. Supported privately by professor RASMUSSEN, ABEL traveled to Copenhagen to visit and discuss with the mathematicians there.

Mathematics in the capital. The mathematical milieu in Copenhagen was not completely different from the one in Christiania.[23] In 1823, the mathematical profession was centered around the university and the academy of science; six years later, *Den Polytekniske Læreanstalt* [The polytechnic college][24] was opened. Despite the university

19 (N. H. Abel, 1823)

20 (ibid.)

21 (N. H. Abel, 1824c).

22 (N. H. Abel, 1824a).

23 The mathematical milieu in Copenhagen in the first half of the nineteenth century has been the subject of a subsequent study by the author in the Danish History of Science project undertaken at the History of Science Department at the University of Aarhus. The interested reader may also wish to consult the dissertation (in Danish) by KURT RAMSKOV (Ramskov, 1995, chapter 1) or STUBHAUG'S book (Stubhaug, 2000, chapter 31) for information.

24 Today *Danmarks Tekniske Universitet* [Technical University of Denmark].

Figure 2.4: CARL FERDINAND DEGEN (1766–1825)

in Copenhagen being older and larger than the one in Christiania, the faculty of philosophy had only two professorships in mathematics (and one in astronomy) which in 1823 were held by DEGEN and E. G. F. THUNE (1785–1829).

Together, DEGEN and THUNE had meant a change of generations in the mathematical milieu at the university when they were appointed 1813 and 1815.[25] DEGEN, being the more creative of the two, had gained international reputation with a publication of tables for the solution of Pellian equations in 1817.[26] To the mathematicians in Christiania — as to the general Norwegian public — Copenhagen was still to some extent considered the intellectual and cultural capital of the country even after the separation in 1814. Thus, by virtue of its former colonial power, the mathematical talents of DEGEN, and the circulation and status of the publications of the Royal Danish Academy of Sciences and Letters, Copenhagen was the obvious choice for a first foreign trip for a young and promising Norwegian mathematician.

The maturing mathematician. ABEL'S first two works in the sciences estimated important enough to receive wider circulation by Norwegian scholars, the alleged solution of the quintic and his computations concerning the magnetic field of the Earth, were both caught in the review system of the time. In the incipient scientific milieu in

25 THUNE was originally appointed as professor of astronomy 1815 before he transferred to the professorship of mathematics 1819.

26 (C. F. Degen, 1817)

Norway, the means of publication were limited and advanced knowledge of the sciences was confined to a few men. Therefore, foreign experts were invited to judge — and possibly publish — ABEL'S first papers. DEGEN made reservations concerning the completeness of ABEL'S research on the solution of the quintic and refused to present ABEL'S paper to the Danish Academy until he had seen the method applied to a numerical example. The numerical example was explicitly requested as a *lapis lydius* — a test of correctness — and it is not improbable that the confrontation with an explicit, difficult problem was what later led ABEL to realize his being in error.

The HANSTEEN-DEGEN period. In his historical introduction to the centennial memorial volume,[27] E. B. HOLST (1849–1915) has emphasized the influence of HANSTEEN and DEGEN on ABEL'S research 1821–24. P. L. M. SYLOW'S (1832–1918) analysis seems applicable on at least two levels: topics and methods. On the topical level, the two protagonists exerted contrary influences. Responding to HANSTEEN'S suggestion, ABEL had briefly worked on a physical problem; either because of this failed encounter or because of a personal inclination, he subsequently focused exclusively on working within pure mathematics. Following an advice of DEGEN, ABEL ventured into the theory of integration in the tradition of EULER and LEGENDRE.[28] Being an important theory of the eighteenth century left open for further developments, DEGEN had, himself, spent some time studying elliptic integrals. But there is no real evidence to suggest that DEGEN could have foreseen what his new disciple would do for the discipline. Although their interests differed, both HANSTEEN and DEGEN were trained in the typical eighteenth century mathematical literature including the men whom ABEL considered his masters at the time. Formal manipulations and physical applicability were considered positive aspects of the approaches of EULER and his mid-eighteenth century contemporaries. The DEGEN-HANSTEEN period marks the end of ABEL'S youthful encounters with the formal approach to analysis and at the same time marks the beginning of a period of intense study of the theory of higher transcendentals which would be ABEL'S masterpiece when judged by his contemporaries.

2.3 The European tour

After ABEL returned to Christiania from his first trip to Copenhagen, he soon realized that there was little more for him to gain while isolated in the limited Norwegian mathematical community. In 1824, ABEL applied with the support of the professors HANSTEEN and RASMUSSEN for a travel grant from the university. ABEL'S primary aim was to visit to the mathematical capital of his time, Paris. There, in Paris, mathematics had been institutionalized and cultivated to the highest level in the wake of

27 (Holst, 1902, 22).

28 In part IV, the influence on ABEL of these mathematicians will be traced, documented, and analyzed.

the French Revolution. Later in the application procedure, Göttingen, the seat of the German champion of mathematics GAUSS was added to the travel plan.

In his first period of creative mathematical production, 1823–24, while inspired by HANSTEEN and DEGEN (see above), ABEL devoted his attention to the theory of integration in the tradition of EULER'S *Institutiones calculi integralis* and LEGENDRE'S *Traité de calcul integral*. By 1824, ABEL had documented his "exceptional abilities in the mathematical sciences"[29] an example of which HANSTEEN presented to the collegium in the form of a manuscript. ABEL had hoped that — besides the travel grant — the collegium would support publication of the result which he believed had international importance and could serve as a door opener on his tour. However, the collegium decided to only support a travel grant for ABEL to go to the Continent. There was only one condition; the grant was only to begin in 1825; until then ABEL had to prepare by studying the "learned languages", which in particular meant French.[30]

2.3.1 Objectives and plans

In the application to the collegium, sources to the contemporary Norwegian ranking of the mathematical centres may be found. The choices were mainly made by ABEL'S benefactors, the mathematics professors HANSTEEN and RASMUSSEN. When they first proposed sending ABEL abroad they suggested that he should go to "the places abroad where the most distinguished mathematicians of our time are located, perhaps primarily to Paris".[31] In ABEL'S official application to the King, he wrote:

> "After I have thus in this country by the use of the available tools sought to approach the erected goal, it would be very beneficial to me to acquaint myself, during a stay abroad at different universities in particular in Paris where so many distinguished mathematicians are located, with the newest creations in the science [mathematics] and enjoy the guidance of those men who in our time have brought it [mathematics] to such a remarkable height."[32]

Only of July 4th 1825 did the idea of sending ABEL to Göttingen enter into the application when the collegium applied for the grant to be made effective. ABEL submitted a more detailed travel plan which RASMUSSEN was supposed to comment upon to the collegium; this plan is no longer extant.[33] Two months later, on September 7, ABEL embarked on his European tour.

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29 HANSTEEN'S opinion as expressed in the accommodating letter from the collegium academicum to the department of the church (N. H. Abel, 1902d, 7).

30 (ibid., 12).

31 (ibid., 7).

32 "*Efter at jeg saaledes her i Landet ved de her forhaanden værende Hjelpemidler har stræbet at nærme mig det foresatte Maal, vilde det være mig særdeles gavnligt ved et Ophold i Udlandet ved forskjellige Universiteter, især i Paris hvor saa mange i udmærkede Mathematikere findes, at blive bekjendt med de nyeste Frembringelser i Videnskaben og nyde de Mænds Veiledning som i vor Tidsalder have bragt den til en saa betydelig Høide.*" (ibid., 13).

33 (ibid., 20, footnote).

Figure 2.5: AUGUST LEOPOLD CRELLE (1780–1855)

2.3.2 ABEL in Berlin

After a brief stop to visit DEGEN in Copenhagen and another to visit SCHUMACHER in Hamburg,[34] ABEL'S first extended stay was in Berlin. He had not had any real plans of going to Berlin, but went there together with a group of friends, all of whom belong to the founding generation of Norwegian scientists, in particular in geology, and who were all on educational tours of Europe. ABEL wrote to HANSTEEN in his first report from the European tour where he obviously had to defend spending time in Berlin:

> "You may have wondered why I first traveled to Germany; I did so partly because I could thereby stay with friends and partly because I would be less likely not to make the most of my time since I can leave Germany at any time to go to Paris which should be the most important place for me."[35]

In the same letter, ABEL described the acquaintance which he had made with one of the local mathematicians, the professional administrator *Geheimrat* [Privy Councilor] CRELLE whom he had been told about by the Copenhagen mathematician H. G. v.

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34 Part of ABEL'S obligation was to carry out experiments for HANSTEEN measuring the Earth's magnetic field at different locations.

35 "*De har maaskee forundret Dem over hvorfor jeg først reiste til Tyskland; men dette gjorde jeg deels fordi jeg da kom til at leve sammen med Bekjendtere deels fordi jeg da var mindre udsat for ikke at anvende Tiden paa den bedste Maade, da jeg kan forlade Tyskland hvert Øjeblik det skal være for at reise til Paris, som bør være det vigtigste Sted for mig.*" (Abel→Hansteen, Berlin, 1825/12/05. N. H. Abel, 1902a, 9–10).

SCHMIDTEN (1799–1831). Introducing himself in his stuttering German, ABEL called upon the busy *Geheimrath* soon after his arrival in Berlin. Initially, their conversation was staggering; only when CRELLE enquired what ABEL had already read, did he realize that he was facing a young man quite versed in the modern mathematics. ABEL presented CRELLE with a copy of his pamphlet on the insolubility of the quintic equation, and CRELLE expressed his difficulty understanding the argument. In due time, ABEL would present CRELLE with an elaborated argument which would gain world wide circulation.

> "I am extremely pleased that I happened to go to Germany and in particular Berlin before I came to Paris; since — as you may have learned from my letter to Hansteen — I have made the splendid acquaintance with Geheimrath Crelle."[36]

The founding of the *Journal für die reine und angewandte Mathematik*. Communication of mathematics in the nineteenth century underwent rapid change. In the days of EULER, mathematics had been confined to professional amateurs and *academicians* who communicated their results either privately in correspondence, in monographs, or in the periodicals of the academies. Only in the beginning of the nineteenth century did professional, independent periodicals devoted to mathematics come into being; ABEL was instrumental in the creation of the first major German journal of mathematics, which CRELLE founded in 1826.

When ABEL first called upon CRELLE in Berlin, they discussed the relatively low status of mathematics in Germany (Prussia). When ABEL happened to mention his astonishment at the lack of German periodicals devoted to mathematics, he struck a nerve with CRELLE. For years, CRELLE had been engaged in an effort to promote mathematics in Prussia. In France, the first journal (*Annales de mathématiques pures et appliquées*) devoted entirely to mathematics had been initiated by J. D. GERGONNE (1771–1859) in 1810.[37] In 1822, CRELLE was forced to abandon plans for a German language journal of mathematics due to lack of contributors.[38] However, with the advent ABEL and other promising young mathematicians — all looking for a way of publishing their results — the time was ripe for another attempt. Following an intensive and continuing campaign to secure funding and with substantial personal investment, CRELLE had the first volume of his *Journal für die reine und angewandte Mathematik* published in the spring of 1826.

CRELLE'S initial idea for the *Journal für die reine und angewandte Mathematik* was to provide a broad German speaking audience with an instrument for presenting and keeping up to date with recent research in pure and applied mathematics — possibly

36 "*Overmaade vel fornøiet er jeg fordi jeg kom til at reise til Tyskland og navnligen til Berlin førend jeg kom til Paris; thi som Du maaskee har erfaret af mit Brev til Hansteen har jeg her gjort et fortræffeligt Bekjendtskab med Geiheimrath Crelle.*" (Abel→Holmboe, 1826/01/16. ibid., 13).

37 (Otero, 1997).

38 (W. Eccarius, 1976, 233).

through translations. Soon, however, the attention devoted to applied mathematics declined and the *Journal für die reine und angewandte Mathematik* became the mouthpiece for a limited group of pure mathematicians. The change in CRELLE'S conception of his *Journal für die reine und angewandte Mathematik* is evident by comparing the introductions with which he prefaced the two first volumes. In the very first volume, CRELLE described the domain of the journal to include both pure mathematics (analysis, geometry, mechanics), and applied mathematics including optics, theories of heat, sound, and probability, and geography and geodesy.[39] This changed quickly, though, and in the introduction to the second volume, CRELLE stood down — both on nationalistic and disciplinary ambitions.

For the first volume, CRELLE took it upon himself to translate the French manuscripts of ABEL and others into German before publication.[40] Despite having been taught German for four years at the Cathedral School (written German for two years),[41] ABEL'S marks in written German were quite inconsistent: 1 and 4 on a scale from 1 to 5 (1 best); for the *Artium*, he scored a 3.[42] ABEL was reluctant to write in that language. When he eventually prepared a paper in German, ABEL was very proud.[43] However, CRELLE soon succumbed to the pressure of internationalizing his journal and accepted publishing papers in foreign languages. In response, after just a single paper prepared in German, ABEL returned to writing exclusively in French.

CRELLE'S library. In Christiania, ABEL had access to a large section of French literature on pure mathematics written by the masters and some of the servants of the subject in the eighteenth and early nineteenth century. However, circulation of results to the geographical periphery was far from instant, and many of the products of the French reorganization of mathematics had not yet been brought to Norway. Therefore, it was an explicit motivation for ABEL'S European tour to go to the largest libraries and bookstores on the Continent which he expected to find together with the rest of the mathematical milieu in Göttingen and Paris. However, one of his most influential encounters with the libraries of the Continent took place in Berlin, probably in the private library of *Geheimrath* CRELLE.

> "The afore-mentioned Crelle also has a perfectly splendid mathematical library which I use as if it had been my own and from which I benefit particularly as it contains all the latest material which he gets as soon as possible."[44]

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39 (A. L. Crelle, 1826).
40 (W. Eccarius, 1976, 236). CRELLE also occasionally edited the manuscripts.
41 (Stubhaug, 1996, 520).
42 (N. H. Abel, 1902d, 3).
43 (Abel→Holmboe, Wien, 1826/04/16. N. H. Abel, 1902a, 27).
44 "*Den samme Crelle har ogsaa et aldeles fortræffeligt mathematisk Bibliothek, som jeg benytter som mit eget og som jeg har særdeles Nytte af da det indeholder alt det nyeste, som han faaer saa snart det er mueligt.*" (Abel→Hansteen, Berlin, 1825/12/05. ibid., 11).

A.-L. CAUCHY'S (1789–1857) new program of founding analysis on the notion of limits as expressed by inequalities had not reached Norway before ABEL left. CAUCHY had expressed his thoughts most influentially in the textbook *Cours d'analyse* intended (but never used) for instruction at the *École Polytechnique* which was printed in 1821. In a review of CAUCHY'S *Exercises de mathématiques*, CRELLE spoke highly of CAUCHY'S insights and his other works, and there can be little doubt that during ABEL'S time in Berlin, CAUCHY'S new analysis was discussed by a circle of mathematicians around CRELLE. One member of the circle, the mathematician M. OHM (1792–1872) [45] reacted to CAUCHY'S new rigorization by devising his own approach to algebraic analysis which was kept the formal aspect which CAUCHY had rejected.[46] In a letter, ABEL records how the circle discontinued its meetings because of G. S. OHM'S (1789–1854) arrogant mentality.[47] It is possible, that mathematical topics may also have played a role.

> "The work [*Exercises de mathématiques*] is full of deep analytical investigations as it would be expected from the acute and inventive author of *Cours d'analyse*, *Leçons sur le calcul infinitésimal*, *Leçons sur l'application du calcul infinitésimal à la géométrie*, etc."[48]

ABEL discovered the works of CAUCHY in 1826. CAUCHY'S new approach to the theory of infinite series took ABEL by storm, and soon ABEL became one of CAUCHY'S most devoted missionaries (see part III). In print, ABEL first disclosed his sympathies in his work on the binomial theorem, which was printed in the early spring of 1827 (see table 2.1), but ABEL'S letters allow us to date his encounter with the new Cauchian rigor in analysis more precisely. In a famous letter dated 16 January 1826, i.e. while in Berlin, ABEL wrote to HOLMBOE:

> "Taylor's theorem, the foundation for the entire higher mathematics, is equally ill founded. I have found only one rigorous proof which is by Cauchy in his *Resumé des leçons sur le calcul infinitesimal*."[49]

It is likely, that it was also in CRELLE'S library that ABEL came across CAUCHY'S famous textbook *Cours d'analyse*, a work which had tremendous consequences for ABEL'S attitude toward rigor.[50]

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45 For the years of birth and death, see (Jahnke, 1987, 103). OHM was the younger brother of the famous physicist OHM.

46 (Jahnke, 1987; Jahnke, 1993).

47 (Abel→Hansteen, Berlin, 1825/12/05. N. H. Abel, 1902a, 11).

48 "*Das Werk ist voller tiefer analytischer Untersuchungen, wie sie sich von dem scharfsinningen und an neuen Ideen reichen Verfasser des "Cours d'analyse", "Leçons sur le calcul infinitésimal," der "Leçons sur l'application du calcul infinitésimal à la géométrie, etc." erwarten lassen.*" (A. L. Crelle, 1827, 400).

49 "*Det Taylorske Theorem, Grundlaget for hele den høiere Mathematik er ligesaa slet begrundet. Kun eet eneste strængt Beviis har jeg fundet og det er af Cauchy i hans Resumé des leçons sur le calcul infinitesimal.*" (Abel→Holmboe, 1826/01/16. N. H. Abel, 1902a, 16).

50 (I. Grattan-Guinness, 1970b, 79). Again, this influence will be documented in part III.

Volume	Number	Time of publication
1	1	February–March 1826
1	2	June 1826
1	3	
1	4	February–March 1827
2	1	5 June 1827
2	2	20 September 1827
2	3	
2	4	12 January 1828
3	1	25 March 1828
3	2	26 May 1828
3	3	
3	4	3 December 1828
4	1	25 January 1829
4	2	28 March 1829
4	3	
4	4	10 June 1829

Table 2.1: Time of publications for CRELLE'S *Journal für die reine und angewandte Mathematik* 1826–1829. Compiled from SYLOW'S notes in (N. H. Abel, 1881, vol. 2).

The Berlin mathematical scene. In Berlin, mathematics was cultivated in three distinct — and largely disjoint — circles: the academy, the university, and the circle around CRELLE. The Academy had played its role in the era of academies when it had housed such eminent mathematicians as EULER and LAGRANGE. However, after LAGRANGE had moved to Paris in 1784 without being suitably replaced, mathematics at the Academy inevitably and irreversibly declined.[51]

In the first ordinary mathematics chair at the university — which had only opened in 1810 — J. G. TRALLES (1763–1822) had resided. The brothers A. VON HUMBOLDT (1769–1859) and W. VON HUMBOLDT (1767–1835), who had been instrumental in bringing the university into being, had made efforts to call GAUSS to the chair, but he had to settle for less; TRALLES' academic record shows a marked bias for *applied* mathematics, and during his reign pure mathematics was not well off in Berlin not was it elsewhere in Germany except for Göttingen.[52] Besides the ordinary professor, a number of extraordinary professors and *Privatdozenten*[53] offered mathematics courses. After TRALLES' death in 1822, E. H. DIRKSEN (1788–1850), who together with OHM had previously served as *Privatdozenten*, was appointed to the ordinary professorship.

The real forum for pure mathematics in Berlin in the 1820s centered around CRELLE and condensed around the *Journal für die reine und angewandte Mathematik* once it was initiated in 1826. CRELLE organized weekly meetings of a group of young mathemati-

51 (Knobloch, 1998).
52 (Biermann, 1988, 20–21), (Rowe, 1998)
53 These *Privatdozenten* include EYTELWEIN, GRUSON, LEHMUS, and LUBBE.

Author	Vol. 1	Vol. 2	Vol. 3	Vol. 4	1826–29
ABEL	97 (7)	100 (4)	56 (6)	125 (6)	378 (23)
STEINER	93 (5)	45 (7)	11 (3)		149 (15)
PONCELET		11 (2)	60 (1)	71 (1)	142 (4)
JACOBI	7 (1)	60 (9)	32 (7)	29 (2)	128 (19)
OLIVIER	66 (7)	54 (4)	1 (1)		121 (12)
DIRICHLET			62 (4)	18 (2)	80 (6)
MÖBIUS			36 (2)	36 (2)	72 (4)
HILL		3 (1)	59 (1)		62 (2)
CRELLE [a]	43 (3)			4 (1)	47 (4)
RAABE		44 (5)			44 (5)
PLÜCKER			13 (1)	22 (1)	35 (2)
CLAUSEN		2 (1)	13 (5)	15 (5)	30 (11)
HUMBOLDT				27 (1)	27 (1)
OLTMANNS		10 (1)		13 (1)	23 (2)
DIRKSEN	10 (2)			9 (1)	19 (3)
HORN		18 (2)		1 (1)	19 (3)
GRUNERT		17 (3)		1 (1)	18 (4)

[a] Only papers explicitly authored by "Crelle" or "The editor" are included. Besides these, many anonymous papers must be attributed to CRELLE.

Table 2.2: List of most productive (in pages, number of papers in parentheses) authors in CRELLE'S *Journal* 1826–29.

cians, mainly contributors to his *Journal für die reine und angewandte Mathematik*; but in the year of the founding of the journal, the meetings had to be discontinued due to personal conflicts within the group. However, the group reorganized under CRELLE'S initiative and met each Monday for a social event combining music and mathematics, and in smaller groups they discussed while strolling the city.

The members of CRELLE'S circle can only be identified indirectly from the list of authors of the early volumes of the *Journal* and from the correspondence of the identifiable central actors.[54] In ABEL'S letters to his friends in Norway, explicit mention is only made of OHM, the extraordinary professor at the university who by an arrogant nature caused the weekly seances to be discontinued. However, ABEL wrote about a few unnamed young mathematicians with whom he entertained himself.

2.3.3 Why never Göttingen?

Already in ABEL'S first letter to HANSTEEN from Berlin, a certain ambiguity concerning his obligation to go to Göttingen can be found:

> "My winter quarters will be here in Berlin and I have not yet decided when I am going to leave. For the sake of Crelle and the Journal, I would like to stay here

54 CRELLE'S *Nachlass* appears to have gone on auction shortly after CRELLE'S death in 1855 and must be considered lost. (W. Eccarius, 1975, 49, footnote)

as long as possible, and as I gather there is no other place in Germany where I would be better off. There is certainly a good library in Göttingen, but that is about all because Gauss who is the only capable one there is completely inapproachable. However, I must go to Göttingen."[55]

ABEL'S devotion toward CRELLE and the *Journal für die reine und angewandte Mathematik* seems to overpower his ambition and obligation to go to Göttingen in accord with the stipend. A month later, a further reservation was expressed in a letter to HOLMBOE, where ABEL referred to the personality of the great GAUSS:

> "I am probably going to remain here in Berlin until the end of February or March; then I will travel via Leipzig and Halle to Göttingen, not to see Gauss because he is said to be intolerably reserved, but for the library which is apparently excellent."[56]

The reservation concerning GAUSS was communicated to HANSTEEN a fortnight later, in a letter in which ABEL outlined his plans to accompany CRELLE on a trip to Göttingen. Furthermore, ABEL had already his eyes firmly set on Paris; he wrote the following to HANSTEEN after describing how he planed to go to Leipzig and Freiburg with one of his Norwegian friends:

> "Then I [will] travel back to Berlin in order to join Crelle on the tour to Göttingen and the Rhine area. In Göttingen, I shall only stay for a short period of time as there is nothing to be gained. Gauss is unapproachable and the library cannot possibly be better than those in Paris."[57]

ABEL'S impression that GAUSS was not easily accessible seems to have been inspired by rumors nourished in Berlin at the time. With his *few but ripe* policy concerning publications, GAUSS' image was largely built from his network of corresponding mathematicians. Modern biographers of GAUSS use the published correspondence with e.g. the F. BOLYAI (1775–1856) s, ABEL, and others to support the picture of the great mathematician as remote and even hostile. However, these events were only taking place, and the letters were not publicly known in the 1820s. The Berlin mathematicians certainly held GAUSS' mathematics in high respect but thought less of the master's personal qualities and openness. HUMBOLDT had — with the help of CRELLE — tried to call GAUSS to the Berlin polytechnic and later the university, but GAUSS had declined all such offers and remained in Göttingen.

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55 "*Mit Vinterquarteer kommer jeg til at holde her i Berlin og jeg er endnu ikke ganske enig med mig selv naar jeg skal reise herfra. For Crelles og Journalens Skyld vilde jeg gjerne være her saalænge som muelig og eftersom jeg hører er der vel intet andet Sted i Tyskland som vil være mig gavnligere. Göttingen har rigtignok et godt Bibliothek, men det er ogsaa det eneste; thi Gauss som er den eneste der der kan noget, er aldeles ikke tilgjængelig. Dog til Göttingen maae jeg det forstaaer sig.*" (Abel→Hansteen, Berlin, 1825/12/05. N. H. Abel, 1902a, 12).

56 "*Jeg kommer formodentlig til at blive her i Berlin til Enden af Februar eller Marts, og reiser da over Leipzig og Halle til Göttingen (ikke for Gauss Skyld, thi han skal være utaalelig stolt men for Bibliothekets Skyld som skal være fortræffeligt).*" (Abel→Holmboe, 1826/01/16. ibid., 18).

57 "*Siden reiser jeg tilbage til Berlin for i Følge med Crelle at tage Touren til Göttingen og Rhin-Egnene. I Göttingen bliver jeg kun kort da der ikke er noget at hole. Gauss er utilgjængelig og Bibliotheket kan ikke være bedre end i Paris.*" (Abel→Hansteen, Berlin, [1826]/01/30. ibid., 20).

2.3.4 The European detour

During February 1826, CRELLE'S possibilities to go to Göttingen crumbled and ABEL'S plans for the rest of the tour changed. Complaining repeatedly of his melancholic nature, ABEL hesitated and stalled at the prospect of travelling to Paris alone. Instead, his Norwegian friends presented an inviting alternative. With their interests largely in geology, the Norwegian travelling entourage made it for the Alps, and ABEL chose to join them. His defence of this less-than-obvious decision can be read in a very charming letter to HANSTEEN:

> "Can I then be blamed for wanting to see some of the southern life. During my journey I can work quite hard. Once I am in Vienna and on the road to Paris, the straight route almost passes through Switzerland. Why should I not also see a bit of that country. My lord! I am not completely without feelings for the beauty of Nature. The entire trip will postpone my arrival in Paris by two months and that does no harm. I will catch up. Do you not think that I would benefit from such a journey?"[58]

The reaction of the sponsor HANSTEEN can only be imagined.

On his detour through Europe, ABEL did continue working on his mathematics, and he called upon the local mathematicians where he could. In Vienna, ABEL brought a letter of introduction from CRELLE to the mathematician K. L. VON LITTROW (1811–1877) at the observatory. His encounter with LITTROW was perhaps the only strictly mathematical benefit gained from the detour itself; mediated by LITTROW, ABEL managed to circulate his result on the insolubility of the quintic equation in an even wider (albeit still German speaking) audience.[59]

2.3.5 Isolation in Paris

When ABEL eventually arrived in Paris in the summer of 1826, he found the mathematical scene abandoned: Most of the Paris mathematicians had left the city for the countryside. However, ABEL made a brief call to LEGENDRE, whom he described as "a really excellent old man".[60] ABEL later met with CAUCHY without having more than a brief and non-technical conversation with him. Besides the opportunity to meet with the Parisian mathematicians, ABEL saw in the famous *Académie des Sciences* a possibility for presenting his research. Before he left Norway, he had prepared a paper on the

58 *"Kan man da fortænke mig i at jeg ønsker ogsaa at see lidt af Sydens Liv og Færden. Paa min Reise kan jeg jo arbeide temmelig brav. Er jeg nu engang i Wien og jeg skal derfra til Paris saa gaaer jo den lige Vei næsten igjennem Schweitz. Hvorfor skulde jeg da ikke ogsaa see lidt deraf? Herre Gud! Jeg er dog ikke uden al Sands for Naturens Skjønheder. Den hele Reise vil gjøre at jeg kommer to Maaneder senere til Paris end ellers og det gjør da ikke noget. Jeg skal nok hente det ind igjen. Troer de ikke at jeg vil have godt af en saadan Reise?"* (Abel→Hansteen, Dresden, 1826/03/29. ibid., 24).

59 ABEL had his article from CRELLE'S *Journal* reviewed anonymously in the newly founded Vienna based journal *Zeitschrift für Physik und Mathematik* (Anonymous, 1826). The review will be described in part II.

60 (ABEL to HOLMBOE, Paris 1826.8.12, (N. H. Abel, 1902a, 40))

insolubility of the quintic equation which had, however, already served its purpose as a door-opener with CRELLE in Berlin where it had been published. Therefore, that result was not eligible for the academy and neither did ABEL submit the manuscript on the integration of differentials which he had also prepared for the collegium in Christiania. Instead, on October 10, ABEL presented a paper, the so-called *Paris memoir*, which he had worked out during the tour and his stay in Paris and which extended the collegium manuscript. As is described in much more detail in part IV, the Paris mémoire was an algebraic approach to the general theory of integration of algebraic differentials and extended the approach to elliptic transcendentals which ABEL had already embarked upon while in Germany.

ABEL'S entire production in Paris was directly influenced by the works of LEGENDRE on elliptic integrals which had recently appeared.[61] ABEL took a new approach to that theory and extended and advanced the program which LEGENDRE had begun to include higher transcendentals into analysis. Where the influence of LEGENDRE was thus clearly reflected in ABEL'S interests, the influence from CAUCHY was less direct. We know from his letters that ABEL bought and read the first issues of CAUCHY'S *Exercises de mathématiques* which include the first presentation of CAUCHY'S theory of complex integration. This particular theory was, however, completely without influence on ABEL'S inversion of elliptic integrals as is described in part II.

2.4 Back in Norway

When ABEL ultimately left Paris around the New Year 1826/27, he headed for Berlin where he worked with CRELLE on editing the *Journal* for some months. There was no permanent position for ABEL to return to in Norway; the only possibility, a teaching position at the University to replace professor RASMUSSEN, had been given to HOLMBOE while ABEL was in Paris. During ABEL'S second visit to Berlin, CRELLE intensified his efforts to convince ABEL to come work for the *Journal für die reine und angewandte Mathematik* on a permanent level and CRELLE began lobbying for a position for ABEL at one of the new institutions of higher education in Berlin. In the spring of 1827, ABEL finally headed back north, back to uncertainty in Christiania.

Back in his native country, ABEL made a living tutoring in mathematics and substituting for HANSTEEN while he went on an expedition to Siberia. Throughout, ABEL continued his mathematical research and his production was intensified when he learned that the young German mathematician JACOBI was producing astonishing results on an important problem in the theory of elliptic functions. A hectic competition ensued during which the two mathematicians advanced the theory far beyond its previous horizon. Initially, ABEL had been able to provide proofs of some of the claims which JACOBI had advanced without giving the proofs. Later, JACOBI'S results

61 (A. M. Legendre, 1811–1817).

1802, Aug. 5	NIELS HENRIK ABEL was born at Finnøy
1815	ABEL moved to Christiania to attend grammar school
1818	BERNT MICHAEL HOLMBOE took over ABEL's mathematics classes
1821	ABEL graduated from grammar school and was immatriculated at the university in Christiania
1823	ABEL visited DEGEN in Copenhagen
1825, Sept. 7	ABEL left for the European tour making the first stop in Berlin
1826, Jul. 10	ABEL arrived in Paris
1826, Dec.	ABEL left Paris and returned to Berlin
1827, May 20	ABEL returned to Christiania
1829, Apr. 6	NIELS HENRIK ABEL died at Froland

Table 2.3: Summary of NIELS HENRIK ABEL's biography

provided inspiration for problems which ABEL tackled. The competition was fierce and only ended when ABEL fell ill around Christmas 1828. Even from his sick-bed, ABEL signed the last papers which he sent for publication in CRELLE'S *Journal*. ABEL died at Froland on April 6 1829 at the age of 26. A few dates later good news arrived from Berlin: CRELLE had finally secured a position for ABEL in Berlin.

Chapter 3

Historical background

The social and institutional conditions of mathematics resemble the general social context in undergoing abrupt transitions in the late eighteenth and early nineteenth century. The period is one of only a handful of instances where the influences of political and social changes can be witnessed directly in mathematical institutions and research. The main argument of the present chapter will be to establish that not only the external, outer shell of mathematics was transformed; the very core of mathematics, the ways mathematicians thought about their subject, was also influenced by social and political upheaval.[1]

3.1 Mathematical institutions and networks

Mathematical circles. The metaphor of *center* and *periphery* has aptly been applied to describe science conducted in geographically "remote" regions such as Scandinavia during periods when the most prominent contributions were made at larger centers in Central Europe.[2] However, as described below, in the case of mathematics in the early nineteenth century a curious picture emerges with a very strong mathematical center in Paris and two lesser centers in Germany: the Gaussian ivory tower in Göttingen and the emerging mathematical scene in Berlin centered around the university, A. L. CRELLE (1780–1855), and his *Journal*. In the following section, N. H. ABEL'S (1802–1829) position within the two mathematical traditions unfolding at these centers — for short denoted the French and German traditions — is described.

Norway. Knowledge of the Norwegian national history during the early nineteenth century is of importance for understanding the conditions under which ABEL grew

1 Some of the historical facts and circumstances have already been touched upon in the preceding chapters but are taken up here again from a slightly different perspective.

2 The terms were first employed in (Shils, 1961) and have since found their way into the history of science.

up and evolved into a prominent mathematician.[3] In the middle of the nineteenth century, national states appeared as the natural units of political and military power. Before that, in the early nineteenth century, unitary states had been the most obvious ways of organizing power. In Scandinavia, two unitary states had coexisted as rivals for centuries, the Danish-Norwegian monarchy and the Swedish monarchy. In 1814, in the aftermath of the Napoleonic Wars, Norway was separated from the twin-monarchy and ceded to Sweden. During the war, a nationalist sentiment had grown strong in Norway; this national pride led to a brief period of independence before the transition to Swedish rule.

Receiving all of his formal education within Norwegian institutions, ABEL belongs to the first generation of truly *Norwegian* scientists. Before the creation of the university in 1813, Norwegians who wanted any kind of higher education had to go to Copenhagen; quite a number of them went and later returned to fill administrative or clerical positions in Norway.

At the Christiania Cathedral School, ABEL received qualified and personal tutoring from B. M. HOLMBOE (1795–1850); and at the University he became the prodigy of the professors S. RASMUSSEN (1768–1850) and C. HANSTEEN (1784–1873). HOLMBOE was ABEL'S senior by only seven years and had been among the very first students to attend the Christiania university where he sat in the mathematics classes of RASMUSSEN. Such relations between prodigies and benefactors may well originate in the fact that the scientific community in Christiania was rather small and yet led by a few men with international relations.

France. In France (in the early nineteenth century that almost exclusively meant Paris), by contemporary standards, the scientific community was anything but small. After the Revolution, educational reforms were introduced to develop the military and civil engineering in France. In order to achieve this goal, large-scale and very advanced instruction in mathematics was set up at two newly founded educational institutions, the *École Normale* and the *École Polytechnique*. A mathematical milieu of substantial size and quality established itself in the French capital in the decades following the Revolution. Teaching at either the *École Polytechnique* or the *École Normale* provided a means of living for mathematicians; and the *Académie des Sciences* provided a possibility to communicate mathematical research. The focus on the teaching of engineers and the sheer volume of classes exerted influences on the contents of the mathematics taught and researched; the eighteenth century prevalence of applicable mathematics continued to dominate, but the communication of the calculus to the previously un-initiated helped provoke research on the foundations of the topic. The liberal ideas of the Revolution meant a dramatic increase in the numbers of publications. This general trend also influenced mathematics; mathematicians could more

[3] The cultural framework of Norwegian society in the first decades of the nineteenth century has been aptly described in (Stubhaug, 1996; Stubhaug, 2000); here only a few aspects need repetition.

easily have their works either printed or published in one of the journals which were set up.[4] However, perhaps as a consequence of its size, the milieu in Paris was a very competitive one in the first decades of the nineteenth century;[5] teachers probably seemed to a Norwegian more reserved there than in Christiania, and mathematical cooperation was actually rarely seen.

German states. In the early nineteenth century, the German speaking region was organized in a multitude of sovereign states. One of the most influential and ambitious ones, Prussia, was dominated from its capital Berlin where a national awareness was spreading to the sciences and mathematics in the 1820s.

The German reaction to the events in France around the turn of the century found a philosophical grounding in the neo-humanistic movement which sought to reintroduce classical ideals of humanism and learning through educational reforms. These reforms — promoted in Prussia by W. VON HUMBOLDT (1767–1835) and others — improved the position of mathematics within the curriculum and promoted a particular view of mathematics.[6] In the opinion of the neo-humanists, mathematics was not to be cultivated for its applicability in the sciences; instead, a "pure" form of mathematics was promoted with its own set of motivations and ideologies. The important mathematical branch of algebraic analysis found a unique form with these philosophically inspired mathematicians in the form of the German *combinatorial school*.[7]

In order to implement the educational reforms, new institutional constructions were devised. In Berlin, a university was opened in 1810 which included mathematics; plans for a polytechnic school — where mathematics should also be taught — had to be postponed but were carried out in the 1820s. Thus, in Prussia, instruction in mathematics of the future teachers — with its focus on pure thought and individual contemplation and research — became institutionalized in the first decades of the nineteenth century.[8] Outside the realm of the university, a group of mathematicians gathered around CRELLE'S *Journal für die reine und angewandte Mathematik*; in the 1820s, this group constituted an alternative mathematical forum in Berlin and a place for younger mathematicians to meet.

3.2 ABEL's position in mathematical traditions

Within the Continental tradition in mathematics. ABEL'S interactions with European mathematics were almost exclusively confined to the centers in Berlin and Paris. In the 1820s, a few other peripheral communities of mathematics existed; in particu-

4 (J. Dhombres, 1985; J. Dhombres, 1986).
5 (I. Grattan-Guinness, 1982) and (I. Grattan-Guinness, 1990, e.g. 1227).
6 (Pyenson, 1983).
7 (Jahnke, 1990; Jahnke, 1996). The combinatorial school is briefly treated upon in Part III. For a fuller account, consult (Jahnke, 1987; Jahnke, 1993).
8 (Begehr et al., 1998; Biermann, 1988; Mehrtens, 1981).

lar on the British Isles, in the Austrian-Hungarian Empire, and in Russia. There are, however, nearly no traces of any kinds of interactions with these mathematicians to be found in ABEL'S works or letters.

In the first decades of the nineteenth century, British mathematicians (both outside and within the *Analytical Society*) were consciously trying to adapt the Continental calculus.[9] Only once — in an 1823-letter to HOLMBOE — did ABEL mention the two contemporary British mathematicians T. YOUNG (1773–1829) and J. IVORY (1765–1842), neither of whom were associated with the *Analytical Society*.[10] From a single sentence in one of his letters, we know that ABEL was aware of the existence of the Czech theologian, philosopher, and mathematician B. BOLZANO (1781–1848). In one of his notebooks, ABEL makes the following rather sudden remark, which initially confused the P. L. M. SYLOW (1832–1918), who by 1902 still only knew of Bolzano as a town in the Alps:[11]

> "Bolzano is a clever fellow from what I have studied"[12]

This remark made SYLOW speculate that ABEL had probably read BOLZANO'S *Rein analytischer Beweis* during the European tour.[13] There is nothing to suggest that ABEL met personally with BOLZANO during his stop in Prague during the European detour.[14] In chapter 12.1, ABEL'S contribution to the rigorization of analysis is described in further detail, and a few more details concerning his relation to BOLZANO are discussed.

Although interested in topics central to the endeavors of the Analytical Society and BOLZANO (certainly independently), ABEL'S inspirations thus seem to come from somewhere else: the centers of Berlin and Paris.

Between the German and the French traditions. ABEL'S mathematical production was confined to the discipline of algebra, the foundations of analysis, and the theory of higher transcendentals. Thus, his interests did not include geometry and — except for a youthful work — also excluded applied mathematics. Although these topics constitute important parts of the French and German traditions in mathematics in the period,[15] ABEL'S work is nevertheless rightfully interpreted within these

9 (Craik, 1999).

10 (N. H. Abel, 1902a, 4–8). IVORY had studied mathematics in Scotland before taking up the subject as his profession. In 1819, he retired to become a private mathematician living in London. YOUNG was an autodidact natural philosopher with an interest in mathematics. He was elected into the French *Académie des Sciences* in 1827; ABEL and JACOBI had also been nominated for the election.

11 (L. Sylow, 1902, 12).

12 "*Bolzano er en dygtig Karl i hvad jeg [. . .]*" (Abel, MS:351:A, 61). The rest is crossed out and difficult to decipher. See (Stubhaug, 2000, 505, fig. 44).

13 (Bolzano, 1817), see (L. Sylow, 1902, 12).

14 Historical speculations based on the similarities of results and the *possible* personal rendezvous of the authors have been found inadequate, as the responses to GRATTAN-GUINNESS' provocative suggestion that CAUCHY plagiarized BOLZANO (I. Grattan-Guinness, 1970a); for a sober review of the ensuing controversy, see (Bottazzini, 1986, 123–124 (note 10)).

15 See e.g. (Dauben, 1981) or (Jahnke, 1994).

traditions. The new French approach to rigor and the German focus on pure mathematics (i.e. mathematics without the justification of applicability) were important backgrounds for ABEL'S mathematics; no expressed concerns for applicability can be found in ABEL'S writings; he was — in every respect — a pure mathematician. On the other hand, as will become evident in chapters 16 and 19, ABEL was no dogmatic rigorist when he aimed at producing new mathematical results.

3.3 The state of mathematics

Some tendencies in the mathematicians' thoughts about mathematics just prior to the period of main interest merit attention. In particular, prominent mathematicians toward the end of the eighteenth century expressed the belief that mathematics was just about to reach its pinnacle of cultivation. From the eighteenth century perspective, where mathematical praxis was to a large part made up of formal and explicit manipulations of known algebraic or analytic dependencies, these methods seemed of limited scope. For instance, J. L. LAGRANGE (1736–1813) — a few years after his innovative paper on polynomial equations from which our story will commence in the next chapter — wrote to J. LE R. D'ALEMBERT (1717–1783) in 1781,

> "It appears to me also that the mine [of mathematics] is already very deep and that unless one discovers new veins it will be necessary sooner or later to abandon it."[16]

Similar dark visions seem to be recurring at intervals — often in the form of *fin-de-siècle pessimism*. Even into the nineteenth century, a similar view was expressed by J.-B. J. DELAMBRE (1749–1822). In 1810, DELAMBRE delivered a commissioned review of the progress made in the mathematical sciences after the French Revolution. He expressed his concern over the future of mathematics in the following way:

> "It would be difficult and rash to analyze the chances which the future offers to the advancement of mathematics; in almost all its branches one is blocked by insurmountable difficulties; perfection of detail seems to be the only thing which remains to be done. [...] All these difficulties appear to announce that the power of our analysis is practically exhausted in the same way as the power of the ordinary algebra was with respect to the geometry of transcendentals at the time of Leibniz and Newton, and it is required that combinations are made which open a new field in the calculus of transcendentals and in the solution of equations which these [transcendentals] contain."[17]

16 "*Il me semble aussi que la mine est presque déjà trop profonde, et qu'à moins qu'on ne découvre de nouveaux filons il faudra tôt ou tard l'abandonner.*" (Lagrange→d'Alembert, Berlin, 1781. Lagrange, 1867–1892, vol. 13, 368); English translation from (Kline, 1990, 623).

17 "*Il seroit difficile et peut-être téméraire d'analyser les chances que l'avenir offre à l'avancement des mathématiques: dans presque toutes les parties, on est arrêté par des difficultés insurmontables; des perfectionnemens de détail semblent la seule chose qui reste à faire; [...] Toutes ces difficultés semblent annoncer que la puissance de notre analyse est à-peu-près épuisée, comme celle de*

Taken together, the two quotations hint at two possible ways out of the apparently imminent stagnation of mathematics: discovery of new questions (veins) and fusions of existing theories. After the evidence has been presented in the following three parts, it will be illustrated how the mathematical community of the early nineteenth century invoked precisely these approaches in a period of such mathematical creativity that the remarks of LAGRANGE and DELAMBRE afterwards seem well off the mark.

3.4 ABEL's legacy

It is well known that after ABEL'S death, his name was tied to a romantic picture of a neglected mathematical genius. His case was used as fuel for arguments ranging from nationalistic awareness to revolutionary issues. His influence on the ensuing century was vast — both as mathematical and even personal inspiration. Here, only ABEL'S mathematical legacy will be discussed, although certain aspects of his personality and destiny are present in the quotations given. More importantly than contributing new results to mathematics, ABEL'S programmatic approach caught the attention of the leading figures in mathematics in the nineteenth century, in particular C. G. J. JACOBI (1804–1851) and K. T. W. WEIERSTRASS (1815–1897).

The judgement on ABEL'S legacy passed by A.-M. LEGENDRE (1752–1833) and JACOBI is legendary, itself. In a letter to CRELLE, LEGENDRE is reported to have said:

> "After having worked by myself, I have felt a great satisfaction paying homage to Mr. Abel's talents, feeling all the merits of the beautiful theorem the discovery of which is due to him and to which the qualification *monumentum aere perennius* can be applied."[18]

JACOBI, who reported LEGENDRE'S homage to ABEL in his review of the third supplement of LEGENDRE'S *Théorie des fonctions elliptiques*, at another place qualified and generalized the praise which should be given to ABEL'S contribution to mathematics:

> "The vast problems which he [ABEL] had proposed to himself — i.e. to establish sufficient and necessary criteria for any algebraic equation to be solvable, for any integral to be expressible in finite terms, his admirable discovery of the theorem encompassing all the functions which are the integrals of algebraic functions, etc. — marks a very special type of questions which nobody before him had dared to imagine. He has gone but he has left a grand example."[19]

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l'algèbre ordinaire l'étoit par rapport à la géométrie transcendante au temps de Leibnitz et de Newton, et qu'il faut des combinaisons qui ouvrent un nouveau champ au calcul des transcendantes et à la résolution des équations qui les contiennent." (Delambre, 1810, 131); translation based on Kline, 1990, 623.

18 "*En travaillant pour mon propre compte, j'ai éprouvé une grande satisfaction, de rendre un éclatant hommage au génie de Mr.* Abel*, en faisant sentir tout le mérite du beau théorème dont l'invention lui est due, et auquel on peut appliquer la qualification de* monumentum aere perennius." Legendre quoted in C. G. J. Jacobi, 1832a, 413.

As can be seen from the quote, to JACOBI, ABEL'S legacy laid more in the way he asked questions than in the solutions and answers which he provided. ABEL'S questions were, in the mind of JACOBI, questions of necessary and sufficient conditions for certain properties to hold. This interpretation highlights two aspects which find instances in the present work: rigorization as the process of making clear, useful, and precise the conditions of theorems, and the new, concept based questions which are guaranteed to be answerable, although the answers might defy contemporary intuitions.

In his youth, WEIERSTRASS was deeply influenced in his career by works written by ABEL. Throughout his life, WEIERSTRASS thought highly of ABEL; the following statements testify to WEIERSTRASS' devotion which at times almost resembles envy.

> "The fortunate Abel; he has contributed something of lasting value! — He [ABEL] was used to always taking the most elevated point of view. — Abel was distinguished by the all-embracing vision directed at the highest position, the ideal."[20]

These quotations are, of course, equally good sources to WEIERSTRASS' views on mathematics in the second half of the nineteenth century as to ABEL'S mathematical production in the 1820s. However, the quotations touch upon the same themes as the quotation from JACOBI above; in due time it will be clearer, on which basis WEIERSTRASS could claim that ABEL had produced lasting results by taking the most general approach toward the idealistic goal of mathematics.

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19 "*Les vastes problèmes qui'il s'était proposés, d'établir des critères suffisants et nécessaires pour qu'une équation algébrique quelconque soit résoluble, pour qu'une intégrale quelconque puisse être exprimée en quantités finies, son invention admirable de la propriété générale qui embrasse toutes les fonctions qui sont des intégrales de fonctions algébriques quelconques, etc., etc., marquent un genre de questions tout à fait particulier, et que personne avant lui n'a osé imaginer. Il s'en est allé, mais il a laissé un grand exemple.*" (Jacobi→Legendre, Potsdam, 1829. Legendre and Jacobi, 1875, 70–71); for a German translation, see (Pieper, 1998, 153).

20 "*Abel der Glückliche; er hat etwas Bleibendes geleistet! — Er [*ABEL*] war gewohnt, überall den höchsten Standpunkt einzunehmen. — Abel zeichnete der allumfassende, auf das höchste, das Ideale gerichtete Blick aus.*" The quotes are all taken from (Biermann, 1966, 218).

Part II

“My favorite subject is algebra”

Chapter 4

The position and role of ABEL's works within the discipline of algebra

In the nineteenth century, the theory of equations acquired its status as a mathematical discipline with its own set of problems, methods, and legitimizations. In the process, N. H. ABEL (1802–1829) played an important role. His works on the algebraic insolubility of the general quintic equation and his penetrating studies of the so-called *Abelian* equations belong to the first results established within this incipient discipline.

Although ABEL'S investigations raised new questions and answered some of them, his methods and his approach was deeply rooted in the works of mathematicians belonging to the previous generations. In particular, ABEL drew upon the algebraic researches of L. EULER (1707–1783). Therefore, in the following chapter 5, these works, similar approaches taken by A.-T. VANDERMONDE (1735–1796), and the even more influential works by J. L. LAGRANGE (1736–1813) and C. F. GAUSS (1777–1855) are described and analyzed. In the ensuing chapters 6–8, ABEL'S algebraic researches are described and their role and impact are analyzed. Focus in this part II will be on describing the change in asking and answering questions pertaining to mathematical objects; more precisely questions concerning the algebraic solubility of equations. Such questions have been central to mathematical development since the Renaissance, but starting in the second half of the eighteenth century, they gave rise to a new mathematical theory. Once this theory-building has been described, the attention is directed toward ABEL'S approach to algebraic questions. ABEL'S studies of the quintic equation provide an example of how a change in the process of asking questions led to unexpected answers. Then, because of the similarity in methods and inspirations, ABEL'S questions concerning the geometric division of the lemniscate are treated to illustrate how an algebraic topic emerged within an apparently non-algebraic realm. Finally, the quest — taken up by ABEL and slightly later by E. GALOIS (1811–1832) — to completely describe solvable equations is outlined to provide a first illustration of the new and more abstract kind of questions which C. G. J. JACOBI (1804–1851) saw as ABEL'S greatest legacy.[1]

4.1 Outline of ABEL's results and their structural position

In the penultimate year of the eighteenth century, the Italian P. RUFFINI (1765–1822) had published the first proof of the impossibility of solving the general quintic algebraically. Working within the same tradition as ABEL, RUFFINI published his investigations on numerous occasions; however, his presentations were generally criticized for lacking clarity and rigour. Not until 1826 — after ABEL had published his proof of this result[2] — did ABEL mention RUFFINI'S proofs, and there is reason to believe that ABEL obtained his proof independently of RUFFINI, yet from the same inspirations.

From LAGRANGE'S comprehensive study of the solution of equations[3] originated the idea of studying the numbers of formally distinct values which a rational function of multiple quantities could take when these quantities were permuted. The idea was cultivated and emancipated into an emerging theory of permutations by A.-L. CAUCHY (1789–1857) who in 1815 provided the theory of permutations with its basic notation and terminology.[4] CAUCHY also established the first important theorem within this theory when he proved a generalization of one of RUFFINI'S results to the effect that no function of five quantities could have three or four different values under permutations of these quantities.

Insolubility of the quintic. ABEL combined the results and terminology of CAUCHY'S theory of permutations with his own innovative investigations of *algebraic expressions* (radicals). ABEL'S proof is a representation of his approach to mathematics. Once he had realized that the quintic might be unsolvable, he was led to study the "extent" of the class of algebraic expressions which could serve as solutions: the "expressive power" of algebraic solutions. Following a minimalistic definition of algebraic expressions, ABEL classified these newly introduced objects in a way imposing a hierarchic structure in the class of radicals. The classification enabled ABEL to link algebraic expressions — formed from the *coefficients* — which occur in any supposed solution formula to rational functions of the *roots* of the equation. By the theory of permutations, which ABEL had taken over from CAUCHY, he reduced such rational functions to only a few standard forms. Considering these forms individually, ABEL demonstrated — by *reductio ad absurdum* — that no algebraic solution formula for the general quintic could exist.

1 See p. 44, above.
2 (N. H. Abel, 1824b; N. H. Abel, 1826a).
3 (Lagrange, 1770–1771).
4 (A.-L. Cauchy, 1815a).

In the first part of the nineteenth century, the century-long search for algebraic solution formulae was brought to a *negative* conclusion: no such formula could be found. To many mathematicians of the late eighteenth century such a conclusion had been *counter-intuitive*, but owing to the work and utterings of men like E. WARING ($\sim$1736–1798),[5] LAGRANGE, and GAUSS the situation was different in the 1820s.

ABEL'S proof was also met with criticism and scrutiny. By and large, though, the criticism was confined to *local* parts of the proof. The *global* statement — that the general quintic was unsolvable by radicals — was soon widely accepted.

Abelian equations. In his only other publication on the theory of equations, *Mémoire sur une classe particulière d'équations résolubles algébriquement* 1829, ABEL took a different approach. The paper was inspired by ABEL'S own research on the division problem for elliptic functions and GAUSS' *Disquisitiones arithmeticae*. In it, ABEL demonstrated a *positive* result that an entire class of equations — characterized by relations between their roots — were algebraically solvable.

For his 1829 approach, ABEL seamlessly abandoned the permutation theoretic pillar of the insolubility-proof. Instead, he introduced the new concept of *irreducibility* and — with the aid of the Euclidean division algorithm — proved a fundamental theorem concerning irreducible equations.

The equations which ABEL studied in 1829 were characterized by having rational relations between their roots.[6] Using the concept of irreducibility, ABEL demonstrated that such irreducible equations of composite degree, $m \times n$, could be reduced to equations of degrees m and n in such a way that only one of these might not be solvable by radicals. Furthermore, he proved that if *all* the roots of an equation could be written as iterated applications of a rational function to one root,[7] the equation would be algebraically solvable.

The most celebrated result contained in ABEL'S *Mémoire sur une classe particulière* was the algebraic solubility of a class of equations later named *Abelian* by L. KRONECKER (1823–1891). These equations were characterized by the following two properties: (1) all their roots could be expressed rationally in one root, and (2) these rational expressions were "commuting" in the sense that if $\theta_i(x)$ and $\theta_j(x)$ were two roots given by rational expressions in the root x, then

$$\theta_i \theta_j (x) = \theta_j \theta_i (x) .$$

By reducing the solution of such an equation to the theory he had just developed, ABEL demonstrated that a chain of *similar* equations of decreasing degrees could be constructed. Thereby, he proved the algebraic solubility of *Abelian* equations.

5 1734 is a more qualified guess for Waring's year of birth than (Scott, 1976) giving "around 1736". See (Waring, 1991, xvi).

6 (N. H. Abel, 1829c).

7 I.e. an equation in which the roots are $x, \theta(x), \theta(\theta(x)), \ldots, \theta^n(x)$ for some rational function θ.

ABEL planned to apply this theory to the division problems for circular and elliptic functions. However, only his reworking of GAUSS' study of cyclotomic equations was published in the paper.

Together, the insolubility proof and the study of *Abelian* equations can be interpreted as an investigation of the extension of the concept of algebraic solubility. On one hand, the insolubility proof provided a negative result which limited this extension by establishing the existence of certain equations in its complement. On the other hand, the *Abelian* equations fell *within* the extension of the concept of algebraic solubility and thus ensured a certain power (or volume) of the concept.

Grand Theory of Solubility. In a notebook manuscript — first published 1839 in the first edition of ABEL'S *Œuvres* — ABEL pursued further investigations of the extension of the concept of algebraic solubility. In the introduction to the manuscript, he proposed to search for methods of deciding whether or not a given equation was solvable by radicals. The realization of this program would, thus, have amounted to a complete characterization of the concept of algebraic solubility.

ABEL'S own approach to this program was based upon his concept of *irreducible equations*. In the first part of the manuscript — which appears virtually ready for the press — ABEL gave his definition of this concept. Arguing from the definition, he proved some basic and important theorems concerning irreducible equations.

In the latter part of the manuscript — which is less lucid and toward the end consists of nothing but equations — ABEL reduced the study of algebraic expressions satisfying a given equation of degree μ to the study of algebraic expressions which could satisfy an *irreducible Abelian* equation whose degree divided $\mu - 1$. However, ABEL'S researches were inconclusive. When ABEL'S attempt at a general theory of algebraic solubility eventually was published in 1839, another major player in the field, GALOIS, had also worked on the subject. Inspired by the same tradition and exemplary problems as ABEL had been, GALOIS put forth a very general theory with the help of which the solubility of any equation could — at least in principle — be decided.

GALOIS' writings were inaccessible to the mathematical community until the middle of the nineteenth century. His style was brief and — at times — obscure and unrigorous. Many mathematicians of the second half of the nineteenth century — starting with J. LIOUVILLE (1809–1882) who first published GALOIS' manuscripts in 1846 — invested large efforts in clarifying, elaborating, and extending GALOIS' ideas. In the process, the theory of equations finally emerged in its modern form as a fertile subfield of modern algebra. Part of this evolution concerned mathematical styles. The highly *computational* mathematical style of the eighteenth century, to which ABEL had also adhered, was superseded. The old style had been marked by lengthy, rather concrete, and painstaking algebraic manipulations. In the nineteenth century, this was replaced by a more *conceptual* reasoning, early glimpses of which can be seen in ABEL'S works on the algebraic solubility of equations.

4.2 Mathematical change as a history of new questions

A permeating theme of the present work is the emergence of new questions in the early nineteenth century. The description and analyses of ABEL'S algebraic works serve to illustrate three aspects of this process:

1. New questions may have unexpected answers which push mathematics forward.
2. New and fertile questions may arise from importing methods or inspirations from one theoretical complex into another; entirely new theories may develop.
3. A deliberate reformulation of hard but improperly formulated questions may transform them into forms more open to mathematical treatment. The process of reformulating the question may involve a process of scrutinizing mathematical intuitions.

New questions with unexpected answers. Ever since procedures to algebraically compute the roots of cubic and bi-quadratic equations were discovered in the middle of the sixteenth century, the search had been on for a generalization to quintic equations. Once R. DU P. DESCARTES' (1596–1650) new notational system translated the problem into purely algebraic manipulations of symbols, the belief became widespread that such a generalization had to be obtainable. Although the goal defied even the greatest mathematicians for centuries, the belief remained intact as late as the second half of the eighteenth century. EULER, for instance, felt assured enough about the general algebraic solubility of equations to utilize it as the basis for proofs of another almost self-evident result: the fundamental theorem of algebra.

In 1770, LAGRANGE decided to study carefully the reasons behind the solubility of equations of degrees 1,2,3, and 4 with the hope of obtaining some kind of general procedure which could subsequently be applied to the fifth degree equation. LAGRANGE'S investigations were important in two respects: firstly, they provided a theorization of the problem into problems of permutations of the roots — a mathematical tool which would become immensely important for the problem, and secondly, LAGRANGE envisioned that the powers of his analysis *were not* powerful enough to deduce the desired result. This second observation can be taken as the first hint that such solutions were beyond the reach of humans.

In the last years of the eighteenth century, the full consequences of the failure to obtain algebraic solutions to the general quintic were realized and published independently by two mathematicians located at opposite ends of the professional spectrum: the German "prince of mathematics" GAUSS and the much lesser known Italian RUFFINI. In 1799, GAUSS remarked as a criticism of EULER that the algebraic solubility of equations should not be taken for granted. The same year, RUFFINI published the first of a series of proofs that the general fifth degree equation could *not* be solved algebraically. RUFFINI'S proofs were, as noted, difficult and had little impact, although RUFFINI communicated with some of the Parisian mathematicians. Instead, mathematicians took some notice of GAUSS' 1801 claim in the prestigious *Disquisitiones arithmeticae* to possess a rigorous proof of the insolubility of the quintic equation.

During the third decade of the 19th century, the question was finally resolved by ABEL and — more generally — by GALOIS. In some respect, RUFFINI had already obtained the answer in 1799, and comparing the proofs of RUFFINI, ABEL, and GALOIS sheds interesting light on the intra- and extra-mathematical mechanisms behind the establishment of mathematical knowledge.

ABEL'S proof of the insolubility of the quintic is a fascinating combination of previously established results and an approach designed to make the question addressable. Despite lacking in certain respects, ABEL'S proof and its conclusion soon gained wide acceptance among the experts. However, for many years to come, some mathematicians found the conclusion so counter-intuitive that they had to doubt the result. It is in this respect that the question led to an unexpected answer.

Asking algebraic questions of transcendental objects By 1823, ABEL had carefully studied GAUSS' *Disquisitiones arithmeticae*, although no explicit reference to it was made in the insolubility-proofs. When ABEL reacted upon a suggestion by C. F. DEGEN (1766–1825) to turn his attention toward the study of higher transcendentals, he found ample inspiration from GAUSS' work. In the *Disquisitiones*, GAUSS applied algebraic studies to the problem of constructing regular polygons with the help of ruler and compass. GAUSS suggested that the method could be carried over to the division problem for curves whose rectification depended on a simple elliptic integral, the lemniscate integral. In a large paper, which included the foundation of the new objects *elliptic functions*, ABEL provided the details supporting GAUSS' claim and was led to a new class of polynomial equations which were always solvable.

Thus, in the midst of a realm apparently inherited by highly transcendental objects, ABEL focuses upon algebraic relations pertaining to and existing among these objects. The theory of higher transcendentals was in a phase of transition in the period, and ABEL'S algebraic focus influenced the future developments during the second quarter of the nineteenth century.

The art of asking answerable questions An important ingredient in bringing about the change in attitude toward the solubility of the quintic had been ABEL'S way of asking questions. In a passage in one of his notebooks, ABEL emphasized that any mathematical problem, when formulated properly, is decidable — be it affirmatively or not.[8] Thus, for goals which had remained unattainable for years, ABEL suggested

8 The belief is also present in HILBERT'S "Wir müssen wissen, wir werden wissen." However, as the development in the twentieth century showed, such a belief has to take into account the accepted

a reformulation of the problem to a question of the form "is this goal achievable?" In the case of the quintic equation, the search for an algebraic solution was reformulated to a question whether such a solution existed at all.

Such change of attitude toward mathematical goals signal — as JACOBI soon realized — a change toward more general and abstract mathematics. In order to answer questions concerning possibility of existence, ABEL used implicit quantification over all possible solutions to the question. His approach was based upon the classification and normalization of these objects which were therefore studied — not individually — but as items belonging to a collection defined by a concept. Thus, a concept based approach to doing mathematics was intimately connected to the kinds of questions asked and addressed.

In the theory of equations, having established the existence of both algebraically solvable and unsolvable exemplars, ABEL raised the question of determining directly whether a given equation would be solvable or not. In a notebook manuscript, ABEL set out to address this question. For certain types of equations, he made some progress; however, it was left to GALOIS to outline a theory, based on the same inspirations as ABEL, which — when elaborated — was powerful enough to answer the question.

rules of mathematical reasoning and the system of primitive truth from which deductions are made.

Chapter 5

Towards unsolvable equations

By the dawn of the nineteenth century, the theory of equations addressed a wide range of questions. For the present purpose, the main question is the one of algebraic solubility, but in the eighteenth century, a multitude of other questions concerning existence and characterization of roots were intertwined with it. Therefore, in order to broaden the perspective, aspects of the history of these approaches are briefly outlined.

The existence of roots. When R. DU P. DESCARTES (1596–1650) in 1637 claimed that any equation of degree n possessed n roots an important theorem of algebra was formulated whose proof became central to subsequent development.[1] His way out was a rather evasive one which consisted of distinguishing the real ones (real meaning "in existence") from the imaginary ones which were products of human imagination. To DESCARTES the assertion that any equation of degree n had n roots took the form of a general property possessed by all equations and the trick of introducing the imagined[2] roots saved him from further argument.[3]

> "Neither the true nor the false roots are always real; sometimes they are imaginary; that is, while we can always conceive of as many roots for each equation as I have already assigned; yet there is not always a definite quantity corresponding to each root so conceived of."[4]

To the next generations of mathematicians the character of the core of the theorem changed slightly. Where DESCARTES had not dealt with the nature of the imagined roots, they did. Soon the problem of demonstrating that all (imagined) roots of a

1 In fact it had been formulated by GIRARD in 1629 (Gericke, 1970, 65).

2 I shall use the term "imagined" to distinguish it from the current technical term "imaginary". The word "complex" will be used to denote "imaginary" in the historical sense, i.e. numbers of the form $a + b\sqrt{-1}$ where a, b are real and $b \neq 0$.

3 Since the time of CARDANO, negative roots had been called *false* or *fictuous* roots. The *true* roots of which DESCARTES spoke were the *positive* ones.

4 "*Au reste tant les vrayes racines que les fausses ne sont pas tousiours reelles; mais quelquefois seulement imaginaires; c'est a dire qu'on peut bien tousiours en imaginer autant que iay dit en chasque Equation; mais qu'il n'y a quelquefois aucune quantité, qui corresponde a celles qu'on imagine.*" (Descartes, 1637, 380); English translation from (Smith and Latham, 1954, 175).

polynomial equations were complex, i.e. of the form $a + b\sqrt{-1}$ for real a, b, was raised; and around the time of C. F. GAUSS (1777–1855), the theorem acquired the name of the *Fundamental Theorem of Algebra*.

When G. W. LEIBNIZ (1646–1716) doubted that the polynomial $x^4 + c^4$ could be split into two real factors of the second degree,[5] the validity of the result seemed for a moment in doubt. L. EULER (1707–1783) demonstrated in 1749 (published 1751) that the set of complex numbers was closed under all algebraic and numerous transcendental operations.[6] Thus, at least by 1751 it would implicitly be known that $\sqrt{i} = \frac{1+i}{\sqrt{2}}$. This made LEIBNIZ'S supposed counter-example evaporate, since he factorized his polynomial as

$$\begin{aligned} x^4 + c^4 &= \left(x^2 - ic^2\right)\left(x^2 + ic^2\right) \\ &= \left(x - \sqrt{i}c\right)\left(x + \sqrt{i}c\right)\left(x + \sqrt{-i}c\right)\left(x - \sqrt{-i}c\right). \end{aligned}$$

Numerous prominent mathematicians of the eighteenth century — among them notably J. LE R. D'ALEMBERT (1717–1783), EULER, and J. L. LAGRANGE (1736–1813) — sought to provide proofs that any real polynomial could be split into linear and quadratic factors which would prove that any imagined roots were indeed complex. In the half-century 1799–1849 GAUSS gave a total of four proofs[7] which, although belonging to an emerging trend of indirect existence proofs, were considered to be superior in rigour when compared to those of his predecessors.

Characterizing roots. The proofs of the *Fundamental Theorem of Algebra* were mostly existence proofs which did not provide any information on the computational aspect. Other similar, nonconstructive results were also pursued. An important subfield of the theory of equations was developed in order to characterize and describe properties of the roots of a given equation from *a priori* inspections of the equation and without explicitly knowing the roots.

LAGRANGE'S study of the properties of the roots of particular equations was an offspring from his attempts to solve higher degree equations through algebraic expressions (see below).[8] LAGRANGE'S interest in numerical equations, i.e. concrete equations in which some dependencies among the coefficients can exist, can be divided into three topics: the nature and number of the roots, limits for the values of these roots, and methods for approximating these. LAGRANGE made use of analytic geometry, function theory, and the Lagrangian calculus in order to investigate these topics.[9]

5 (K. Andersen, 1999, 69).
6 (ibid., 70).
7 GAUSS' proofs can be found in (C. F. Gauss, 1863–1933, vol. 3) and have been collected in German translation in (C. F. Gauss, 1890).
8 (Hamburg, 1976, 28).
9 (ibid., 29–30).

Elementary symmetric relations. A different example of *a priori* properties of the roots of an equation was conceived of by men as G. CARDANO (1501–1576), F. VIÈTE (1540–1603), A. GIRARD (1595–1632), and I. NEWTON (1642–1727) in the sixteenth and seventeenth centuries. From inspection of equations of low degree they obtained (generally by analogy and without general proofs) by a tacit theorem on the factorization of polynomials the dependency of the coefficients of the equation

$$x^n + a_{n-1}x^{n-1} + a_{n-2}x^{n-2} + \cdots + a_1 x + a_0 = 0$$

on the roots $x_1, \ldots, x_n$ given by

$$\begin{aligned} a_{n-1} &= -\left(x_1 + \cdots + x_n\right) \\ a_{n-2} &= x_1x_2 + \cdots + x_{n-1}x_n \\ &\vdots \\ a_1 &= \pm\left(x_1x_2\ldots x_{n-1} + \cdots + x_2x_3\ldots x_n\right) \\ a_0 &= \mp x_1x_2\ldots x_n. \end{aligned} \tag{5.1}$$

These equations established the *elementary symmetric relations* between the roots and the coefficients of an equation. When proofs of these relations first emerged, they were obtained through formal manipulations of the tacitly introduced factors and were, thus, firmly within the established algebraic style.

The relations (5.1) were to become a central tool in the theory of equations once NEWTON and E. WARING (∼1736–1798) realized that they were the basic, or elementary, ones upon which all other symmetric functions of the roots depended rationally.[10]

5.1 Algebraic solubility before LAGRANGE

Among the multitude of possible questions concerning the unknown roots, one is particularly linked to the question of solving equations algebraically. It arose when mathematicians began investigating the form in which the roots can be written and is thus a first step in the direction of asking general solubility questions.[11]

The general approach taken in solving equations of degrees 2, 3 or 4 had since the first attempts been to reduce their solution to the solution of equations of lower degree. The example of the third degree equation solved by S. FERRO (1465–1526) around 1515, by N. TARTAGLIA (1499/1500–1557) in 1539, and by CARDANO, who published the solution in 1545, might be illustrative[12]. Although CARDANO'S arguments and style were geometric, its algebraic content is presented in algebraic notation in box 1.

10 See section 5.2.4.

11 This aspect shall be dealt with below (see page 62ff) and section 8.4.

12 In the present form, revised to expose central concepts, CARDANO'S solution closely resembles the young school-boy's notes found in the section *Ligninger af tredje Grads Opløsning (af Cardan)* in ABEL'S notebook (Abel, MS:829, 139–141).

The algebraic reduction of the cubic equation When the general third degree equation

$$x^3 + ax^2 + bx + c = 0$$

was subjected to the transformation

$$x \longmapsto y - \frac{a}{3}$$

it took the canonical form (in which the term of the second highest degree did not appear)

$$y^3 + ny + p = 0. \tag{5.2}$$

Letting $y = u + v$, CARDANO obtained

$$\begin{aligned} 0 &= (u+v)^3 + n\,(u+v) + p \\ &= u^3 + v^3 + (3uv + n)\,(u+v) + p, \end{aligned}$$

and the equation could be satisfied if

$$\begin{cases} u^3 + v^3 + p = 0, \text{ and} \\ \quad 3uv + n = 0. \end{cases}$$

This system of equations could easily be reduced to the quadratic system (by letting $U = u^3$, $V = v^3$)

$$\begin{cases} U + V = -p \\ 27UV = -n^3 \end{cases}$$

or

$$U^2 + Up - \frac{n^3}{27} = 0, \tag{5.3}$$

the solution of which was well known. Thus, U and V could be found, and finding u, v was only a matter of extracting 3rd roots

$$u = \sqrt[3]{U} \text{ and } v = \sqrt[3]{V},$$

giving one of the roots y of (5.2) as

$$y = u + v = \sqrt[3]{U} + \sqrt[3]{V}.$$

□

Box 1: The algebraic reduction of the cubic equation

Purely formal methods were used in reducing the problem of the third degree equation to one of solving an equation of lower degree, here (5.3). A similar approach was adopted by L. FERRARI (1522–1565) in 1545 and by R. BOMBELLI (1526–1572) between 1557 and 1560 to solve the general fourth degree equation. By the seventeenth century, the search for reductions of the general fifth degree equation into equations of lower degrees was establishing itself as a prestigious mathematical problem. LEIBNIZ and E. W. TSCHIRNHAUS (1651–1708) worked on the problem. In 1683 TSCHIRNHAUS published a procedure which, if applied to the general fifth degree equation, would reduce it to a binomial one with the help of a polynomial equation of degree 4. However, as LEIBNIZ soon demonstrated, determining the coefficients of that polynomial unavoidably involved solving an equation of degree 24 which rendered TSCHIRNHAUS'S reduction useless for solving the fifth degree equation algebraically.[13] Another independent and unsuccessful attempt at reducing the fifth degree equation was made by J. GREGORY (1638–1675), whose proposed reduction was based on a sixth degree auxiliary (resolvent) equation.[14]

The procedure of reduction to lower degree equations — so naturally suggested by incomplete induction from low degree equations — thus failed to give results for higher degree equations. The search had largely been conducted in an empirical way by proposing different reducing functions. It was wanting of a general and theoretical investigation; this was initiated around 1770.

The search for resolvent equations conducted throughout the sixteenth, seventeenth, and eighteenth centuries is properly seen as the quest to *find* algebraic solutions for all polynomial equations, thereby explicitly and constructively demonstrating their algebraic solubility. A polynomial equation of degree n such as

$$x^n + a_{n-1}x^{n-1} + a_{n-2}x^{n-2} + \cdots + a_1 x + a_0 = 0$$

is said to be algebraically solvable if its roots $x_1, \ldots, x_n$ can all be expressed by *algebraic expressions* in the coefficients $a_0, \ldots, a_{n-1}$ — the roots must be expressible as finite combinations of the coefficients and constants using the five algebraic operations addition, subtraction, multiplication, division, and root extraction.

From the second half of the eighteenth century, the diverse and largely empirical attempts to provide concrete reductions was superseded by theoretical and general investigations, mainly by LAGRANGE 1770–1771. In the work of LAGRANGE, the inclination towards general investigations was accompanied by the idea of studying permutations.[15] Both parts were essential in finally establishing that the long sought-for algebraic solution of the quintic equation was impossible.

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13 (Kracht and Kreyszig, 1990, 27–28) and (Kline, 1990, 599–600).

14 (Whiteside, 1972, 528).

15 For LAGRANGE'S focus on the *general*, see (Grabiner, 1981a, 317) and (Grabiner, 1981b, 39).

LEONHARD EULER. In his paper (L. Euler, 1732b), read to the St. Petersburg Academy and published in 1738, EULER gave his solutions to the equations of degree 2, 3, and 4 and demonstrated that they could all be written in the form[16]

$$\begin{aligned} \sqrt{A} &\text{ for the second degree equation,} \\ \sqrt[3]{A}+\sqrt[3]{B} &\text{ for the third degree equation, and} \\ \sqrt[4]{A}+\sqrt[4]{B}+\sqrt[4]{C} &\text{ for the fourth degree equation,} \end{aligned} \tag{5.4}$$

where the quantities A, B, C were roots in certain *resolvent equations* of lower degree which could be obtained from the original equation.[17] EULER appears to have been the first to introduce the term "resolvent" and to attribute to it the central position it was to take in the future research on the solubility of equations.[18]

Extending these results, EULER conjectured that the resolvents also existed for the general equation of the fifth degree — and more generally for any higher degree equation — and that the roots could be expressed in analogy with (5.4).[19]

> "Although this emphasizes the three particular cases [of equations of degrees 2, 3, and 4], I, nevertheless, think that one could possibly, not without reason, conclude that also higher equations would possess similar solving equations. From the proposed equation
>
> $$x^5 = ax^3 + bx^2 + cx + d,$$
>
> I expect to obtain an equation of the fourth degree
>
> $$z^4 = \alpha z^3 - \beta z^2 + \gamma z - \delta$$
>
> the roots of which will be A, B, C, and D,
>
> $$x = \sqrt[5]{A} + \sqrt[5]{B} + \sqrt[5]{C} + \sqrt[5]{D}.$$
>
> In the general equation
>
> $$x^n = ax^{n-2} + bx^{n-3} + cx^{n-4} + \text{etc.}$$
>
> the resolvent equation will, I suspect, be of the form
>
> $$z^{n-1} = \alpha z^{n-2} - \beta z^{n-3} + \gamma z^{n-4} - \text{etc.},$$
>
> whose $n-1$ known roots will be A, B, C, D, etc.,
>
> $$z = \sqrt[n]{A} + \sqrt[n]{B} + \sqrt[n]{C} + \sqrt[n]{D} + \text{etc.}$$
>
> If this conjecture is valid and if the resolvent equations, which can obviously be said to have assignable roots, can be determined, I can obtain equations of lower degrees, and in continuing this process produce the true root of the equation."[20]

16 (ibid., 7).
17 The resolvent equation in the example of the third degree equation is (5.3).
18 (F. Rudio, 1921, ix, footnote 2).
19 According to (Eneström, 1912–1913, 346) already LEIBNIZ seemed conviced that the root of the general equation of the 5th degree could be written in the form

$$x = \sqrt[5]{A} + \sqrt[5]{B} + \sqrt[5]{C} + \sqrt[5]{D}.$$

The quotation illustrates how EULER'S conjecture amounted to the algebraic solubility of all polynomial equations. Returning to the problem, EULER sought to provide further evidence for his conjecture.[21]

EULER was led to a related problem concerning the multiplicity of values of radicals. By calculating the number of values of the multi-valued function consisting of $n-1$ radicals

$$\sqrt[n]{A}+\sqrt[n]{B}+\sqrt[n]{C}+\sqrt[n]{D}+\dots,$$

EULER found that the function had n^{n-1} essentially different values, which apparently contradicted the fact that the equation of degree n should only have n roots. In a paper written in 1759, EULER refined his hypothesis of 1732 and conjectured that the roots of the resolvent A, B, C, D were dependent. EULER'S new conjecture was that the root would be expressible in the form

$$x=\omega+\mathfrak{A}\sqrt[n]{v}+\mathfrak{B}\sqrt[n]{v^2}+\mathfrak{C}\sqrt[n]{v^3}+\dots+\mathfrak{D}\sqrt[n]{v^{n-1}},$$

where the coefficients $\omega, \mathfrak{A}, \mathfrak{B}, \mathfrak{C}, \dots, \mathfrak{D}$ were rational functions of the coefficients, and the $n-1$ other roots would be obtained by attributing to $\sqrt[n]{v}$ the $n-1$ other values $\mathfrak{a}\sqrt[n]{v}, \mathfrak{b}\sqrt[n]{v}, \mathfrak{c}\sqrt[n]{v}\dots$ where $\mathfrak{a}, \mathfrak{b}, \mathfrak{c}$ were the different n^{th} roots of unity.[22] As will be illustrated in chapter 7.1.2, N. H. ABEL (1802–1829) used a similar kind of argument.

20 "*8. Ex his etiamsi tribus tantum casibus tamen non sine sufficienti ratione mihi concludere videor superiorum quoque aequationum dari huiusmodi aequationes resolventes. Sic proposita aequatione*

$$x^5=ax^3+bx^2+cx+d$$

coniicio dari aequationem ordinis quarti

$$z^4=\alpha z^3-\beta z^2+\gamma z-\delta,$$

cuius radices si sint A, B, C et D, fore

$$x=\sqrt[5]{A}+\sqrt[5]{B}+\sqrt[5]{C}+\sqrt[5]{D}.$$

Et generatim aequationis

$$x^n=ax^{n-2}+bx^{n-3}+cx^{n-4}+\text{etc.}$$

aequatio resolvens, prout suspicor, erit huius formae

$$z^{n-1}=\alpha z^{n-2}-\beta z^{n-3}+\gamma z^{n-4}-\text{etc.}$$

cuius cognitis radicibus omnibus numero $n-1$, quae sint A, B, C, D etc., erit

$$x=\sqrt[n]{A}+\sqrt[n]{B}+\sqrt[n]{C}+\sqrt[n]{D}+\text{etc.}$$

Haec igitur coniectura si esset veritati consentanea atque si aequationes resolventes possent determinari, cuiusque aequationis in promtu foret radices assignare; perpetuo enim pervenitur ad aequationem ordine inferiorem hocque modo progrediendo tandem vera aequationis propositae radix innotescet." (L. Euler, 1732b, 7–8); for a German translation, see (L. Euler, 1788–1791, vol. 3, 9–10).

21 (F. Rudio, 1921, ix–x).

22 (ibid., x–xi).

ALEXANDRE-THÉOPHILE VANDERMONDE. Another very important component of the theory of equations in the early nineteenth century was the turn towards focusing on the expressive powers of algebraic expressions. This approach can be traced back to VANDERMONDE who in 1770 presented the *Académie des Sciences* in Paris with a treatise entitled *Mémoire sur la résolution des équations*.[23] There, he described the purpose of his investigations:

> "One seeks the most simple general values which can conjointly satisfy an equation of a certain degree."[24]

As H. WUSSING has remarked, this weakly formulated program only gained importance through VANDERMONDE'S use of examples from low degree equations.[25] VANDERMONDE'S aim was to build algebraic functions from the elementary symmetric ones which could assume the value of *any* root of the given equation. His approach was very direct, constructive, and computationally based. For example the elementary symmetric functions in the case of the general second degree equation

$$(x - x_1)(x - x_2) = x^2 - (x_1 + x_2)x + x_1x_2 = 0$$

are x_1x_2 and $x_1 + x_2$. The well known solution of the quadratic is

$$\frac{1}{2}\left(x_1 + x_2 + \sqrt{(x_1 + x_2)^2 - 4x_1x_2}\right),$$

which gives the two roots x_1 and x_2 when the square root is considered to be a two-valued function. Similarly, although with greater computational difficulties, VANDERMONDE treated equations of degree 3 or 4. In those cases, he also constructed algebraic expressions having the desired properties. When he attacked equations of degree 5, however, he ended up with having to solve a resolvent equation of degree 6. Similarly, his approach led from a sixth degree equation to resolvent equations of degrees 10 and 15. Having seen the apparent unfruitfulness of the approach, VANDERMONDE abandoned it. Later, the idea of studying the algebraic expressions formed from the elementary symmetric functions became central to ABEL'S research.

Both EULER'S and VANDERMONDE'S approaches are, in spite of their apparently unsuccessful outcome, interesting in interpreting ABEL'S work on the theory of equations. Firstly, ABEL'S proof of the impossibility of solving the general quintic by radicals (see chapter 6) is a fusion of ideas advanced by LAGRANGE and VANDERMONDE, although there is no evidence that ABEL was familiar with VANDERMONDE'S work. Secondly, ABEL'S attempted general theory of algebraic solubility (see chapter 8) bears resemblances to paths followed by EULER, VANDERMONDE and LAGRANGE.

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23 (Vandermonde, 1771). This paragraph on VANDERMONDE is largely based on (Wussing, 1969, 52–53).

24 "*On demande les valeurs générales les plus simples qui puissent satisfaire concurremment à une Équation [sic] d'un degré déterminé.*" (Vandermonde, 1771, 366).

25 (Wussing, 1969, 53)

Figure 5.1: JOSEPH LOUIS LAGRANGE (1736–1813)

In section 8.4, I demonstrate how ABEL rigorized the assumptions of EULER'S conjecture and provided the conjecture with a proof. Before going into ABEL'S impossibility proof, it is necessary to present important results obtained by ABEL'S predecessors (including LAGRANGE) of which he made use, and demonstrate the change in approach and belief that made ABEL'S demonstration possible.

5.2 LAGRANGE's theory of equations

Nobody influenced ABEL'S work on the theory of equations more than LAGRANGE. The present section briefly outlines the parts of LAGRANGE'S large and very influential treatise *Réflexions sur la résolution algébrique des équations* which were of particular importance to ABEL'S work.[26] LAGRANGE'S work is well studied and has often — and rightfully so, I think — been seen as one of the first major steps towards linking the theory of equations to group theory.[27] However, with the focus mainly on ABEL'S approach, emphasis is given only to points of direct relevance for this.

When LAGRANGE in 1770–1771 had his *Réflexions sur la résolution algébrique des équations* published in the *Mémoires* of the *Berlin Academy*, he was a well established mathematician held in high esteem. The *Réflexions* was a thorough summary of the

26 (Lagrange, 1770–1771).

27 See for instance (Wussing, 1969, 49–52, 54–56), (Kiernan, 1971), (Hamburg, 1976), or (Scholz, 1990, 365–372).

nature of solutions to algebraic equations which had been uncovered until then. Like EULER and VANDERMONDE had done, LAGRANGE investigated the known solutions of equations of low degrees hoping to discover a pattern feasible to generalizations to higher degree equations. Where EULER had sought to extend a particular algebraic form of the roots, and VANDERMONDE had tried to generalize the algebraic functions of the elementary symmetric functions, LAGRANGE'S innovation was to study the number of values which functions of the coefficients could obtain under permutations of the roots of the equation. Although he exclusively studied the *values* of the functions under permutations, his results marked a first step in the emerging independent theory of permutations. In turn, this permutation theory was soon, through its central role in E. GALOIS'S (1811–1832) theory of algebraic solubility, incorporated in an abstract theory of groups which grew out of progress made in the nineteenth and twentieth centuries.[28]

The work *Réflexions sur la résolution algébrique des équations* was divided into four parts reflecting the structure of LAGRANGE'S investigation.

1. "On the solution of equations of the third degree" (Lagrange, 1770–1771, 207–254)

2. "On the solution of equations of the fourth degree" (*ibid*. 254–304)

3. "On the solution of equations of the fifth and higher degrees" (*ibid*. 305–355)

4. "Conclusion of the preceding reflections with some general remarks concerning the transformation of equations and their reduction to a lower degree" (*ibid*. 355–421)

Of these the latter part is of particular interest to the following discussion. Its aim was to provide a link between the number of values a function could obtain under permutations and the degree of the associated resolvent equation. Most accounts of LAGRANGE'S contribution in the theory of equations emphasize the 100$^{\text{th}}$ section dealing with the rational dependence of *semblables fonctions*, a topic which became central after the introduction of GALOIS theory.[29]

5.2.1 Formal values of functions

Central to LAGRANGE'S treatment of the general equations of all degrees were his concepts of formal functional equality and formal appearance of expressions.[30] LAGRANGE considered two rational functions (formally) equal only when they were given by the same algebraic formulae, in which xy and yx were considered equal

28 (Wussing, 1969).
29 For instance (J. Pierpont, 1898, 333–335) and (Scholz, 1990, 370).
30 (Kiernan, 1971, 46).

because both multiplication (and addition) were implicitly assumed to be commutative, associative, and distributive. This concept of formal equality was intertwined with LAGRANGE'S focus on the formal appearance of expressions which made the *form* and not the *value* the important aspect of expressions. Denoting the roots of the general μ^{th} degree equation by $x_1, \ldots, x_\mu$, LAGRANGE considered the variables (roots) to be *independent* symbols. For example, in LAGRANGE'S view, the two expressions $x_1 - x_2$ and $x_2 - x_1$ were *always* (formally) different, although particular *values* could be given to x_1 and x_2 such that the values of the two expressions were equal. The independence of the symbols $x_1, \ldots, x_\mu$ reflected the fact that in a *general* equation, the coefficients were considered independent; to treat *special*, e.g. numerical equations, a modified approach had to be adapted.

LAGRANGE'S formal approach reflects a general eighteenth century conception of polynomials not as functional mappings but as expressions combined of various symbols: variables and constants, either known or unknown. LAGRANGE was not particularly explicit about this notion of formal equality which occurs throughout his investigations; however, he emphasized that

> "it is only a matter of the form of these values and not their absolute [numerical] quantities."[31]

The focus on formal values was lifted when GALOIS saw that in order to address *special* equations in which the coefficients were not completely general — some or all of them might be restricted to certain numerical values — he had to consider the numerical equality of the symbols in place of LAGRANGE'S formal equality.

5.2.2 The emergence of permutation theory

An important part of LAGRANGE'S approach was the introduction of symbols denoting the roots which enabled him to treat them as if they had been known.[32] This allowed him to focus his attention on the action of permutations on formal expressions in the roots. LAGRANGE set up a system of notation in which

$$f\left[(x')\,(x'')\,(x''')\right]$$

meant that the function f was (formally) altered by any (non-identity) permutation of x', x'', x'''.[33] For instance, the expression $x' + \alpha x'' + \alpha^2 x'''$ would be altered by any non-identity permutation if α was an independent symbol (or a number, say, $\alpha = 2$). If the function remained unaltered when x' and x'' were interchanged, LAGRANGE wrote it as

$$f\left[(x', x'')\,(x''')\right].$$

31 *"il s'agit ici uniquement de la forme de ces valeurs et non de leur quantité absolute."* (Lagrange, 1770–1771, 385).

32 (Kiernan, 1971, 45). Reminiscences of this can also be found with EULER.

33 (Lagrange, 1770–1771, 358).

For example, $x'x'' + x'''$ would remain unaltered by interchanging x' and x'', but any permutation involving x''' would alter it. Finally, if the function was symmetric (i.e. formally invariant under all permutations of x', x'', x'''), he wrote

$$f\left[\left(x', x'', x'''\right)\right].$$

The most important examples of such functions were the elementary symmetric functions,

$$x' + x'' + x''',$$
$$x'x'' + x'x''' + x''x''', \text{ and}$$
$$x'x''x'''.$$

With this notation and his concept of formal equality, LAGRANGE derived far-reaching results on the number of (formally) different values which rational functions could assume under all permutations of the roots. With the hindsight that the set of permutations form an example of an abstract group, a permutation group, one may see that LAGRANGE was certainly involved in the early evolution of permutation group theory. As we shall see in the following section, he was led by this approach to *Lagrange's Theorem*, which in modern terminology expresses that the order of a subgroup divides the order of the group. However, since LAGRANGE dealt with the actions of permutations on rational functions, he was conceptually still quite far from the concept of groups. LAGRANGE'S contribution to the later field of group theory laid in providing the link between the theory of equations and permutations which in turn led to the study of permutation groups from which (in conjunction with other sources) the abstract group concept was distilled.[34] More importantly, LAGRANGE'S idea of *introducing* permutations into the theory of equations provided subsequent generations with a powerful tool.

5.2.3 LAGRANGE's resolvents

Another result found by LAGRANGE, of which ABEL later made eminent and frequent use in his investigations, concerned the polynomial having as its roots all the different values which a given function took when its arguments were permuted. Starting with the case of the quadratic equation having as roots x and y

$$z^2 + mz + n = 0, \tag{5.5}$$

LAGRANGE studied the values $f\left[(x)\,(y)\right]$ and $f\left[(y)\,(x)\right]$ which were all the values a rational function f could obtain under permutations of x and y. He then demonstrated that the equation in t

$$\Theta = \left[t - f\left[(x)\,(y)\right]\right] \times \left[t - f\left[(y)\,(x)\right]\right] = 0$$

34 (Wussing, 1969).

had coefficients which depended rationally on the coefficients m and n of the original quadratic (5.5).[35] This may not be so surprising because today this is easily realized by observing that the coefficients are symmetric in $f[(x)(y)]$ and $f[(y)(x)]$. However, this was precisely the result, which LAGRANGE was about to prove.

Subsequently, LAGRANGE carried out the rather lengthy argument for the general cubic. Thereby, he proved that the equation which had the six values of f under all permutations of the three roots of the cubic as its roots would be rationally expressible in the coefficients of the cubic.

Based on these illustrative cases of equations of low (second and third) degrees, LAGRANGE could state the following two results as a general theorem generalizing the argument sketched for the quadratic above.[36] In the general case, the degree of the equation was denoted μ, and the polynomial having all the values which the given function f assumes under permutations of the μ roots was denoted Θ and its degree ϖ. LAGRANGE then stated:

1. The degree ϖ of Θ divides $\mu!$ where μ is the degree of the proposed equation, and

2. The coefficients of the equation $\Theta = 0$ depend rationally on the coefficients of the original equation.

In his proof of this general theorem, LAGRANGE'S notation and machinery restricted his argument slightly. Because he worked with permutations acting on functions and had no way of clarifying the underlying sets of permutations, his arguments — which contain all the necessary ideas — may seem to rely on analogies with the cases of low degrees.[37] Be that as it may, by any contemporary standards, LAGRANGE'S argument must have been a convincing proof and LAGRANGE'S general theorem became an immensely important tool in the investigations of future algebraists.

> "From this it is clear that the number of different functions [i.e. different values obtained by permuting the arguments] must increase following the products of natural numbers
>
> $$1, \quad 1.2, \quad 1.2.3, \quad 1.2.3.4, \quad \ldots, \quad 1.2.3.4.5\ldots\mu.$$
>
> Having all these functions one will have the roots of the equation $\Theta = 0$; thus, if it is represented as
>
> $$\Theta = t^{\varpi} - Mt^{\varpi-1} + Nt^{\varpi-2} - Pt^{\varpi-3} + \cdots = 0,$$

35 (Lagrange, 1770–1771, 361).

36 (ibid., 369–370).

37 Since LAGRANGE'S proof can easily be adapted to newer frameworks of proof, this interpretation may be a matter of personal taste. However, I do see a major difference between LAGRANGE'S proof by analogy and pattern and the proof later given by CAUCHY (see below).

> one will have $\varpi = 1.2.3.4\dots\mu$ and the coefficient M will equal the sum of all the obtained functions, the coefficient N will equal the sum of all products of these functions multiplied two by two, the coefficient P will equal the sum of all products of the functions multiplied three by three, and so on. [...]
>
> And since we have demonstrated above that the expression Θ must necessarily be a rational function of t and the coefficients $m, n, p, \dots$ of the proposed equation, it follows that the quantities $M, N, P, \dots$ are necessarily rational functions of $m, n, p, \dots$ which one can find directly as we have seen done in the preceding sections."[38]

Expressed in modern mathematical language, the first part of the above result is the equivalent of the *Lagrange's Theorem* of group theory, which states that the order of any subgroup divides the order of the group. As we shall see in section 5.6, the first *general proof* was given by A.-L. CAUCHY (1789–1857) based on his approach to working with permutations.

The second part of the result was used extensively by ABEL, although he never gave references when applying it. ABEL used the result in a form equivalent to the following theorem, formulated in a compact notation.

Theorem 1 *If $\phi\left(x_1,\dots,x_\mu\right)$ is a rational function which takes on the values $\phi_1,\dots,\phi_\varpi$ under all permutations of its arguments $x_1,\dots,x_\mu$ and the equation*

$$\Theta = \prod_{k=1}^{\varpi}(v-\phi_k) = \sum_{k=0}^{\varpi} A_k v^k \tag{5.6}$$

is formed, then all the coefficients $A_0,\dots,A_\varpi$ are symmetric functions of $x_1,\dots,x_\mu$. □

The link between the above theorem as used by ABEL and LAGRANGE'S second result can be obtained through a result which I denote *Waring's formulae*. These formulae, obtained by NEWTON and WARING by different routes and described in the next section, were incorporated by LAGRANGE in his work and must have been accepted as common knowledge in LAGRANGE'S era. As quoted above, LAGRANGE'S

38 "*D'où l'on voit clairement que le nombre des fonctions différentes doit croître suivant les produits des nombres naturels*

$$1,\quad 1.2,\quad 1.2.3,\quad 1.2.3.4,\quad \dots,\quad 1.2.3.4.5\dots\mu.$$

Ayant toutes ces fonctions on aura donc les racines de l'équation $\Theta = 0$; de sorte que, si on la représente par

$$\Theta = t^{\varpi} - Mt^{\varpi-1} + Nt^{\varpi-2} - Pt^{\varpi-3} + \dots = 0,$$

on aura $\varpi = 1.2.3.4\dots\mu$; et le coefficient M sera égal à la somme de toutes les fonctions trouvées, le coefficient N égal à la somme de tous les produits de ces fonctions multipliées deux à deux, le coefficient P égal à la somme de tous les produits des mêmes fonctions multipliées trois à trois, et ainsi de suite. [...]
Et comme nous avons démontré ci-dessus que l'expression de Θ doit être nécessairement une fonction rationnelle de t et des coefficients $m, n, p, \dots$ de l'équation proposée, il s'ensuit que les quantités $M, N, P, \dots$ seront nécessairement des fonctions ratinnelles de $m, n, p, \dots$ qu'on pourra trouver directement, comme nous l'avons pratiqué dans les Sections précédentes." (Lagrange, 1770–1771, 369).

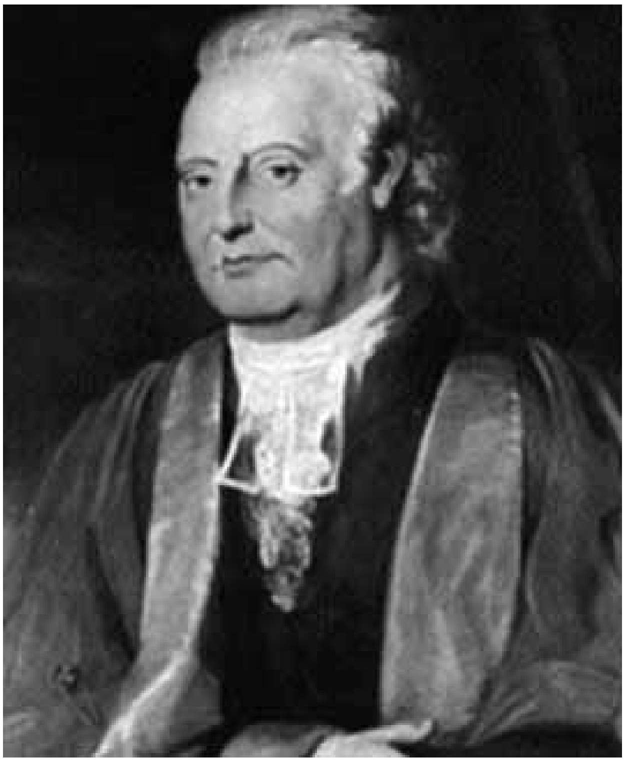

Figure 5.2: EDWARD WARING (1734–1798)

theorem stated that the coefficients, here $A_0, \ldots, A_{\bar{\omega}}$, were rational functions of the coefficients of the given equation. By *Waring's formulae*, any such rational function of the coefficients was a symmetric function of the roots.

5.2.4 *Waring's formulae*

The elementary symmetric functions of the roots of an equation, which since the times of VIÈTE and NEWTON had been known to agree with the coefficients (see section 5), was seen by the little known British mathematician WARING to provide a basis for the study of all symmetric functions of the equation's roots. In his *Miscellanea analytica* of 1762, WARING demonstrated that all rational symmetric functions of the roots could be expressed rationally in the elementary symmetric functions.[39] In his other more influential work *Meditationes algebraicae*,[40] to which LAGRANGE referred,[41] the result was contained in the first chapter. There, WARING dealt with the determination of the power sums of the roots $x_1, \ldots, x_\mu$ (modern notation)

$$\sum_{k=1}^{\mu} x_k^m \text{ for integer } m$$

39 (Waerden, 1985, 76–77).

40 (Waring, 1770).

41 (Lagrange, 1770–1771, 369–370).

from the coefficients of the equation.[42] The solution was the so-called *Waring's Formulae* giving a procedure alternative to one given earlier by NEWTON. From this, WARING proceeded to show how any function of the roots of the form (modern notation writing Σ_μ for the symmetric group)

$$\sum_{\sigma \in \Sigma_\mu} x_{\sigma(1)}^{a_1} x_{\sigma(2)}^{a_2} \cdots x_{\sigma(\mu)}^{a_\mu} \text{ with } a_1, \ldots, a_\mu \text{ non-negative integers} \tag{5.7}$$

could be expressed as an integral function of the power sums of the roots.[43] Thus, WARING had demonstrated that all rational and symmetric functions of $x_1, \ldots, x_\mu$ depended rationally on the power sums and thus on the coefficients of the equation by the preceding result.[44]

Although this important theorem was stated and proved by WARING, it entered the mathematical toolbox of the early nineteenth century mainly through LAGRANGE'S adaption of it in his *Réflexions* (which is the reason for treating it at this place). While WARING'S notation and letter-manipulating approach had hampered his presentation, LAGRANGE dealt with it in a clear and integrated fashion in the *Réflexions*.[45] There, he observed that if the function f had the form

$$f\left[\left(x', x''\right)\left(x'''\right)\left(x^{\text{iv}}\right)\ldots\right],$$

indicating that x' and x'' appeared symmetrically, the roots of the equation $\Theta = 0$ (5.6) would be equal in pairs, whereby the degree could be reduced to $\frac{\mu!}{2}$. After briefly studying a few other types of functions f, LAGRANGE concluded that if f had the form

$$f\left[\left(x', x'', x''', \ldots, x^{(\mu)}\right)\right],$$

i.e. was a symmetric function of the roots, the degree of the equation $\Theta = 0$ (5.6) could be reduced to one and f would be given rationally in the coefficients of the original equation.

5.3 Solubility of cyclotomic equations

Thirty years after LAGRANGE'S creative studies on known solutions to low degree equations, and in particular properties of rational functions under permutations of their arguments, another great master published a work of profound influence on early nineteenth century mathematics. In Göttingen, GAUSS was located at a physical distance from the emerging centers of mathematical research in Paris and Berlin. By 1801, the Parisian mathematicians had for some time been publishing their results in

42 (Waring, 1770, 1–5).
43 (ibid., 9–18).
44 By formal equality, all terms of the same degree would have to have identical coefficients, and thus any rational symmetric function could be decomposed into functions of the form (5.7).
45 (Lagrange, 1770–1771, 371–372).

Figure 5.3: CARL FRIEDRICH GAUSS (1777–1855)

French — and, within a generation, the German mathematicians would also be writing in their native language, at least for publications intended for A. L. CRELLE'S (1780–1855) *Journal für die reine und angewandte Mathematik*. But GAUSS published his fundamental work *Disquisitiones arithmeticae* as a Latin monograph as was still customary for his generation of German scholars.

The book cosisted of seven sections, although allusions and references were made to an eighth section which GAUSS never completed for publication.[46] The main part was concerned with the theory of congruences, the theory of forms, and related number theoretic investigations. Together, these topics provided a new foundation, emphasis, and disciplinary independence — as well as a wealth of results — for nineteenth century number theorists — in particular G. P. L. DIRICHLET (1805–1859) — to elaborate. In dealing with the classification of forms and describing primitive roots, GAUSS made use of "implicit group theory".[47] Despite the fact that both LAGRANGE and GAUSS worked with particular instances of groups, neither of them introduced an abstract concept of groups.

One of the new tools applied by GAUSS in the theory of congruences was that of *primitive roots*. In the articles 52–57, GAUSS gave his exposition of EULER'S treatment of primitive roots. A primitive root k of modulus μ is an integer $1 < k < \mu$ such that

46 (C. F. Gauss, 1863–1933, vol. 1, 477). It is, however, included among the *Nachlass* in the second volume of the *Werke* (ibid.).

47 (Wussing, 1969, 40–44).

the set of remainders of its powers $k^1, k^2, \dots, k^{\mu-1}$ modulo μ coincides with the set $\{1, 2, \dots, \mu-1\}$, possibly in a different order. A central result obtained was the existence of the $p-1$ different primitive roots $1, 2, \dots, p-1$ of modulus p if p were assumed to be prime.

5.3.1 The division problem for the circle

In the last section of his *Disquisitiones arithmeticae*,[48] GAUSS turned his investigations toward the equations defining the division of the periphery of the circle into equal parts. He was interested in the ruler-and-compass constructibility[49] of regular polygons and was therefore led to study in detail *how*, i.e. by the extraction of which roots, the binomial equations of the form

$$x^n - 1 = 0 \tag{5.8}$$

could be solved algebraically. If the roots of this equation could be constructed by ruler and compass, then so could the regular p-gon. It is evident from GAUSS' mathematical diary that this problem had occupied him from a very early stage in his mathematical career and had been a deciding factor in his choice of mathematics over classical philology.[50] The very first entry in his mathematical progress diary from 1796 read:

> "[1] The principles upon which the division of the circle depend, and geometrical divisibility of the same into seventeen parts, etc. [1796] March 30 Brunswick."[51]

In his introductory remarks, GAUSS noticed that the approach which had led him to the division of the circle could equally well be applied to the division of other transcendental curves of which he gave the lemniscate as an example.

> "The principles of the theory which we are going to explain actually extend much farther than we will indicate. For they can be applied not only to circular functions but just as well to other transcendental functions, e.g. to those which depend on the integral $\int \left[1/\sqrt{\left(1-x^4\right)}\right] dx$ and also to various types of congruences."[52]

48 (C. F. Gauss, 1801). For historical studies, see for instance (Wussing, 1969, 37–44), (Schneider, 1981, 37–50), or (Scholz, 1990, 372–376).

49 Throughout, I refer to *Euclidean construction*, i.e. by ruler and compass when I speak of *constructions* or *constructibility*.

50 (Biermann, 1981, 16).

51 "*[1.] Principia quibus innititur sectio circuli, ac divisibilitas eiusdem geometrica in septemdecim partes etc. [1796] Mart. 30. Brunsv[igae]*" (C. F. Gauss, 1981, 21, 41); English translation from (J. J. Gray, 1984, 106).

52 "*Ceterum principia theoriae, quam exponere aggredimur, multo latius patent, quam hic extenduntur. Namque non solum ad functiones circulares, sed pari successu ad multas functiones transscendentes applicari possunt, e.g. ad eas, quae ab integrali* $\int \frac{dx}{\sqrt{(1-x^4)}}$ *pendent, praetereaque etiam ad varia congruentiarum genera.*" (C. F. Gauss, 1801, 412–413); English translation from (C. F. Gauss, 1986, 407).

However, as he was preparing to write a treatise on these topics, GAUSS chose to leave this extension out of the *Disquisitiones*. GAUSS never wrote the promised treatise, and after ABEL had published his first work on elliptic functions culminating in the division of the lemniscate,[53] GAUSS gave him credit for bringing these results into print.[54]

A first simplification of the study of the constructibility of a regular n-gon was made when GAUSS observed that he needed only to consider cases in which n was a prime since any polygon with a composite number of edges could be constructed from the polygons with the associated prime numbers of edges. Equations expressing the sine, the cosine, and the tangent were well known, but none of those were as suitable for GAUSS' purpose as the equation $x^n - 1 = 0$ of which he knew that the roots were[55]

$$\cos\frac{2k\pi}{n} + i\sin\frac{2k\pi}{n} = 1 \text{ when } 0 \leq k \leq n-1.$$

Inspecting these roots, GAUSS observed that the equation $x^n - 1 = 0$ for odd n had a single real root, $x = 1$, and the remaining imaginary roots were all given by the equation

$$\mathrm{X} = \frac{x^n - 1}{x - 1} = x^{n-1} + x^{n-2} + \cdots + x + 1 = 0, \tag{5.9}$$

the roots of which GAUSS thought of as forming the *complex* Ω. When GAUSS used the term "complex" (Latin: *complexum*) he thought of it as a collection of objects (here roots) without any structure imposed.[56] Initially, GALOIS used the French term *groupe* in a similar (naive) way before it later gradually acquired its status as a mathematical term.[57] This evolution of everyday words into mathematical concepts appears to be a recurring feature of mathematics in the early nineteenth century when so many terms became precisely defined and re-defined.[58] GAUSS demonstrated that if r designated any root in Ω, all roots of (5.8) could be expressed as powers of r, thereby saying that any root in Ω was a primitive n^{th} root of unity.

5.3.2 Irreducibility of the equation $\frac{x^n-1}{x-1} = 0$

An interesting feature of GAUSS' approach was his focusing on the *complex* or *system* of roots instead of the individual roots. This slight shift in the conception of roots enabled GAUSS (as it had enabled LAGRANGE)[59] to study properties of the equations which

53 (N. H. Abel, 1827b).
54 (Crelle→Abel, 1828/05/18. N. H. Abel, 1902a, 62).
55 GAUSS wrote P (periphery) for 2π; however the use of i for $\sqrt{-1}$ is his.
56 Later, in 1831, GAUSS introduced the term *complex numbers* to denote numbers which had hitherto been designated *imaginary*; (Gericke, 1970, 57). I fail to see any connection between the term *complexum* as used here and the later, technical term.
57 (Wussing, 1969, 78).
58 See also section 21.2.
59 In the preface, GAUSS briefly described his debt to the number theoretic investigations of the "modern authors" FERMAT, EULER, LAGRANGE, and LEGENDRE. If GAUSS had read LAGRANGE'S *Réflexions*, he did not refer explicitly to it.

could only be captured in studies of the entire system of roots. To GAUSS, the most important properties were those of *decomposability* and *irreducibility*. GAUSS demonstrated through an *ad hoc* argument that the function X (5.9) could not be decomposed into polynomials of lower degree with rational coefficients. In modern terminology, he proved that the polynomial X was irreducible over $\mathbb{Q}$.

GAUSS' proof assumed that the function

$$X = x^{n-1} + x^{n-2} + \cdots + x + 1$$

was divisible by a function of lower degree

$$P = x^{\lambda} + Ax^{\lambda-1} + Bx^{\lambda-2} + \cdots + Kx + L, \tag{5.10}$$

in which the coefficients $A, B, \ldots, K, L$ were rational numbers. Assuming $X = PQ$, GAUSS introduced the two systems of roots $\mathfrak{P}$ and $\mathfrak{Q}$ of P and Q respectively. From these two systems GAUSS defined another two consisting of the reciprocal roots[60]

$$\hat{\mathfrak{P}} = \left\{ r^{-1} : r \in \mathfrak{P} \right\} \text{ and } \hat{\mathfrak{Q}} = \left\{ r^{-1} : r \in \mathfrak{Q} \right\}.$$

Although GAUSS consistently termed the roots of $\hat{\mathfrak{P}}$ and $\hat{\mathfrak{Q}}$ *reciprocal roots*, it is easy for us to see that they are what we would term *conjugate roots* since any root in $\mathfrak{P}$ has unit length.

GAUSS split the subsequent argument into four different cases. The opening one is the most interesting one, namely the case in which $\mathfrak{P} = \hat{\mathfrak{P}}$, i.e. when all roots of $P = 0$ occur together with their conjugates. It may be surprising that GAUSS considered other cases as we would expect him to know that in any polynomial with real coefficients the imaginary roots occur in conjugate pairs. K. JOHNSEN has argued that this apparently unnecessary complication in GAUSS' argument can be traced back to a more general concept of irreducibility over fields different from $\mathbb{Q}$, for instance the field $\mathbb{Q}(i)$.[61] If so, there are no explicit hints at such a concept in the *Disquisitiones*, and the result which GAUSS proved only served a very specific purpose in his larger argument, and did not give a general concept, general criteria, or a body of theorems concerning irreducibility over $\mathbb{Q}$ or any other field. The proof relies more on number theory (higher arithmetic) than on general theorems and criteria concerning irreducible equations, let alone any general concept of fields distinct from the rational numbers $\mathbb{Q}$.

After observing that P was the product of $\frac{\lambda}{2}$ paired factors of the form

$$(x - \cos\omega)^2 + \sin^2\omega,$$

GAUSS concluded that these factors would assume real and positive values for all real values of x, which would then also apply to the function $P(x)$. He then formed $n-1$

60 The notation $\hat{\mathfrak{P}}$ and $\hat{\mathfrak{Q}}$ for these is mine.
61 (Johnsen, 1984).

auxiliary equations[62]

$$P^{(k)} = 0 \text{ where } 1 \leq k \leq n-1$$

defined by their root systems $\mathfrak{P}^{(k)}$ consisting of k^{th} powers of the roots of $P = 0$,

$$\mathfrak{P}^{(k)} = \left\{ r^k : r \in \mathfrak{P} \right\},$$
$$P^{(k)}(x) = \prod_{s \in \mathfrak{P}^{(k)}} (x - s) = \prod_{r \in \mathfrak{P}} \left(x - r^k \right).$$

Following the introduction of the numbers p_k defined by

$$p_k = P^{(k)}(1) = \prod_{s \in \mathfrak{P}^{(k)}} (1 - r) = \prod_{r \in \mathfrak{P}} \left(1 - r^k \right),$$

GAUSS used properties derived in a previous article to establish

$$\sum_{k=1}^{n-1} p_k = \sum_{k=1}^{n-1} P^{(k)}(1) = nA. \tag{5.11}$$

Furthermore,

$$\prod_{k=1}^{n-1} P^{(k)}(x) = \prod_{k=1}^{n-1} \prod_{r \in \mathfrak{P}} \left(x - r^k \right) = \prod_{r \in \mathfrak{P}} \prod_{k=1}^{n-1} \left(x - r^k \right) = \prod_{r \in \mathfrak{P}} X = X^{\lambda}, \text{ and}$$
$$\prod_{k=1}^{n-1} p_k = \prod_{k=1}^{n-1} P^{(k)}(1) = X^{\lambda}(1) = n^{\lambda} \text{ since } X(1) = n.$$

From the article describing the construction of an equation with the k^{th} powers of the roots of a given equation as its roots, GAUSS knew that the coefficients of $P^{(1)}, \ldots, P^{(n-1)}$ would be rational numbers if the coefficients of P were rationals. Much earlier, in article 42, he had furthermore demonstrated that the product of two polynomials with rational but not integral coefficients could not be a polynomial with integral coefficients. Since X had integral coefficients and P had rational coefficients by assumption, it followed that the coefficients of $P^{(1)}, \ldots, P^{(n-1)}$ would indeed be integers, since any $P^{(k)}$ was a factor of X^{λ} with rational coefficients. Consequently, the quantities p_k would have to be integral, and since their product was n^{λ} and there were $n - 1 > \lambda$ of them, at least $n - 1 - \lambda$ of the quantities p_k would have to be equal to 1 and the others would have to equal n or some power of n since n was assumed to be prime. But if the number of quantities equal to 1 was g it would follow that

$$\sum_{k=1}^{n-1} p_k \equiv g \pmod{n},$$

which GAUSS saw would contradict (5.11) since $0 < g < n$.

62 The notation $P^{(k)}$ and $\mathfrak{P}^{(k)}$ is mine.

The other cases, which in the presently adopted notation can be described as

$$2.\mathfrak{P} \neq \hat{\mathfrak{P}} \text{ and } \mathfrak{P} \cap \hat{\mathfrak{P}} \neq \emptyset,$$
$$3.\mathfrak{Q} \cap \hat{\mathfrak{Q}} \neq \emptyset, \text{ and}$$
$$4.\mathfrak{P} \cap \hat{\mathfrak{P}} = \emptyset \text{ and } \mathfrak{Q} \cap \hat{\mathfrak{Q}} = \emptyset,$$

could all be brought to a contradiction, either directly or by referring to the first case described above.

The fruitfulness of the proof of the irreducibility of X was that it demonstrated that if X was decomposed into factors of lower degrees (such as 5.10) some of these had to have irrational coefficients. Thus any attempt at determining the roots would have to involve equations of degree higher than one. The purpose of the following investigation was to gradually reduce the degree of these equations to minimal values by refining the system of roots.

5.3.3 Outline of GAUSS's proof

Continuing from the result above that any root r in Ω was a primitive n^{th} root of unity, GAUSS wrote $[1], [2], \ldots, [n-1]$ for the associated powers of r. He introduced the concept of periods by defining the *period* (f, λ) to be the set of the roots $[\lambda], [\lambda g], \ldots, \left[\lambda g^{f-1}\right]$, where f was an integer, λ an integer not divisible by n, and g a primitive root of the modulus n. Connected to the period, he introduced the *sum of the period*, which he also designated (f, λ),

$$(f, \lambda) = \sum_{k=0}^{f-1} \left[\lambda g^k\right],$$

and the first result, which he stated concerning these periods, was their independence of the choice of g.

Throughout the following argument, GAUSS let g designate a primitive root of modulus n and constructed a sequence of equations through which the periods $(1, g)$, i.e. the roots in $X = 0$ (5.9), could be determined. Assuming that the number $n-1$ had been decomposed into primes as

$$n - 1 = \prod_{k=1}^{u} p_k,$$

GAUSS partitioned the roots of Ω into $\frac{n-1}{p_1}$ periods, each of p_1 terms. From these, he formed p_1 equations $X' = 0$ having the $\frac{n-1}{p_1}$ sums of the form (p_1, λ) as its roots. By a central theorem proved using symmetric functions,[63] he proved that the coefficients of these latter equations depended upon the solution of yet another equation of degree p_1. Thus the solution of the original equation of degree n had been reduced to solving p_1 equations $X' = 0$ each of degree $\frac{n-1}{p_1}$ and a single equation of degree p_1.

63 (C. F. Gauss, 1801, §350).

By repeating the procedure, GAUSS could solve the equation $X' = 0$ by solving p_2 equations of degree $\frac{n-1}{p_1 p_2}$ and a single equation of degree p_2. Similarly, the procedure could be iterated further until the solution of the equation $X = 0$ of degree $n-1$ had been reduced to solving u equations of degrees $p_1, p_2, \ldots, p_u$ since the other equations would ultimately have degree 1.

A special case emerged if $n-1$ was a power of 2. It was well known that square roots could always be constructed by ruler and compass. Therefore, if n had the form

$$n = 1 + 2^k,$$

the construction of the roots of (5.8) could be carried out by ruler and compass. By applying this to $k = 4$, GAUSS demonstrated that the regular 17-gon could be constructed by ruler and compass giving the first new constructible regular polygon since the time of EUCLID (~295 B.C.) (~295BC).[64]

By the same argument he had devised to consider only prime values of n, GAUSS could also conclude that the construction of the regular n-gon was possible by ruler and compass when n had the form

$$n = 2^m \prod_{k=1}^{h} (1 + 2^{u_k})$$

when $\{u_k\}$ was a set of distinct integers such that $\{1 + 2^{u_k}\}$ were primes, the so-called *Fermat primes*. The converse implication, that only such n-gons were constructible, was claimed without detailed proof by GAUSS:

> "Whenever $n-1$ involves prime factors other than 2, we are always led to equations of higher degree, namely to one or more cubic equations when 3 appears once or several times among the prime factors of $n-1$, to equations of the fifth degree when $n-1$ is divisible by 5, etc. **We can show with all rigor that these higher-degree equations cannot be avoided in any way nor can they be reduced to lower-degree equations**. The limits of the present work exclude this demonstration here, but we issue this warning lest anyone attempt to achieve geometric constructions for sections other than the ones suggested by our theory (e.g. sections into 7, 11, 13, 19, etc. parts) and so spend his time uselessly."[65]

---

64 GAUSS, himself, was very aware of the progress he had made, see (C. F. Gauss, 1986, 458) and (Schneider, 1981, 38–39). As always, the date given for EUCLID is taken from the *Dictionary of Scientific Biography*.

65 "*Quoties autem* $n-1$ *alios factores primos praeter* 2 *implicat, semper ad aequationes altiores deferimur; puta ad unam pluresve cubicas, quando* 3 *semel aut pluries inter factores primos ipsius* $n-1$ *reperitur, ad aequationes quinti gradus, quando* $n-1$ *divisibilis est per* 5 *etc.,* ***omnique rigore demonstrare possumus, has aequationes elevatas nullo modo nec evitari nec ad inferiores reduci posse****, etsi limites huius operis hanc demonstrationem hic tradere non patiantur, quod tamen monendum esse duximus, ne quis adhuc alias sectiones praeter eas, quas theoria nostra suggerit, e.g. sectiones in 7, 11, 13, 19 etc. partes, ad constructiones geometricas perducere speret, tempusque inutiliter terat.*" (C. F. Gauss, 1801, 462); English translation from (C. F. Gauss, 1986, 459). Bold-face has been substituted for the original small-caps.

The class of equations (the cyclotomic ones), which GAUSS had demonstrated had constructible roots, was also interesting from the point of algebraic solubility of equations. In his proof, GAUSS had demonstrated that they were indeed solvable by radicals including only square roots, whereby the first new non-elementary class of solvable equations of high degrees had been established. By the time GAUSS wrote his *Disquisitiones*, he had come to suspect that not all equations were solvable by radicals. A few years later, ABEL could consider this newly found class to be a special example of equations having the nice property of being algebraically solvable.

5.4 Belief in algebraic solubility shaken

In the seventeenth century, the belief in the algebraic solubility (in radicals) of all polynomial equations seems to have been in little dispute. The question of solubility was not an issue when the prominent mathematicians such as TSCHIRNHAUS searched for a general solution. Half-way through the eighteenth century the problem had taken a slight turn when EULER in 1732 proposed to investigate the *hypothesis* that the roots of the general n^{th} degree equation could be written as a sum of $n-1$ root extractions of degree $n-1$. Although he advanced this as a hypothesis and his search for definite proof was in vain, he based his 1749 "proof" of the *Fundamental Theorem of Algebra* on the belief that any polynomial equation could be reduced to *pure equations*.[66] Towards the end of the eighteenth century, the outspoken beliefs of the most prominent mathematicians had changed, though. Mathematicians with a keen interest in the subject started to suspect that the reduction to pure equations was beyond — not only their grasp — but the power of their existing tools. At the turn of the century one of the most influential mathematicians, GAUSS, declared the reduction to be outright impossible.

The belief in the algebraic solubility of general equations did *not* vanish completely with GAUSS' proclamation of its impossibility. In chapter 6.9, where I discuss the reception of ABEL'S work on the theory of equations, I shall also discuss the "inertia" of the mathematical community in this respect.

5.4.1 "Infinite labor"

On the British Isles, WARING had recognized patterns which led him to the known solutions of low degree equations. Based on analogies, he thought that solutions to all equations could be formed but that the amount of involved computations would explode beyond anything practical.

66 By *pure equations* EULER (and with him GAUSS) meant equations describing *explicit* functions, i.e. a pure equation for x is of the form

$$x = \text{some expression.}$$

> "From the preceding examples and earlier observations, we may compose resolutions appropriate to any given equation; but in equations of the fifth and higher degree the calculations require practically infinite labor."[67]

The inner tension in WARING'S statement — that the solution was possible in principle but perhaps not in practice — is confusing. It remains unclear exactly what it meant to him that he *could* construct solutions but that the effort required would be infinite.

In France, LAGRANGE felt strong confidence in his approach to the study of polynomial equations. His detailed studies of low degree equations led him to the conclusion that each root $x_1, \ldots, x_n$ of the general equation of degree n could be expressed through a resolvent equation of degree $n-1$ which had the roots

$$\sum_{k=1}^{n} \omega_j^k x_k \text{ (for } j = 1, \ldots, n-1)$$

where $\omega_1, \ldots, \omega_{n-1}$ were the imaginary n^{th} roots of unity. When LAGRANGE sought to prove this result for the fifth degree equation, however, he had to accept that his effort was inconclusive. If such a reduction was to be possible at all, other resolvents were required.

> "It thus appears that from this one can conclude by induction that every equation, of whatever degree, will also be solvable with the help of a resolvent [equation] whose roots are represented by the same formulae
>
> $$x' + y'' + y^2x''' + y^3x^{\text{iv}} + \ldots.$$
>
> But, as we have demonstrated in the previous section in connection with the methods of MM. Euler and Bezout, these lead directly to the same resolvent equations, there seems to be reason to convince oneself in advance that this conclusion is defective for the fifth degree. From this it follows, that if the algebraic solution of equations of degrees higher than four is not impossible, it must depend on certain functions of the roots, which are different from the preceding ones."[68]

Although his investigations had not led to the goal of generalizing known solutions of low degree equations to a solution to the general fifth degree equation, LAGRANGE was confident that he had presented and founded a true theory — based

67 (Waring, 1770, 162). The Latin original has not been available. Therefore, reference is given to the English translation (ibid.).

68 "*Il semble donc qu'on pourrait conclure de là par induction que toute équation, de quelque degré qu'elle soit, sera aussi résoluble à l'aide d'une réduite dont les racines soient représentées par la même formule*

$$x' + yx'' + y^2x''' + y^3x^{\text{iv}} + \ldots.$$

Mais, d'après ce que nous avons démontré dans la Section précédente à l'occasion des méthodes de MM. Euler et Bezout, lesquelles conduisent directement à de pareilles réduites, on a, ce semble, lieu de se convaincre d'avance que cette conclusion se trouvera en défaut dès le cinquième degré; d'où il s'ensuit que, si la résolution algébrique des équations des degrés supérieurs au quatrième n'est pas impossible, elle doit dépendre de quelques fonctions des racines, différentes de la précédente." (Lagrange, 1770–1771, 356–357).

upon combinations, i.e. permutations — inside which the solution could be investigated. However, for equations of the fifth and higher degrees the required number of calculations and combinations would be exceeding practical possibilities.

> "These are, if I am not mistaken, the true principles of the solution of equations, and the most appropriate analysis leading to it. As one can see, it all comes down to a sort of calculus of combinations, by which one finds *à priori* the results for which one should be prepared. It should, by the way, be applicable to equations of the fifth degree and higher degrees, of which the solution is until now unknown. But this application demands a too great number of researches and combinations, of which the success is still in serious doubt, for us to follow this path in the present work. We hope, though, to be able to follow it at another time, and we content ourselves by having laid the foundations of a theory which appears to us to be new and general."[69]

LAGRANGE never wrote the definitive work which he had reserved the right to do. By the time GALOIS had substantiated LAGRANGE'S claim for generality and applicability of his theory of combinations (see chapter 8.5), LAGRANGE was no longer around to celebrate the ultimate vindication of his research in the field of algebraic solubility.

Both WARING and LAGRANGE believed by 1770 that their theories were the necessary stepping stones towards the study of solutions to general equations. However, they both acknowledged that the amount of work required to apply these theories to the quintic equation was beyond their own limitations. Before the end of the century, even more radical opinions were voiced in print.

5.4.2 Outright impossibility

In the introduction to his first proof (published 1799 but constructed two years earlier) of the *Fundamental Theorem of Algebra*, GAUSS gave detailed discussions and criticisms of previously attempted proofs. In EULER'S attempt dating back to 1749, GAUSS found the implicit assumption that any polynomial equation could be solved by radicals.

> "In a few words: It is without sufficient reason assumed that the solution of any equation can be reduced to the resolution of pure equations. Perhaps it would not be too difficult to prove the impossibility for the fifth degree with all rigor; I will communicate my investigations on this subject on another occasion. At this place, it suffices to emphasize that the general solution of equations, in this sense,

69 *"Voilà, si je me ne trompe, les vrais principes de la résolution des équations et l'analyse la plus propre à y conduire; tout se réduit, comme on voit, à une espèce de calcul des combinaisons, par lequel on trouve* à priori *les résultats auxquels on doit s'attendre. Il serait à propos d'en faire l'application aux équations du cinquième degré et des degrés supérieurs, dont la résolution est jusqu'à présent inconnue; mais cette application demande un trop grand nombre de recherches et de combinaisons, dont le succés est encore d'ailleurs fort douteux, pour que nous puissions quant à présent nous livrer à ce travail; nous espérons cependant pouvoir y revenir dans un autre temps, et nous nous contenterons ici d'avoir posé les fondements d'une théorie qui nous paraît nouvelle et générale."* (Lagrange, 1770–1771, 403).

> remains very doubtful, and consequently that any proof whose entire strength depends on this assumption in the current state of affairs has no weight."[70]

In 1799, GAUSS'S aim was to scrutinize EULER'S proof of the *Fundamental Theorem of Algebra*. For this purpose, it was sufficient for GAUSS to express his suspicion that the algebraic solution of general equations was not established with the necessary rigor. Thus — at least as it stands — GAUSS'S criticism seems to confront the foundations and not the validity of this hidden assumption in EULER'S proof. Although it is doubtful whether or not GAUSS possessed a demonstration that the validity could also be questioned, he certainly suggested the possibility. Two years later in his influential *Disquisitiones* 1801, GAUSS addressed the problem again in connection with the cyclotomic equations (see quotation below). Possibly alluding to LAGRANGE'S "very great computational work" GAUSS described the solution of higher degree equations not merely beyond the existing tools of analysis but outright impossible.

> "The preceding discussion had to do with the *discovery* of auxiliary equations. Now we will explain a very remarkable property concerning their *solution*. Everyone knows that the most eminent geometers have been unsuccessful in the search for a general solution of equations higher than the fourth degree, or (to define the search more accurately) for the **reduction of mixed equations to pure equations**. And there is little doubt that this problem is not merely beyond the powers of contemporary analysis but proposes the impossible (cf. what we said on this subject in *Demonstrationes nova*, art. 9 [above]). Nevertheless it is certain that there are innumerable mixed equations of every degree which admit a reduction to pure equations, and we trust that geometers will find it gratifying if we show that our equations are always of this kind."[71]

While GAUSS was voicing his opinion on the insolubility of higher degree equations in Latin from his position in Göttingen, the support for the solubility of the quintic was shaken even more radically by an Italian. P. RUFFINI (1765–1822) had published his first proof of the impossibility of solving the quintic in 1799, the same year GAUSS had first uttered his doubts about its possibility. But while GAUSS had

70 "*Seu, missis verbis, sine ratione sufficienti supponitur, cuiusvis aequationis solutionem ad solutionem aequationum purarum reduci posse. Forsan non ita difficile foret, impossibilitatem iam pro quinto gradu omni rigore demonstrare, de qua re alio loco disquisitiones meas fusius proponam. Hic sufficit, resolubilitatem generalem aequationum, in illo sensu acceptam, adhuc valde dubiam esse, adeoque demonstrationem, cuius tota vis ab illa suppositione pendet, in praesenti rei statu nihil ponderis habere.*" (C. F. Gauss, 1799, 17–18); for a translation into German, see (C. F. Gauss, 1890, 20–21).

71 "*Disquisitiones praecc. circa* inventionem *aequationum auxiliarium versabantur: iam de earum* solutione *proprietatem magnopere insignem explicabimus. Constat, omnes summorum geometrarum labores, aequationum ordinem quartum superantium resolutionem generalem, sive (ut accuratius quid desideretur definiam)* ***affectarum reductionem ad puras****, inveniendi semper hactenus irritos fuisse, et vix dubium manet, quin hocce problema non tam analyseos hodiernae vires superet, quam potius aliquid impossibile proponat (Cf. quae de hoc argumento annotavimus in* Demonstr. nova etc. arg. 9*). Nihilominus certum est, innumeras aequationes affectas cuiusque gradus dari, quae talem reductionem ad puras admittant, geometrisque gratum fore speramus, si nostras aequationes auxiliares semper huc referendas esse ostenderimus.*" (C. F. Gauss, 1801, 449); English translation from (C. F. Gauss, 1986, 445). Bold-face has been substituted for the original small-caps.

only alluded to a proof without communicating it, RUFFINI had taken the step of publishing his arguments.

5.5 RUFFINI's proofs of the insolubility of the quintic

In the 1820s, the search for algebraic solutions to equations of higher degree was proved to be in vain when ABEL demonstrated the algebraic insolubility of the quintic. However, ABEL was not the first to claim the insolubility; more than 25 years before him, the Italian RUFFINI had published his investigations which led him to the same conclusion and a proof thereof. RUFFINI'S works were not widely known, and during his investigations ABEL was unaware of their existence (see section 6.7). ABEL based his investigations on the analysis by LAGRANGE and works of CAUCHY on the theory of permutations. Although not directly inspired by RUFFINI'S works, RUFFINI played an indirect role in fertilizing the ground for ABEL'S work. The indirect influence of RUFFINI through the very direct influence of CAUCHY on the development leading to ABEL'S work is two-fold. Firstly, these men smoothed the transition from the *beliefs* described in the previous section to the rigorous knowledge of the insolubility of the quintic. Secondly, their investigations took the still young theory of permutations to a more advanced level; and in doing so, they provided an important characterization of the number of values a rational function can obtain under permutations of its arguments.

5.5.1 Insolubility proved

Although GAUSS had proclaimed his belief that the insolubility of the quintic might not be difficult to prove with all rigor, the Italian RUFFINI, in 1799, was the first mathematician to state the insolubility as a result and provide the claim with a proof. RUFFINI'S style of presentation was long, cumbersome, and at times not free of errors; and his initial proof was met with immediate criticism for these reasons. But convinced of the result and his proof, RUFFINI kept elaborating and clarifying his theory in print for the next 20 years, producing a total of five different versions of the proof. The proofs were published in Italian as monographs in Bologna and in the mathematical memoirs of the *Società Italiana delle Scienze*, Modena. Although published and distributed, the impact of RUFFINI'S work was limited; among the few non-Italians to take a viewpoint on RUFFINI'S work was CAUCHY (see section 5.5.3). In 1810, J.-B. J. DELAMBRE (1749–1822) was aware of RUFFINI'S 1802-proof which he described as "difficult" and "not suited for inclusion in works meant as a first introduction" to the subject.[72] I shall mainly deal with RUFFINI'S initial proof given in his textbook,[73] which he elaborated

72 (Delambre, 1810, 86–87).
73 (Ruffini, 1799).

Figure 5.4: PAOLO RUFFINI (1765–1822)

on numerous occasions, and his final proof published 1813.[74]

5.5.2 RUFFINI's first proof

The writings of RUFFINI were deeply inspired by LAGRANGE'S analysis of the solubility of equations described in section 5.2.[75] LAGRANGE'S ideas, concepts, and notation permeate RUFFINI'S works; and on numerous occasions RUFFINI openly acknowledged his debt to LAGRANGE.[76] As LAGRANGE had done, RUFFINI studied equations of low degrees in order to establish patterns subjectable of generalization. Prior to applying his analysis to the fifth degree equation, RUFFINI propounded the corner stone of his investigation. Central to his line of argument was his classification of permutations. Founded in LAGRANGE'S studies of the behavior of functions when their arguments were permuted, RUFFINI set out to classify all such permutations of arguments which left the function (formally) unaltered. RUFFINI'S concept of *permutation* (Italian: "permutazione") differed from the modern one, and can most easily be understood if translated into the modern concept introduced by CAUCHY in the 1840s of

74 (Ruffini, 1813). The presentation of RUFFINI'S proofs will largely rely on secondary sources, primarily (Burkhardt, 1892), (Wussing, 1969, 56–59), and (Kiernan, 1971, 56–60). In (R. G. Ayoub, 1980), his proofs are interpreted using concepts from GALOIS theory.

75 (Lagrange, 1770–1771).

76 See for instance his preliminary discourse in (Ruffini, 1799, 3–4).

simple permutations	powers of a cycle
	powers of a non-cycle
composite permutations	intransitive ones
	transitive, imprimitive ones
	transitive, primitive ones

Table 5.1: RUFFINI's classification of permutations

systems of conjugate substitutions.[77] Thus, a *permutation* for RUFFINI corresponded to a collection of interchangements (i.e. transitions from one arrangement of symbols e.g. $123\ldots n$ to another e.g. $213\ldots n$) which left the given function formally unaltered.[78]

Classification of permutations. RUFFINI divided his permutations into *simple* ones which were generated by iterations (i.e. powers) of a single interchangement[79] and *composite* ones generated by more than one interchangement. His simple permutations consisting of powers of a single interchangement were subdivided into two types distinguishing the case in which the single interchangement consisted of a single cycle from the case in which it was the product of more than one cycle.

RUFFINI'S composite permutations were subsequently subdivided into three types.[80] A permutation (i.e. set of interchangements) was said to be of the first type if two arrangements existed which were not related by an interchangement from the permutation.[81] Translated into the modern terminology of permutation groups, this type corresponds to *intransitive* groups. RUFFINI defined the second type to contain all permutations which did not belong to the first type and for which there existed some non-trivial subset of roots S such that, in modern notation, $\sigma(S) = S$ or $\sigma(S) \cap S = \emptyset$ for any interchangement σ belonging to the permutation. Such transitive groups were later termed *imprimitive*. The last type consisted of any permutation not belonging to any of the previous types, and thus corresponds to *primitive* groups.

Building on this classification of all permutations into the five types (table 5.1), RUFFINI introduced his other key concept of *degree of equivalence* (Italian: "grado di uguaglianza") of a given function f of the n roots of an equation as the number of different permutations not altering the formal value of f. Denoting the degree of equivalence by p, RUFFINI stated the result of LAGRANGE (see section 5.2) that p must divide $n!$.

77 (Burkhardt, 1892, 133).

78 In modern notation: With f the given function of n quantities, a *permutazione* to RUFFINI was a set $G \subseteq \Sigma_n$ such that $f \circ \sigma = f$ for all $\sigma \in G$.

79 RUFFINI'S simple permutations correspond to the modern concept of cyclic permutation groups.

80 (Ruffini, 1799, 163).

81 I.e. there exists two arrangements a and b such that $\sigma(a) \neq b$ for all σ in the set of interchangements.

Possible numbers of values. RUFFINI at this point turned towards the fifth degree equation. By an extensive and laborious study, helped by his classification, RUFFINI was able to establish that if $n = 5$ the degree of equivalence p could not assume any of the values

$$15, 30, \text{ or } 40.$$

Since the number of different values of the function f could be obtained by dividing $n!$ by p, he had therefore demonstrated that no function f of the five roots of the quintic could exist which assumed

$$\frac{5!}{15} = 8, \frac{5!}{30} = 4, \text{ or } \frac{5!}{40} = 3$$

different values under permutations of the five roots.

Although still embedded in the Lagrangian approach to permutations, RUFFINI'S main result can be viewed as a determination of the index (corresponding to his *degree of equivalence*, p) of all subgroups in Σ_5.

Degrees of radical extractions. In order to prove the impossibility of solving the quintic algebraically, RUFFINI assumed without proof that any radical occurring in a supposed solution would be rationally expressible in the roots of the equation. He never verified this hypothesis, which ABEL later independently formulated and proved. Based on the assumption and the result that no function of the roots $x_1, \ldots, x_5$ could have 3, 4, or 8 values, RUFFINI could prove the insolubility by a nice and short argument which ran as follows.

He first considered a situation in which among two functions Z and M of $x_1, \ldots, x_5$ there existed a relationship of the form

$$Z^5 - M = 0$$

corresponding to the extraction of a fifth root of a rational function. The situation was drawn from the study of a possible solution to the quintic equation where it corresponded to the inner-most root extraction being a fifth root. By implicitly assuming that Z was altered by some interchangement Q which left M unaltered, RUFFINI first observed that Q would have to be a 5-cycle. If Z was unaltered by a non-identity interchangement P, it would also be unaltered by $Q^{-1}PQ$ which belonged to the same permutation. By reference to a result, which he had previously established by examining each of the different types of permutations, RUFFINI found (art. 273) that Q under these conditions would belong to the same permutation as $Q^{-1}PQ$ and therefore could not alter Z, contradicting the assumptions made about Q. Thus, no such non-identity interchangement P could exist, and the 120 values of Z corresponding to different arrangements of $x_1, \ldots, x_5$ were necessarily distinct. Consequently, the first radical to be extracted could not be a fifth root, and since no function of the five roots having three or four values existed, it could not be a third or a fourth root, neither. Therefore, it had to be a square root.

At this point, RUFFINI focused on the second radical to be extracted and the above argument applied equally well to rule out the case of a fifth root. Similarly, it could not be a square root or a fourth root since these would lead to functions having four (2×2) or eight (2×4) values, which were proved to be non-existent. RUFFINI had thus established that any supposed solution to the quintic equation would have to begin with the extraction of a square root followed by the extraction of a third root. However, as he laboriously proved by considering each case in turn, the six-valued function obtained by these two radical extractions did not become three-valued after the initial square root had been adjoined.

The proof which RUFFINI gave for the insolubility of the quintic was thus based on three central parts:

1. The classification of permutations into types (table 5.1)

2. A demonstration, based on (1), that no function of the five roots of the general quintic could have 3, 4, or 8 values under permutations of the roots.

3. A study of the two inner-most (first) radical extractions of a supposed solution to the quintic, in which the result of (2) was used to reach a contraction.

The mere extent of the classification and the caution necessary to include all cases[82] combined with RUFFINI'S intellectual debt to LAGRANGE may serve to view RUFFINI'S work as filling in some of the "infinite labor" described by WARING and LAGRANGE in expressing their doubts about the solubility of higher degree equations (see section 5.4.1 above). However, RUFFINI'S investigations led to the complete reverse result: that the solution of the quintic was impossible.

One of RUFFINI'S friends and critical readers, P. ABBATI (1768–1842), gave several improvements of RUFFINI'S initial proof. The most important one was that he replaced the laborious arguments based on thorough consideration of particular cases by arguments of a more general character.[83] These more general arguments greatly simplified RUFFINI'S proofs that no function of the five roots of the quintic could have 3, 4, or 8 different values. ABBATI was convinced of the validity of RUFFINI'S result but wanted to simplify its proof, and RUFFINI incorporated his improvements into subsequent proofs, from 1802 and henceforth.

Others, however, were not so convinced of the general validity of RUFFINI'S results. Mathematicians belonging to the "old generation" were somewhat stunned by the non-constructive nature of the proofs, which they described as "vagueness". For instance, the mathematician G. F. MALFATTI (1731–1807) severely criticized RUFFINI'S result since it contradicted a general solution which he, himself, previously had

82 According to (Burkhardt, 1892, 135), RUFFINI actually missed the subgroup generated by the cycles (12345) and (132).
83 (ibid., 140).

given.[84] RUFFINI responded with another publication of a version of his proof answering to MALFATTI'S criticism; but before the discussion advanced further, MALFATTI died.

5.5.3 RUFFINI's final proof

In his fifth, and final, publication of his insolubility theorem 1813, RUFFINI recapitulated important parts of LAGRANGE'S theory, in which he emphasized the distinction between numerical and formal equality, before giving the refined version of his proof. According to (Burkhardt, 1892, 155–156), the proof can be dissected into the following parts comparable to the parts of the 1799 proof (see point 3 above):

1. If two functions y and P of the roots $x_1, \ldots, x_5$ of the quintic are related by

   $$y^p - P = 0$$

   (for any p) and P remains unaltered by the cyclic permutation (12345), there must exist a value y_1 of y which in turn changes into y_2, y_3, y_4, and y_5. Consequently,

   $$y_k = \beta^k y_1$$

   where β is a fifth root of unity.

2. If P is furthermore unaltered by the cyclic permutation (123), then y_1 must change into γy_1 where γ is a third root of unity.

3. The permutation (13452) is comprised of the two cycles (12345) (123) and y must remain unaltered. Therefore, $\beta^5\gamma^5 = 1$ which in turn implies that $\gamma = 1$, demonstrating that y cannot be altered by any of the permutations (123), (234), (345), (451), or (512). By combining these 3-cycles the 5-cycle (12345) can be obtained, and thus y cannot be altered by the 5-cycle, neither.

4. Consequently, it is impossible by sequential root extractions to describe functions which have more than two values, and the insolubility is demonstrated.

5.5.4 Reactions to RUFFINI's proofs

In a paper published 1845,[85] P. L. WANTZEL (1814–1848) gave a fusion argument incorporating the permutation theoretic arguments of RUFFINI'S final proof into the setting of ABEL'S proof.[86]

84 (Malfatti, 1804).
85 (Wantzel, 1845).
86 See also (Burkhardt, 1892, 156).

RUFFINI corresponded with CAUCHY, who in 1816 was a promising young Parisian *ingenieur*.[87] CAUCHY praised RUFFINI'S research on the number of values which a function could acquire when its arguments were permuted, a topic CAUCHY, himself, had investigated in an treatise published the year before 1815 with due reference to RUFFINI (see below). Following this exchange of letters CAUCHY wrote RUFFINI another letter in September 1821, in which he acknowledged RUFFINI'S progress in the important field of solubility of algebraic equations:

> "I must admit that I am anxious to justify myself in your eyes on a point which can easily be clarified. Your memoir on the general solution of equations is a work which has always appeared to me to deserve to keep the attention of geometers. In my opinion, it completely demonstrates the algebraic insolubility of the general equations of degrees above the fourth. The reason that I had not lectured on it [the insolubility] in my course in analysis, and it must be said that these courses are meant for students at the École Royale Polytechnique, is that I would have deviated too much from the topics set forth in the curriculum of the École."[88]

At least by 1821, the validity of RUFFINI'S claim that the general quintic could not be solved by radicals was propounded, not only by a somewhat obscure Italian mathematician and the allusions of GAUSS, but also one of the most promising and ambitious French mathematicians of the early nineteenth century. However, it should take further publications, notably by the young ABEL, before this validity would be accepted by the broad international community of mathematicians.

5.6 CAUCHY' theory of permutations and a new proof of RUFFINI's theorem

In November of 1812, CAUCHY handed in a *memoir* on symmetric functions to the *Institut de France* which was published three years later as two separate papers in the *Journal d'École Polytechnique*.[89] The first of the two papers is of special interest in the history of solubility of polynomial equations. It bears the long but precise title *Mémoire sur le nombre des valuers qu'une fonction peut acquérir, lorsqu'on y permute de toutes manières possibles les quantités qu'elle renferme*.[90] Although CAUCHY'S issue was not the solubility-question, his paper was to become extremely important for subsequent research. It was primarily concerned with a more general version of RUFFINI'S result

87 (Ruffini, 1915–1954, vol. 3, 82–83).

88 *"Je suis impatient, je l'avous, de me justifier à Vos yeux sur un point qui peut être facilement éclairi. Votre mémoire sur la résolution générale des équations est un travail qui m'a toujours paru digne de fixer l'attention des géomètres, et qui, à mon avis, démontre complètement l'insolubilité algébrique des équations générales d'un dégré supérieur au quatrième. Si je n'en ai pas parlé dans mon cours d'analyse, c'est que, ce cours étant destiné aux élèves d'École Royale Polytechnique, je ne devois pas trop m'écarter des matières indiquées dans les programmes de l'école."* (ibid., vol. 3, 88–89).

89 (A.-L. Cauchy, 1815a; A.-L. Cauchy, 1815b).

90 (A.-L. Cauchy, 1815a).

Figure 5.5: AUGUSTIN-LOUIS CAUCHY (1789–1857)

that no function of five quantities could have three or four different values when its arguments were permuted (see above). Before going into this particular result, however, CAUCHY devised the terminology and notation which he was going to use. Precisely in formulating exact and useful notation and terminology, CAUCHY advanced well beyond his predecessors and laid the foundations upon which the nineteenth-century theory of permutations would later build.

Notational advances. With CAUCHY, the term "permutation" came to mean an arrangement of indices, thereby replacing the "arrangements" of which RUFFINI spoke. A "substitution" was subsequently defined to be a transition from one permutation to another (which is the modern meaning of "permutation"), and CAUCHY devised writing it as, for instance,

$$\binom{1.2.4.3}{2.4.3.1}. \tag{5.12}$$

CAUCHY'S convention was that in the expression K, to which the substitution (5.12) was to be applied, the index 2 was to replace the index 1, the index 4 to replace 2, 3 should replace 4, and 1 should replace 3. More generally, CAUCHY wrote

$$\binom{A_1}{A_2}$$

for the substitution which transformed the permutation A_1 into A_2 in the above-mentioned way.[91] He then defined $\binom{A_1}{A_6}$ to be the *product* of two substitutions $\binom{A_2}{A_3}$ and $\binom{A_4}{A_5}$ if it gave the same result as the two applied sequentially,[92] in which case CAUCHY wrote

$$\binom{A_1}{A_6} = \binom{A_2}{A_3}\binom{A_4}{A_5}.$$

Furthermore, he defined the *identical* substitution and *powers* of a substitution to have the meanings we still attribute to these concepts today.[93] The smallest integer n such that the n^{th} power of a substitution was the identity substitution, CAUCHY called the *degree* of the substitution.[94] All these notational advances played a central part in formalizing the manipulations on permutations and were soon generally adopted.

LAGRANGE'S Theorem. In order to demonstrate LAGRANGE'S theorem, CAUCHY let K denote an arbitrary expression in n quantities,

$$K = K(x_1, \ldots, x_n).$$

With $N = n!$, he labelled the N different *permutations* of these n quantities

$$A_1, \ldots, A_N.$$

The values which K would acquire when the corresponding substitutions of the form $\binom{A_1}{A_u}$ were applied were correspondingly labelled $K_1, \ldots, K_N$,

$$K_u = K\binom{A_1}{A_u} \text{ for } 1 \leq u \leq N.$$

If these were all distinct, the expression K would obviously have N different values when its arguments were interchanged. In the contrary case, CAUCHY assumed that for M indices the values of K were equal

$$K_\alpha = K_\beta = K_\gamma = \ldots.$$

The core of the proof was CAUCHY'S realization that if the permutation A_λ was fixed and the substitution $\binom{A_\alpha}{A_\beta}$ was applied to A_λ giving A_μ, i.e.

$$A_\mu = \binom{A_\alpha}{A_\beta} A_\lambda,$$

the corresponding values K_λ and K_μ would be identical. Consequently, the different values of K came in bundles of M and CAUCHY had deduced that M had to divide $n!$. The central concept of *degree of equivalence*, which RUFFINI had introduced to mean

91 (A.-L. Cauchy, 1815a, 67).
92 (ibid., 73).
93 (ibid., 73, 74)
94 (ibid., 76). ABEL was later to change this term to the now standard *order*.

the number of substitutions which left the given function unaltered, was renamed the *indicative divisor* (French: "*diviseur indicatif*") by CAUCHY and was exactly what he had denoted by M. Terming the number of different values of K under all possible substitutions the *index* of the function K and denoting it by R, CAUCHY had obtained the formula

$$n! = R \times M. \tag{5.13}$$

The RUFFINI-CAUCHY Theorem. After explicitly providing the function of n quantities $a_1, \ldots, a_n$ given by

$$\prod_{1 \leq i < j \leq n} (a_i - a_j)$$

to prove the existence of functions having two different values under all substitutions, CAUCHY turned to the result that no function of five or more quantities could have three values when its arguments weere interchanged. He gave credit to RUFFINI'S works before describing the generalization, which he had made:[95]

> "The number of different values of a non-symmetric function of n quantities cannot be less than the largest prime number p restrained by[96] n without being equal to 2."[97]

CAUCHY split his proof of this theorem into three sections:

In the first part, CAUCHY demonstrated that under the hypothesis $R < p$ (R being the *index*, i.e. the number of values of K), the function K remained unaltered under any substitution of degree (order) p. His proof consisted of denoting by $\binom{A_s}{A_t}$ a substitution of degree m and letting $A_1, \ldots, A_m$ denote the m permutations obtained by applying powers of the substitution $\binom{A_s}{A_t}$ to the first permutation A_1. CAUCHY called $A_1, \ldots, A_m$ a *circle of permutations*. He could then prove the central property that for positive, integral values of x,

$$\begin{pmatrix} A_s \\ A_t \end{pmatrix}^{mx+r} = \begin{pmatrix} A_s \\ A_t \end{pmatrix}^{r}, \tag{5.14}$$

and when $\binom{A_s}{A_t}$ was applied to the $N = n!$ permutations $A_1, \ldots, A_N$, these split themselves into $\frac{N}{m}$ circles each holding m permutations (see table 5.2). In case the number M which indicated the number of permutations corresponding to a single value of K was larger than $\frac{N}{m}$, there had to be two permutations, A_x and A_y both in the same circle, corresponding to a single value of K. The substitution $\binom{A_x}{A_y}$ applied to A_x gave A_y, but since A_x and A_y belonged to the same circle, A_y corresponded to applying a

95 CAUCHY explicitly refered to RUFFINI'S book (Ruffini, 1799) and to the article (Ruffini, 1805).

96 By "*p contenu dans n*" CAUCHY meant that p was additively contained in n, i.e. $p \leq n$.

97 "*Le nombre des valeurs différentes d'une fonction non symétrique de n quantités ne peut s'abaisser au-dessous du plus grand nombre premier p contenu dans n sans devenir égal à* 2." (A.-L. Cauchy, 1815a, 72).

Each circle of permutations is represented by a row in the following table:

$$\begin{array}{llll} A_2 = \binom{A_s}{A_t} A_1, & \dots, & A_m = \binom{A_s}{A_t} A_{m-1}, & A_1 = \binom{A_s}{A_t} A_m, \\ A_{m+2} = \binom{A_s}{A_t} A_{m+1}, & \dots, & A_{2m} = \binom{A_s}{A_t} A_{2m-1}, & A_{m+1} = \binom{A_s}{A_t} A_{2m}, \\ \dots & \dots & \dots & \dots \\ A_{N-m+2} = \dots, & \dots, & A_N = \binom{A_s}{A_t} A_{N-1}, & A_{N-m+1} = \binom{A_s}{A_t} A_{N-m}. \end{array}$$

These permutations can be reordered when (5.14) is taken into account:

$$\begin{array}{llll} A_1, & A_2 = \binom{A_s}{A_t} A_1, & \dots, & A_m = \binom{A_s}{A_t}^{m-1} A_1, \\ A_{m+1}, & A_{m+2} = \binom{A_s}{A_t} A_{m+1}, & \dots, & A_{2m} = \binom{A_s}{A_t}^{m-1} A_{m+1}, \\ \dots & \dots & \dots & \dots \\ A_{N-m+1}, & A_{N-m+2} = \binom{A_s}{A_t} A_{N-m+1}, & \dots, & A_N = \binom{A_s}{A_t}^{m-1} A_{N-m+1}. \end{array}$$

The notation $\binom{A_s}{A_t} A_1$ indicates that the substitution $\binom{A_s}{A_t}$ be applied to the permutation A_1.

Table 5.2: The $\frac{N}{m}$ circles formed by applying $\binom{A_s}{A_t}$ to $A_1, \dots, A_N$.

power of $\binom{A_s}{A_t}$ to A_x. Consequently, the substitution $\binom{A_x}{A_y}$ was equal to a power of $\binom{A_s}{A_t}$. If m were a prime, the converse would also be true, since if

$$\begin{pmatrix} A_s \\ A_t \end{pmatrix}^k = \begin{pmatrix} A_x \\ A_y \end{pmatrix}$$

and $(k, m) = 1$, there existed α, β such that $\alpha k + \beta m = 1$, i.e.

$$\begin{pmatrix} A_s \\ A_t \end{pmatrix} = \begin{pmatrix} A_s \\ A_t \end{pmatrix}^{\alpha k + \beta m} = \begin{pmatrix} A_s \\ A_t \end{pmatrix}^{\alpha k} = \begin{pmatrix} A_x \\ A_y \end{pmatrix}^{\alpha}.$$

The details of this argument were left out by CAUCHY, but were later provided by ABEL.[98] Since A_x and A_y corresponded to the same value of K, the function would not change if the substitution $\binom{A_x}{A_y}$ were applied. Consequently, K would also remain unaltered when the substitution $\binom{A_s}{A_t}$ was applied, and the number of different values of K, which CAUCHY had denoted M, could not be greater than $\frac{N}{m}$, whereby he had reached a contradiction. Setting $m = p$, CAUCHY had obtained the desired result.

In the second part, CAUCHY demonstrated by decomposing p-cycles into 3-cycles that if the value of K remained unaltered by all substitutions of degree p it would also be unaltered by any circular substitution of order 3. The important step was obtained by realizing that the product of the two circular substitutions of order p

$$\begin{pmatrix} \alpha\beta\gamma\delta\dots\zeta\eta \\ \beta\gamma\delta\varepsilon\dots\eta\alpha \end{pmatrix} \text{ and } \begin{pmatrix} \beta\gamma\delta\varepsilon\dots\eta\alpha \\ \gamma\alpha\beta\delta\dots\zeta\eta \end{pmatrix} \tag{5.15}$$

98 (N. H. Abel, 1826a).

was the 3-cycle

$$\begin{pmatrix}\alpha\beta\gamma\\ \gamma\alpha\beta\end{pmatrix}. \tag{5.16}$$

Thus, given any 3-cycle (5.16), the two p-cycles (5.15) could be formed. Under the hypothesis, these p-cycles left K unaltered, whereby the same was true of their product, i.e. the 3-cycle (5.16).

In the third and final part of the proof CAUCHY established that if the value of K was unaltered by all 3-cycles, the function K would either be symmetric or have two different values. In his proof, analogous to the second part described above, he decomposed the 3-cycle

$$\begin{pmatrix}\alpha\beta\gamma\\ \gamma\alpha\beta\end{pmatrix}$$

into the product of the two transpositions

$$\begin{pmatrix}\alpha\beta\\ \beta\alpha\end{pmatrix}\begin{pmatrix}\beta\gamma\\ \gamma\beta\end{pmatrix}$$

which he wrote as $(\alpha\beta)\,(\beta\gamma)$. This step of the proof corresponds to proving that the *alternating group* A_n is generated by all 3-cycles.

In the remaining part of the paper, CAUCHY demonstrated for functions of six arguments, if $R < 5$ the function would necessarily be symmetric or have two values. Generally, CAUCHY noted, for $n > 4$ no functions of n quantities were known which had less than n values without this number being either 1 or 2. After these two early papers on the theory of permutations, CAUCHY would let the topic rest for 30 years being preoccupied with his many other research themes and his teaching. When he finally returned to the theory of permutations in the 1840s, CAUCHY demonstrated the following generalization of his 1815 result: That no function of n quantities could take on less than n values without either being symmetric or taking on exactly 2 values.[99]

With his paper,[100] CAUCHY founded the theory of permutations by providing it with its principal objects: the permutations. He introduced terms and notation which enabled him to grasp the substitutions as objects abstracted from their action on the formal values of a function, and he provided an important theorem in this new theory which he based on an elegant, non-computational proof.

5.7 Some algebraic tools used by GAUSS

GAUSS' first proof of the Fundamental Theorem of Algebra had, in a central way, depended on geometrical (topological) intuitions. In 1815, GAUSS published a second proof of the theorem,[101] this time applying *algebraic* methods. In the process, GAUSS

99 (Dahan, 1980, 281–282).
100 (A.-L. Cauchy, 1815a).
101 (C. F. Gauss, 1815). Eventually, GAUSS would publish two further proofs (one in 1816 and one in 1849) bringing his total to four.

spelled out some of the most important algebraic tools of the early 19th century; therefore some of his tools are briefly sketched in the present context. During the proof, GAUSS dealt with results such as the Euclidean algorithm applied to polynomials and the "elementariness" of the elementary symmetric functions, both of which will become immensely important in ABEL'S theory of algebraic solubility as described in subsequent chapters. Whether ABEL studied any of GAUSS' proofs of the fundamental theorem of algebra is not clear; there are no explicit references to these proofs in ABEL'S writings, nor is ABEL anywhere concerned with the existence of roots.[102] Thus, the similarity of methods in GAUSS' proof and ABEL'S subsequent algebraic research may equally well be attributed to their belonging to the same common framework and mathematical tradition.

Explicitly stressing the connection to the procedure used to determine the greatest common divisor of integers, GAUSS applied the Euclidean algorithm to polynomials. Besides producing the greatest common divisor, the procedure also proved that two polynomials Y, Y' have no (non-trivial) common divisor if and only if there exists another pair of polynomials Z, Z' such that

$$ZY + Z'Y' = 1.$$

The second tool which GAUSS introduced concerned symmetric functions, and amounts to the central theorem on symmetric functions. By firstly decomposing any symmetric function of $a, b, c, \ldots$ in a sum of terms

$$Ma^{\alpha}b^{\beta}c^{\gamma}\ldots$$

and secondly imposing an ordering on such terms, GAUSS was able to prove that any symmetric function could be realized as an entire function of the elementary symmetric functions.

Besides these tools, GAUSS' argument rested upon central properties of the quantity which he termed the determinant (today called the discriminant) of $Y(x) = \prod (x - x_k)$,

$$\prod_{i \neq j} \left(x_i - x_j\right).$$

GAUSS was able to demonstrate that the determinant vanishes if and only if Y and $\frac{d}{dx}Y$ have a common divisor, i.e. a common root.

102 Without references, KLINE writes as if ABEL had given a proof of the fundamental theorem of algebra (Kline, 1990, 599). I have not been able to identify such a proof, nor have I any idea how KLINE had come to believe that ABEL had even worked on it.

Chapter 6

ABEL on the algebraic insolubility of the quintic: limiting the class of solvable equations

In spite of the efforts of P. RUFFINI (1765–1822) and C. F. GAUSS (1777–1855), the search for an algebraic solution of the quintic remained an attractive problem to a generation of young and aspiring mathematicians. In Norway, N. H. ABEL (1802–1829) thought he had solved it, but soon realized that he had been misled. In Germany, C. G. J. JACOBI (1804–1851) worked on the problem,[1] and in France E. GALOIS (1811–1832), too, thought he had found a solution, only to be disappointed.[2] All of them attacked the problem while they still attended pre-university education. The easy formulation and yet century-long history of the problem, and a general belief that its solution should be possible and not too difficult, made it appear as a good opening into doing creative mathematics.

Inspired by the stimulation of his new, and young, mathematics teacher B. M. HOLMBOE (1795–1850), ABEL studied the masters and began to engage in creative mathematics of his own. In 1821, he thought he had produced a solution to the general fifth degree equation. In the incipient intellectual atmosphere of Christiania, few authorities capable of determining the validity of ABEL'S reasoning could be found. But more importantly, the scientific milieu of Norway was still without a means of publication of technical mathematical results deserving international recognition. For these reasons, professor C. HANSTEEN (1784–1873) sent ABEL'S manuscript to professor C. F. DEGEN (1766–1825) in Copenhagen for evaluation and possibly publication in the transactions of the *Royal Danish Academy of Sciences and Letters*. The accompanying letter which HANSTEEN must have written and the paper, itself, are no longer preserved. Our only primary source of information is the letter which DEGEN wrote back to HANSTEEN, in which he asked for an elaborated version of the argument and

1 (G. L. Dirichlet, 1852, 4).
2 (Toti Rigatelli, 1996, 33).

an application to a specific numerical example.

> "As for the talented Mr. Abel, I will be happy to present his treatise to the Royal Academy of Science. It shows, even if the goal has not been reached, an extraordinary head and extraordinary insights, especially for someone his age. Nevertheless, I excuse myself to require the condition that Mr. A. sends an *elaborated deduction* of his result together with a *numerical example*, taken from, for instance, an equation such as $x^5 - 2x^4 + 3x^2 - 4x + 5 = 0$. I believe that this will be a rather necessary lapis lydius [Lydian stone] for him, as I recall what happened to Meier Hirsche[3] and his *ευρηκα* [Eureka]; item [furthermore] I would, since the latter part of the communicated manuscript would not be easily readable to the majority of the members of the Academy, ask for another copy of it."[4]

We have no indication that ABEL ever produced an elaborated deduction; apparently the numerical examples worked their part — as the probes of truth — as DEGEN had suggested and led ABEL to a radically new insight. In 1824, he published, at his own expense, a short work in French entitled *Mémoire sur les équations algébriques ou l'on démontre l'impossibilité de la résolution de l'équation générale du cinquième degré*.[5] It demonstrated the impossibility of solving the general equation of the fifth degree by algebraic means — ABEL had left the last essential requirement out of the title. ABEL intended the memoir to be his best introduction on his planned tour of the Continent. Since he had to pay for the publication himself, he compressed the proof to cover only six pages and his style of presentation suffered accordingly. In numerous points he was unclear or left advanced arguments out. When ABEL came into contact with A. L. CRELLE (1780–1855) in Berlin, he found himself in a position to make his discovery available to a broader public. He rewrote the argument elaborating the ideas of the 1824 proof, and had CRELLE translate it into German for publication in the very first issue of *Journal für die reine und angewandte Mathematik* which appeared in 1826.[6] Through this paper — and the French report of it,[7] which ABEL wrote for the *Bulletin des sciences mathématiques, astronomiques, physiques et chimiques* edited by BARON DE FERRUSAC (1776–1836) [8] — the world gradually came to know that a young Norwegian had settled the question of solubility of the general quintic in the negative.

3 M. HIRSCHE (1765–1851) was a teacher of mathematics in Berlin who in 1809 published a collection of exercises. There, he thought he had given the general solution to all equations. He quickly discovered his error, perhaps by a Lydian probe as DEGEN recommends. (N. H. Abel, 1902e, Oplysninger til Brevene, p. 125)

4 "*Hvad den talentfulde Hr. Abel angaar, da vil jeg med Fornøielse fremlægge hans Afhandling for det Kgl. V. S. Den viser, om end ikke Maalet skulde være opnaaet, et ualmindeligt Hoved og ualmindelige Indsigter, især i hans Alder. Dog maatte jeg som Bøn tilføie den Betingelse: At Hr. A. sender en* udførligere Deduction *af sit Resultat og tillige et* numerisk Exempel, *tagen f. Ex. af en Ligning som denne:* $x^5 - 2x^4 + 3x^2 - 4x + 5 = 0$. *Dette vil efter min Overbevisning være en saare nødvendig lapis lydius for ham Selv, da man veed, hvorledes det gik Meier Hirsche med hans ευρηκα; item maatte jeg, da den sidste Deel af den mig communicerede Afh. ikke vilde være ret læselig for de fleeste af S.'s Medlemmer, udbede mig en anden Afskrift af samme.*" (Degen→Hansteen, Kjøbenhavn, 1821/05/21. N. H. Abel, 1902b, 93).

5 (N. H. Abel, 1824b).

6 (N. H. Abel, 1826a).

7 (N. H. Abel, 1826c).

8 Dates from (Stubhaug, 1996, 580).

In this chapter, I give a presentation of ABEL'S proof using the tools and methods available to him. As described in the introduction,[9] this approach allows me to place ABEL'S proof in a historical context within mathematics. For expositions of ABEL'S proof involving the modern concepts introduced in Galois theory, see for instance (R. Ayoub, 1982; M. I. Rosen, 1995; Skau, 1990).

6.1 The first break with tradition

In the opening paragraph of the paper in CRELLE'S *Journal für die reine und angewandte Mathematik*, ABEL described the approach he had taken. In order to answer the question of solubility of equations, he proposed to investigate the forms of all algebraic expressions in order to determine if they could "solve" the equation. Although ABEL throughout spoke of *algebraic functions*, I use the term *algebraic expressions* to avoid any confusion with the modern concept of a function as a mapping between sets. The algebraic expressions which ABEL considered were algebraic combinations of the coefficients of the given equation, and thus his approach was in line with the one taken earlier by A.-T. VANDERMONDE (1735–1796) (see section 5.1).[10]

> "As is known, the algebraic equations up to the fourth degree can be solved in general. Equations of higher degrees, however, only in particular cases, and if I am not mistaken, the question:
>
> Is it *possible* to solve equations of higher than the fourth degree in general?
>
> has not yet been answered in a satisfactory manner. The present treatise is concerned with this question.
>
> To solve an equation algebraically is but to express its roots by algebraic functions of its coefficients. Therefore, one must first consider the general *form* of algebraic functions and subsequently investigate whether it is possible that the given equation can be satisfied by inserting the expression of an algebraic function in place of the unknown quantity."[11]

In the quote, ABEL also introduced an important notion of *satisfiability*. An equation was said to be satisfied by an algebraic expression if the expression was a root of the equation. Consequently, an equation was said to be satisfiable if an algebraic expression existed which satisfied it. This differed from the notion of algebraic *solubility* which required that *all* the roots of the equation could be expressed algebraically.

9 See section 1.4.

10 (Kiernan, 1971, 67).

11 "*Bekanntlich kann man algebraische Gleichungen bis zum vierten Grade allgemein auflösen, Gleichungen von höhern Graden aber nur in einzelnen Fällen, und irre ich nicht, so ist die Frage:*

Ist es möglich, *Gleichungen von höhern als dem vierten Grade allgemein aufzulösen?*

noch nicht befriedigend beantwortet worden. Der gegenwärtige Aufsatz hat diese Frage zum Gegenstande.

Eine Gleichung algebraisch auflösen heißt nichts anders, als ihre Wurzeln durch eine algebraische Function der Coefficienten ausdrücken. Man muß also erst die allgemeine Form *algebraischer Functionen betrachten und alsdann untersuchen, ob es möglich sei, der gegebenen Gleichung auf die Weise genug zu thun, daß man den Ausdruck einer algebraischen Function statt der unbekannten Größe setzt.*" (N. H. Abel, 1826a, 65).

This shift from the trial-and-error based search for solutions toward a theoretical and general investigation of the class of algebraic expressions marks ABEL'S first break with the traditional approach to the theory of equations. ABEL investigated the extent to which algebraic expressions could satisfy given polynomial equations and was led to describe necessary conditions. By this choice of focus, ABEL implicitly introduced a new object, *algebraic expression*, into the realm of algebra, and the first part of his paper can be seen as an opening study of this object, devised in order to obtain a firm description of it and to prove the first central theorem concerning it.[12] In section 19.3, another aspect of ABEL'S concept of algebraic expression is taken up.

6.2 Outline of ABEL's proof

The paper in CRELLE'S *Journal für die reine und angewandte Mathematik* can be divided into four sections reflecting the overall structure of ABEL'S proof. In the first section, ABEL introduced his definition of algebraic functions and classified these by their orders and degrees. He used this definition to study the restrictions imposed on the form of algebraic expressions if they had to be solutions to a given *solvable* equation. In doing so, he proved the result — which RUFFINI had failed to see — that any radical (algebraic sub-expression) contained in a supposed solution would depend rationally on the roots of the equation (see section 6.3).

In the second section, ABEL reproduced the elements of A.-L. CAUCHY'S (1789–1857) theory of permutations from 1815 needed for his proof.[13] These included CAUCHY'S notation and the result described above as the CAUCHY-RUFFINI theorem (section 5.6) demonstrating that no function of the five roots of the general quintic could take on three or four different values under permutations of these roots (see section 6.4).

The third part contained detailed and highly explicit investigations of functions of five quantities taking on two or five different values under all permutations of the roots. Through an explicit theorem, which linked the number of values under permutations to the degree of the root extraction (see section 6.5), ABEL demonstrated that all non-symmetric rational functions of five quantities could be reduced to two basic forms.

Finally, these preliminary sections were combined to provide ABEL'S impossibility proof by discarding each of a number of cases *ad absurdum* (section 6.6). ABEL'S argument can be outlined in the following steps:

1. ABEL introduced a classification of algebraic expressions to obtain a standard form, rational in the roots, which all possible solutions to the general quintic equation had to possess.

12 Studying algebraic expressions as objects has been seen as a first step in what later became the introduction of functions as mappings (especially automorphisms) into algebra and separating functions from their ties with analysis. (Kiernan, 1971, 70)

13 (A.-L. Cauchy, 1815a).

2. The classification also enabled ABEL to link the number of values under permutations to the exponent of the involved root extraction.

3. By adapting CAUCHY'S theory of permutations, a restriction of the possible number of values under permutations to 2 or 5 was achieved.

4. Finally, ABEL reduced each of the possible cases by indirect proofs.

In general, ABEL used references in accordance with the nineteenth century tradition. Throughout, ABEL'S approach to the question of solubility of the quintic was based on counting the number of values which a rational function took when its arguments were permuted. Thus, he clearly worked in the tradition initiated by J. L. LAGRANGE (1736–1813), and it is a little remarkable that no reference to — or even mention of — LAGRANGE was ever made in ABEL'S published works on the theory of equations. I take this as an indication that during the half-century elapsed since LAGRANGE'S trend-setting research,[14] his results and approach had become common practice in the field. On the other hand, ABEL made explicit reference to CAUCHY'S work on the theory of permutations,[15] from which he had borrowed the CAUCHY-RUFFINI theorem without proof in his original 1824 version.[16] In the proof published two years later in CRELLE'S *Journal für die reine und angewandte Mathematik*,[17] ABEL provided the theorem with his own shorter proof, keeping the reference. Thus, by the same argument as above, CAUCHY'S much younger theory had not yet been as widely established.

6.3 Classification of algebraic expressions

The objects which ABEL called *algebraic functions* — and which I term *algebraic expressions* — were *explicit algebraic functions*: finite combinations of constant and variable quantities obtained by basic arithmetical operations. If the operations included only addition and multiplication, the expression was said to be *entire*; if, furthermore, division was involved, it was called *rational*; and if, additionally, root extractions were allowed, the expression was denoted an *algebraic expression*. Subtraction and extraction of roots of composite degree were explicitly reduced to addition and the extraction of roots of prime degree, respectively, in order to be contained in the above operations. In the subsequent classification, ABEL benefited from the simplicity introduced by this *minimal* definition in which only root extractions of prime degree were considered.

The purpose of ABEL'S investigations of algebraic expressions was to obtain an important auxiliary theorem for his impossibility proof. Based on a definition which in-

14 (Lagrange, 1770–1771).
15 (A.-L. Cauchy, 1815a).
16 (N. H. Abel, 1824b).
17 (N. H. Abel, 1826a).

troduced algebraic expressions as objects, ABEL derived a standard form for these objects. Applying it to algebraic expressions which satisfied a given equation, he found that these could always be given a form in which all occurring components depended rationally on the roots of the equation.

In his effort to obtain a classification of algebraic expressions, ABEL introduced a hierarchy based on the concepts of *order* and *degree*. These concepts introduced a structure in the class of algebraic expressions allowing ordering and induction to be carried out.

In dealing with the proof which ABEL gave of his auxiliary theorem, we are introduced to two other concepts which are even more fundamental to his theory of algebraic solubility. These are the Euclidean division algorithm and the concept of irreducibility. In section 6.3.3, the proof is presented in quite some detail to demonstrate how ABEL made use of these concepts. They were to become even more important in his unpublished general theory of solubility (see chapter 8).

6.3.1 Orders and degrees

ABEL'S classification of algebraic functions (expressions) was hierarchic; his means to obtain structure were the two concepts of *order* and *degree*. The order was introduced to capture the *depth* of nested root extractions, whereas the degree kept track of root extractions at the same level by imposing a finer structure. ABEL defined rational expressions to be of order 0, and the order concept was thereafter defined inductively. Thus, if f was a rational function of expressions of order $\mu - 1$ and root extractions of prime degree of such expressions, f would be an algebraic expression of order μ. With this idea, ABEL obtained the following standard form of algebraic expressions of order μ:

$$f\left(g_1, \ldots, g_k; \sqrt[p_1]{r_1}, \ldots, \sqrt[p_m]{r_m}\right), \tag{6.1}$$

where f was a rational expression, the expressions $g_1, \ldots, g_k$ and $r_1, \ldots, r_m$ were algebraic expressions of order $\mu - 1$, and $p_1, \ldots, p_m$ were primes.

Thus, as indicated, ABEL'S concept of order counted the number of nested root extractions of prime degree. For instance, if R was a rational function (i.e. of order 0), $\sqrt{R}$ was of order 1, $\sqrt[3]{\sqrt{R}}$ of order 2, and similarly $\sqrt[3]{\sqrt{R}} + \sqrt{R}$ was of order 2. Also $\sqrt[4]{R}$ was of order 2, since it would have to be decomposed as two nested square roots, $\sqrt{\sqrt{R}}$.

Within each order, ABEL described another hierarchy controlled by the concept of *degree*. While the order served to denote the number of nested root extractions of prime degree, ABEL'S concept of the degree of an algebraic expression counted the number of co-ordinate root extractions at the top level. Thus in (6.1), it was the minimal value of m which would suffice to write the expression in this form. In table 6.1, I have illustrated the concepts by listing the orders and degrees of one of the

Expression	**Order**	**Degree**
$\sqrt[3]{R+\sqrt{Q^3+R^2}}+\sqrt[3]{R-\sqrt{Q^3+R^2}}$	2	2
$\sqrt[3]{R+\sqrt{Q^3+R^2}}$	2	1
$R+\sqrt{Q^3+R^2}$	1	1
Q^3+R^2	0	0

Table 6.1: The order and degree of some expressions in CARDANO'S solution to the general cubic $x^3+a_2x^2+a_1x+a_0=0$. R and Q are assumed to be certain rational functions of the given quantities, here the coefficients a_0, a_1, a_2.

G. CARDANO (1501–1576) solutions to the general cubic. Any rationally related root extractions were, ABEL said, to be combined and any algebraic expressions of order μ and degree 0 were to be simplified as algebraic expressions of order $\mu-1$.

ABEL never considered whether his definitions of order and degree were total, i.e. whether any algebraic expression could (uniquely) be ascribed an order and a degree; throughout his investigations of algebraic expressions, ABEL tacitly used that to any such object corresponded a unique order and a unique degree. It is obvious that these concepts introduced a hierarchy on the class of algebraic expressions (see table 6.1).

6.3.2 Standard form

Based on his hierarchy of algebraic expressions, ABEL demonstrated a central theorem concerning these newly defined objects. It was to serve as a concrete standard form for algebraic expressions. First, ABEL found a slightly modified standard form (6.1) by writing an algebraic expression v of order μ and degree m as

$$v=f\left(r_1,\ldots,r_k,\sqrt[p]{R}\right), \tag{6.2}$$

where f was rational, $r_1,\ldots,r_k$ were expressions of order μ but degree at most $m-1$, whereas R was an expression of order $\mu-1$ such that $\sqrt[p]{R}$ could not be expressed rationally in $r_1,\ldots,r_k$, and p was a prime. ABEL obtained this alternative standard form (6.2) from (6.1) by allowing the arguments $r_1,\ldots,r_k$ to be of the same order as v, but of lower degree. The two standard forms were equivalent and the hierarchic structure in the class of algebraic expressions was preserved.

Writing the *rational expression* f as the ratio of two entire expressions,

$$v=\frac{T\left(r_1,\ldots,r_k,\sqrt[p]{R}\right)}{V\left(r_1,\ldots,r_k,\sqrt[p]{R}\right)},$$

ABEL specified the form of v as the ratio of two polynomials in $\sqrt[p]{R}$ of degree at most $p-1$,

$$v=\frac{T}{V}. \tag{6.3}$$

After denoting by $V_1, \ldots, V_{p-1}$ the values of V by inserting $\alpha^k \sqrt[p]{R}$ for $\sqrt[p]{R}$ in V (α a p^{th} root of unity), ABEL multiplied numerator and denominator of (6.3) by $V_1 V_2 \ldots V_{p-1}$. The denominator thereby became a rational function of $r_1, \ldots, r_k$ "as it is known".[18] The conclusion can be seen as an application of ABEL'S implicit version of LAGRANGE'S theorem 1.[19]

By this analogous of multiplying the denominator by conjugates,[20] ABEL had shown that the expression v could be written as a *polynomial* in $\sqrt[p]{R}$,

$$v = f\left(r_1, \ldots, r_k, \sqrt[p]{R}\right) = \sum_{u=0}^{p-1} q_u R^{\frac{u}{p}},$$

where R was of order $\mu - 1$ and all the coefficients $q_0, \ldots, q_{p-1}$ were functions of order μ and degree at most $m-1$ such that $R^{\frac{1}{p}}$ could not be expressed rationally in the coefficients. ABEL also stated that the coefficient q_1 could be assumed equal to 1. In this last step, ABEL'S conclusions concerning the orders and degrees of the other coefficients were too bold, as W. R. HAMILTON (1805–1865) and L. KÖNIGSBERGER (1837–1921) in 1839 and 1869, respectively, were to point out (see section 6.9.1).[21] In general, this step — obtained by dividing each coefficient by q_1 — might effect the order of R which could now be μ. However, as KÖNIGSBERGER also noticed, the mistake was not an essential one and has no consequences for the rest of the proof (see section 6.9.1).

In ABEL'S version, the standard form of algebraic expressions can be described by theorem 2.

Theorem 2 *Let v be an algebraic expression of order μ and degree m. Then*

$$v = q_0 + p^{\frac{1}{n}} + q_2 p^{\frac{2}{n}} + \cdots + q_{n-1} p^{\frac{n-1}{n}}, \tag{6.4}$$

where n is a prime, $q_0, q_2, \ldots, q_{n-1}$ are algebraic expressions of order μ and degree at most $m-1$, and p is an algebraic expression of order μ [ABEL stated $\mu - 1$, see below] such that $p^{\frac{1}{n}}$ cannot be expressed as a rational function of $q_0, q_2, \ldots, q_{n-1}$. (N. H. Abel, 1826a, 70) □

In his modified version, KÖNIGSBERGER only concluded that the algebraic expression p was of order μ and degree at most $m-1$, and that the order of $p^{\frac{1}{n}}$ was μ.

18 (N. H. Abel, 1826a, 69).

19 The function V can be interpreted as depending upon all the roots of the equation $X^p = R$, i.e. $V = V\left(\sqrt[p]{R}, \alpha\sqrt[p]{R}, \ldots, \alpha^{p-1}\sqrt[p]{R}\right)$ although only the first argument is actually involved. The values $V_0, \ldots, V_{p-1}$ are then obtained by transposing the first argument with any other argument, and the theorem 1 states that the product $\prod_{u=0}^{p-1} V_u$ is a rational function of $\sqrt[p]{R}, \ldots, \alpha^{p-1}\sqrt[p]{R}$ and the coefficients of V.

20 In order to obtain a *real* denominator of the fraction

$$\frac{a+ib}{c+id}$$

its numerator and denominator are both multiplied by $c - id$.

21 (W. R. Hamilton, 1839; Königsberger, 1869).

Once ABEL had reduced the algebraic expressions to their standard forms (6.4), he devoted an entire section to demonstrate the central description of algebraic expressions which could satisfy a given equation.

6.3.3 Expressions which satisfy a given equation

ABEL began with the assumption that the given equation

$$\sum_{u=0}^{k} c_u y^u = 0, \tag{6.5}$$

in which the coefficients were rational functions of some quantities $x_1, \ldots, x_n$, would be satisfied by inserting for y an algebraic expression of the form (6.4). He deduced that (6.5) would be transformed into an equation in $p^{\frac{1}{n}}$ and found that he could write it as

$$\sum_{u=0}^{n-1} r_u p^{\frac{u}{n}} = 0, \tag{6.6}$$

in which $r_0, \ldots, r_{n-1}$ were rational functions of $p, q_0, q_2, \ldots, q_{n-1}$.

The central result which ABEL obtained in this connection was that for this equation to be satisfied the coefficients $r_0, \ldots, r_{n-1}$ all had to vanish (lemma 1). His proof is a beauty and clearly reflects the central methods involved in his approach to the theory of equations.

Lemma 1 *If the equation (6.6) is satisfied, the coefficients $r_0, \ldots, r_{n-1}$ all vanish.* □

ABEL transformed the assumption that (6.6) could be satisfied into the assumption that the two equations

$$\begin{cases} z^n - p = 0 \\ \displaystyle\sum_{u=0}^{n-1} r_u z^u = 0 \end{cases}$$

had one or more common roots. If some of the coefficients $r_0, \ldots, r_{n-1}$ did not vanish the latter equation would have degree at most $n-1$. Thus, the two equations could at most share $n-1$ roots, and ABEL denoted the number of common roots by k. When he formed the equation having precisely these k roots as its roots,

$$\prod_{u=1}^{k} (z - z_u) = \sum_{u=0}^{k} s_u z^u = 0 \tag{6.7}$$

he realized that the coefficients $s_0, \ldots, s_{k-1}$ depended rationally on $r_0, \ldots, r_{n-1}$. ABEL gave no details at this point, but I assume that he obtained the result applying the Euclidean division algorithm to polynomials and considered this procedure to be well established. C. SKAU considers the Euclidean algorithm among the central pillars of ABEL'S impossibility proof.[22] In section 7.3.1, I illustrate how it, indeed,—together with the concept of irreducibility—played an important role in ABEL'S theory of equations.

In the very same paragraph, ABEL let

$$\sum_{u=0}^{\mu} t_u z^u = 0 \tag{6.8}$$

denote the factor of (6.7) of lowest degree with rational coefficients and continued with the following statement implicitly introducing irreducibility which ABEL had not used or defined thus far:

> "Let that equation [here (6.7)] be
>
> $$s_0 + s_1 z + s_2 z^2 \cdots + s_{k-1} z^{k-1} + z^k = 0$$
>
> and let
>
> $$t_0 + t_1 z + t_2 z^2 \cdots + t_{\mu-1} z^{\mu-1} + z^\mu$$
>
> be a factor of its first term [left hand side], where t_0, t_1 etc. are rational functions of $p, r_0, r_1 \ldots r_{n-1}$; then also
>
> $$t_0 + t_1 z + t_2 z^2 \cdots + t_{\mu-1} z^{\mu-1} + z^\mu = 0$$
>
> and it is clear, that it can be assumed to be impossible to find an equation of the same form of lower degree."[23]

Thus, certain roots of (6.7) would also be roots of (6.8), ABEL argued, and the μ roots of (6.8) would also be roots of $z^n - p = 0$. In the case $\mu = 1$, it would be easy to write z, i.e. $p^{\frac{1}{n}}$, as a rational function of t_0 and t_1, and thereby as a rational function of $p, r_0, \ldots, r_{n-1}$ from (6.6), contrary to the assumption imposed by theorem 2.

Since $\mu \geq 2$, ABEL let z and αz denote two distinct common roots of (6.8) and $z^n - p = 0$. When he inserted them into (6.8), he obtained

$$\sum_{u=0}^{\mu-1} t_u \left(\alpha^u - \alpha^\mu\right) z^u = 0 \tag{6.9}$$

---

22 (Skau, 1990, 54).

23 "*Die Gleichung sei*

$$s_0 + s_1 z + s_2 z^2 \cdots + s_{k-1} z^{k-1} + z^k = 0$$

und

$$t_0 + t_1 z + t_2 z^2 \cdots + t_{\mu-1} z^{\mu-1} + z^\mu$$

ein Factor ihres ersten Gliedes, wo t_0, t_1 etc. rationale Functionen von $p, r_0, r_1 \ldots r_{n-1}$ sind, so ist auch

$$t_0 + t_1 z + t_2 z^2 \cdots + t_{\mu-1} z^{\mu-1} + z^\mu = 0$$

und es ist klar, daß man es als unmöglich annehmen kann, eine Gleichung von niedrigerem Grade von der nemlichen Form zu finden." (N. H. Abel, 1826a, 71).

which was an equation of degree at most $\mu - 1$ having some of the roots of the irreducible (6.8) as its roots. In this connection, ABEL actually used the word "irreducible" for the first time (see the quotation below). Consequently, the polynomial of (6.9) would have to be the zero polynomial and a contraction had been reached:

> "But since the equation $z^\mu + t_{\mu-1}z^{\mu-1} \cdots = 0$ is irreducible, it must, since it is of the $\mu - 1$'st degree give
>
> $$\alpha^\mu - 1 = 0, \quad \alpha - \alpha^\mu = 0 \quad \ldots \quad \alpha^{\mu-1} - \alpha^\mu = 0;$$
>
> which is impossible."[24]

The contradicted assumption was that at least one coefficient among $r_0, \ldots, r_{n-1}$ was non-zero, and thus the result (lemma 1) had been demonstrated.

When ABEL considered n different values $y_1, \ldots, y_n$ of y resulting from substituting $\alpha^k p^{\frac{1}{n}}$ for $p^{\frac{1}{n}}$ in the expression (6.4) for y, he found that these all constituted roots of the equation when it was assumed to be algebraically solvable. Through laborious, albeit not very difficult, algebraic manipulations including a tacit application of LAGRANGE'S theorem 1 on resolvents, ABEL then demonstrated that if the equation was solvable, the coefficients $q_0, q_2, \ldots, q_{n-1}$ as well as $p^{\frac{1}{n}}$ would all depend rationally on these roots (and certain roots of unity, such as α). Thereby, he demonstrated that all components of a *top-level* algebraic expression solving a solvable equation were rational functions of the equation's roots. By considering any of these components and working downward in the hierarchy, ABEL demonstrated that this applied equally well to *any* component involved in the solution. Thus, he had proved the following explicitly formulated and very important auxiliary theorem, corresponding to RUFFINI'S open hypothesis.[25]

Theorem 3 *"When an equation can be solved algebraically, it is always possible to give to the root [solution] such a form that all the algebraic functions of which it is composed can be expressed by rational functions of the roots of the given equation."*[26]

The study of algebraic expressions which ABEL had conducted as a preliminary to his impossibility proof had produced two central results for the proof. Firstly, it had provided a hierarchy on the algebraic expressions based on the nesting of root extractions. Secondly, it had resulted in the auxiliary theorem stated just above, which

24 *"Da nun aber die Gleichung $z^\mu + t_{\mu-1}z^{\mu-1} \cdots = 0$ irreducibel ist, so muß sie, weil sie vom $\mu - 1^{ten}$ Grade ist, einzeln*

$$\alpha^\mu - 1 = 0, \quad \alpha - \alpha^\mu = 0 \quad \ldots \quad \alpha^{\mu-1} - \alpha^\mu = 0$$

geben; was nicht sein kann." (ibid., 72).

25 ABEL carried out his deductions in ignorance of RUFFINI'S work (see section 6.7).

26 *"Wenn eine Gleichung algebraisch auflösbar ist, so kann man der Wurzel allezeits eins solche Form geben, daß sich alle algebraische Functionen, aus welchen sie zusammengesetzt ist, durch rationale Functionen der Wurzeln der gegebenen Gleichung ausdrücken lassen."* (ibid., 73).

ensured ABEL that any expression which he was to encounter in the hierarchy of a solvable equation, would depend rationally on the roots of the given equation.

6.4 ABEL and the theory of permutations: the CAUCHY-RUFFINI theorem revisited

The second preliminary pillar of ABEL'S impossibility proof was made up of his studies of permutations and his proof of the CAUCHY-RUFFINI theorem describing the possible numbers of values of rational functions under permutations of their arguments. Prior to giving his proof of this central result, ABEL summarized much of what CAUCHY had done in his 1815-paper,[27] and in doing so ABEL took over CAUCHY'S notation and much of his terminology. But while CAUCHY had begun the process of liberating the substitutions from the expressions on which they acted, ABEL continued the tradition of LAGRANGE. Although he occasionally spoke of the "substitution" [*Vervandlung*] as an independent object, all his deductions concerned their actions on expressions.

> "Now let
>
> $$v\begin{pmatrix}\alpha\beta\gamma\delta\dots\\ abcd\dots\end{pmatrix}$$
>
> be the value, which an arbitrary function v takes, when therein x_a, x_b, x_c, x_d etc. are inserted instead of $x_\alpha, x_\beta, x_\gamma, x_\delta$ etc.; then it is clear that, when by $A_1, A_2 \dots A_\mu$ one denotes the different forms which $1, 2, 3, 4 \dots n$ can possibly take by interchanges of the exponents $1, 2, 3 \dots n$, the different values of v can be expressed as
>
> $$v\begin{pmatrix}A_1\\ A_1\end{pmatrix},\ v\begin{pmatrix}A_1\\ A_2\end{pmatrix},\ v\begin{pmatrix}A_1\\ A_3\end{pmatrix}\ \dots\ v\begin{pmatrix}A_1\\ A_\mu\end{pmatrix}."^{28}$$

With this notation, ABEL proved LAGRANGE'S theorem that the number of different values of the function v would be a divisor of $n!$. Next, he introduced the concept of recurring substitutions [*wiederkehrende Verwandlungen*] of *order* p, thereby replacing the word *degree* chosen by CAUCHY. In the 1840s, CAUCHY was to take up ABEL'S terminology on this point.[29] Through a counting argument based on what

27 (A.-L. Cauchy, 1815a).

28 "*Nun sei*

$$v\begin{pmatrix}\alpha\beta\gamma\delta\dots\\ abcd\dots\end{pmatrix}$$

der Werth, welchen eine beliebige Function v bekommt, wenn man darin x_a, x_b, x_c, x_d etc. statt $x_\alpha, x_\beta, x_\gamma, x_\delta$ etc. setzt, so ist klar, daß wenn man durch $A_1, A_2 \dots A_\mu$ die verschiedenen Formen bezeichnet, deren $1, 2, 3, 4 \dots n$ durch Verwechselung der Zeiger $1, 2, 3 \dots n$ fähig ist, die verschiedenen Werthe von v durch

$$v\begin{pmatrix}A_1\\ A_1\end{pmatrix},\ v\begin{pmatrix}A_1\\ A_2\end{pmatrix},\ v\begin{pmatrix}A_1\\ A_3\end{pmatrix}\ \dots\ v\begin{pmatrix}A_1\\ A_\mu\end{pmatrix}$$

ausgedrückt werden können." (N. H. Abel, 1826a, 74).

29 (Wussing, 1969, 67).

later was termed the *pigeon hole principle*,[30] ABEL proved that if v took fewer than p different values, and $\binom{A_1}{A_m}$ was a recurring substitution of order p, some two among the p values

$$v\begin{pmatrix} A_1 \\ A_m \end{pmatrix}^0, \dots, v\begin{pmatrix} A_1 \\ A_m \end{pmatrix}^{p-1}$$

had to be identical,

$$v\begin{pmatrix} A_1 \\ A_m \end{pmatrix}^R = v$$

for some R. At this point, the argument was hampered by a typographical error, which might have rendered it unintelligible to some readers (see section 6.9). By tacit use of the Euclidean algorithm, ABEL found that it would be possible to determine integers α, β such that

$$R\alpha = 1 + p\beta$$

proving

$$v\begin{pmatrix} A_1 \\ A_m \end{pmatrix} = v.$$

The argument thus amounted to proving that if v took fewer than p values under permutations, v would be invariant under any substitution of order p (p a prime). All these steps had been taken by CAUCHY, and ABEL simply filled in the last details and supplied a proof in his shorter presentational style.

As CAUCHY had done, ABEL subsequently proved that any 3-cycle was the product of two recurring p-cycles and that any 3-cycle could be decomposed into 2-cycles. Thereby, he had demonstrated that if the number of values of v was less than the largest prime $p \leq n$ it had to be either 1 or 2. In the process, he also found that if the function had two values these would correspond to odd and even numbers of transpositions. The result can be summarized in the following theorem.

Theorem 4 *Let v be a function of n quantities $x_1, \dots, x_n$. Let the number of values which v takes under all permutations of $x_1, \dots, x_n$ be denoted by λ and let p denote the largest prime which is less than or equal to n. If $\lambda < p$ then $\lambda \in \{1, 2\}$.* □

In his paper, ABEL had — thus far — obtained the following two preliminary results:

1. Based on a hierarchic classification of algebraic expressions, the concept of irreducibility, and the Euclidean algorithm, ABEL had found that *any radical occurring in a supposed algebraic solution of an equation depended rationally on the roots of that equation* (see theorem 3).

30 Also known as the *Dirichlet boxing-in principle*.

2. ABEL had inherited a result, the CAUCHY-RUFFINI theorem, which *limited the possible numbers of values of rational functions under permutations of their arguments* (see theorem 4). Applied to the quintic, he observed that the result proved that no function of five quantities could exist which took on three or four different values when its arguments were interchanged. ABEL proceeded by exploring the remaining cases, i.e. function of five quantities, which took on two or five different values.

Although ABEL chose to present his detailed studies of particular cases before linking these two preliminaries, I have chosen to provide this logical link in the following section.

6.5 Permutations linked to root extractions

A very central link between the two preliminaries described above was provided toward the end of ABEL'S argument.[31] There, he linked the number of values taken by a function v under all permutations of its arguments to the minimal degree of a polynomial equation which had v as a root and symmetric functions as coefficients. This equation is the irreducible equation corresponding to v and was later to take a very central position in his general theory of solubility (see chapter 8).

ABEL let v designate any rational function of $x_1, \ldots, x_n$ which took on m different values $v_1, \ldots, v_m$ under permutations of the quantities $x_1, \ldots, x_n$. By this, he meant that the function v had the m different formal appearances $v_1, \ldots, v_m$ when its arguments were permuted. Of course, v itself was identical to one of these values but as the typesetting suggests, ABEL distinguished the values from the function. ABEL formed the equation

$$\prod_{k=1}^{m} (v - v_k) = \sum_{k=0}^{m} q_k v^k = 0,$$

and claimed that the coefficients $q_0, \ldots, q_m$ were symmetric functions of the quantities $x_1, \ldots, x_n$. ABEL gave no reference and no proof of this assertion, which is now an easy consequence of one of LAGRANGE'S theorems concerning resolvents (theorem 1).

ABEL also maintained that it was impossible to express v as a root of any equation of lower degree with symmetric coefficients. He proved this through a *reductio ad absurdum* by assuming that

$$\sum_{k=0}^{\mu} t_k v^k = 0 \tag{6.10}$$

was such an equation where the t_k were symmetric, and $\mu < m$. If v_1 was a root of (6.10) it would be possible to divide the polynomial in (6.10) by $(v - v_1)$ and obtain

31 (N. H. Abel, 1826a, 81–82)

another polynomial P_1,

$$0 = \sum_{k=0}^{\mu} t_k v^k = (v - v_1)\, P_1.$$

When the quantities $x_1, \ldots, x_n$ were permuted, it followed that the equation (6.10) would be transformed into

$$\sum_{k=0}^{\mu} t_k v_u^k = 0$$

for some u since the t_k's were symmetric. Since v_u was therefore a root of (6.10), division in (6.10) by $(v - v_u)$ was possible. Thus, ABEL could decompose (6.10) in m different ways corresponding to each of the values of v

$$0 = \sum_{k=0}^{\mu} t_k v^k = (v - v_u)\, P_u \text{ for } 1 \leq u \leq m.$$

Because the formal values $v_1, \ldots, v_\mu$ were distinct, it followed that $\mu = m$ and ABEL had reached a contradiction.

The corner stone of ABEL'S argument was the demonstration that if v was a root of the equation (6.10), any value v_u which v might take on under permutations of $x_1, \ldots, x_n$ would also be a root of that equation. He summarized the connection thus provided in the following way:

> "When a rational function of multiple quantities has m different values, then it will always be possible to find an equation of degree m, the coefficients of which are symmetric functions, and which has all the values [of v] as roots; but it is not possible to find an equation of the described form of lower degree which has one or more of these values as roots."[32]

In this way, ABEL linked the rather new concept of number of values under permutations to the older one of number of values of expressions of the form $\sqrt[n]{y}$. It had long been accepted that square roots were two-valued, cubic roots three valued etc., and ABEL thus connected these two apparently different ways of counting the number of values of an algebraic expression. The following points summarize ABEL'S important applications of this correspondence:

1. If $v = v(x_1, \ldots, x_n)$ is a rational function which takes the m different values $v_1, \ldots, v_m$ under permutations of $x_1, \ldots, x_n$, an *irreducible equation* with symmetric functions $t_0, \ldots, t_m$ of $x_1, \ldots, x_n$ as coefficients can be associated with v,

$$\prod_{k=1}^{m} (v - v_k) = \sum_{k=0}^{m} t_k v^k = 0.$$

32 "*Wenn eine rationale Function mehrerer Größen m verschiedene Werthe hat, so läßt sich allezeit eine Gleichung vom Grade m finden, deren Coefficienten symmetrische Functionen sind, und welche jene Werthe zu Wurzeln haben; aber es ist nicht möglich eine Gleichung von der nämlichen Form von niedrigerem Grade aufzustellen, welche einen oder mehrere jener Werthe zu Wurzeln hat.*" (ibid., 82).

2. On the other hand, if a rational function $v = v(x_1, \ldots, x_n)$ satisfies an equation of degree m with symmetric functions of $x_1, \ldots, x_n$ as its coefficients, the function v must have *at most* m different values under permutations of $x_1, \ldots, x_n$. If the equation is furthermore known to be irreducible, v must take on *exactly* m values. Thus, to the relation $v = \sqrt[m]{R}$ corresponds an equation of degree m with symmetric coefficients.

6.6 ABEL's combination of results into an impossibility proof

The fourth component of ABEL'S impossibility proof concerned detailed and highly explicit, "computational" investigations of functions of five quantities having two or five values. ABEL sought to reduce all such functions to a few standard forms, an approach completely in line with the classification which opened his paper. These investigations have been subjected to quite a lot of criticism, rethinking, and eventually incorporation into a broader theory, all of which will be dealt with in subsequent chapters.

6.6.1 Careful studies of functions of five quantities

The CAUCHY-RUFFINI theorem described in sections 5.6 and 6.4 had ruled out the existence of functions of five quantities which had three or four different values when their arguments were permuted. The remaining relevant (non-symmetric) cases were concerned with functions having two or five values. In the case of two-valued functions, ABEL reduced all such functions to the alternating one which CAUCHY had also studied; and when the function had five values, ABEL could write it as a fourth degree polynomial in one of the variables with coefficients symmetric in the remaining four.

Two-valued functions. In order to describe functions of five quantities having two values under permutations, ABEL let v denote such a function of $x_1, \ldots, x_5$ having the two values v_1, v_2. Furthermore, he let v' denote a second such function (with the values v_1' and v_2') and formed two further functions

$$t_1 = v_1 + v_2, \text{ and}$$
$$t_2 = v_1 v_1' + v_2 v_2'.$$

ABEL claimed that the functions t_1 and t_2 were both symmetric.[33] The two functions v and v' were related through these symmetric functions by

$$v_1 = \frac{t_1 v_2' - t_2}{v_2' - v_1'}.$$

Then, ABEL chose for v_1' the alternating function s

$$v_1' = s = \prod_{1 \leq i < j \leq 5} \left(x_i - x_j\right),$$

and concluded that $v_2' = -v_1'$ and, therefore,

$$v_1 = \frac{1}{2} t_1 + \frac{t_2}{2s^2} s.$$

By these simple manipulations, ABEL had obtained the *standard form*

$$p + qs \text{ with } p \text{ and } q \text{ symmetric}$$

of all functions of five quantities having two values under permutations. In his own words, he concluded

> "that any function of five quantities which has two different values can be expressed as $p + q.\rho$ where p and q are two symmetric functions and
>
> $$\rho = (x_1 - x_2)(x_1 - x_3)\dots(x_4 - x_5)."^{34}$$

As is evident from the computations, the deduction is valid for any function of *any* number of quantities which takes on only two values under permutations.

Five-valued functions symmetric under permutations of four quantities. For functions of five quantities having five different values under permutations, the situation was much more complicated. ABEL chose to study such functions by means of functions of five quantities $x_1, \dots, x_5$ which were symmetric under permutations of the last four quantities. He reduced such functions v to the form

$$v = \sum_{u=0}^{4} r_u x_1^u \tag{6.11}$$

where r_u were symmetric functions of $x_2, \dots, x_5$ by the following argument.

[33] Although ABEL was not explicit about this point, t_1 and t_2 are both symmetric because any two functions having two values are *semblables* in the sense of LAGRANGE, i.e. they are altered in the same ways by the same permutations. Thus, the values of t_1 and t_2 are partitioned into classes corresponding to odd and even numbers of transpositions.

[34] *"daß jede Function von fünf Größen, welche zwei verschiedene Werthe hat, durch $p + q.\rho$ ausgedrückt werden kann, wo p und q zwei symmetrische Functionen sind und*

$$\rho = (x_1 - x_2)(x_1 - x_3)\dots(x_4 - x_5)$$

ist." (N. H. Abel, 1826a, 78).

First, ABEL tacitly applied an equivalent to *Waring's formulae* (see section 5.2.4) to express v rationally in x_1 and the elementary symmetric functions $A_0, \ldots, A_3$ occurring as coefficients in the equation

$$0 = \prod_{k=2}^{5} (x - x_k) = x^4 + A_3 x^3 + A_2 x^2 + A_1 x + A_0.$$

To ABEL, the calculations to obtain this were straightforward and not worth mentioning. When ABEL factorized the general quintic as

$$0 = \prod_{k=1}^{5} (x - x_k) = (x - x_1) \sum_{k=0}^{4} A_k x^k = \sum_{k=0}^{5} a_k x^k,$$

he found that the coefficients $A_0, \ldots, A_4$ depended rationally on $a_0, \ldots, a_5$. Consequently, v could also be expressed rationally in x_1 and $a_0, \ldots, a_5$ as

$$v = \frac{t}{\phi(x_1)},$$

where both t and $\phi(x_1)$ were entire functions of $x_1, a_0, \ldots, a_5$. By inserting the other roots $x_2, \ldots, x_5$ for x_1 in $\phi(x_1)$, ABEL obtained another four entire functions in which the coefficients were symmetric functions of $x_1, \ldots, x_5$. When ABEL multiplied both numerator and denominator by $\prod_{k=2}^{5} \phi(x_k)$,[35] tacitly used LAGRANGE'S theorem (1) on resolvents, and reduced the degree according to the relationship imposed by the quintic equation, he obtained v in the desired form of a fourth degree polynomial in x_1.

Five-valued functions in general. In order to obtain a standard form of *all* functions of five quantities having five values, ABEL relied on an extensive investigation of particular cases. Denoting by v any function of five quantities, which took on the five values $v_1, \ldots, v_5$ when all its arguments were permuted, ABEL introduced an indeterminate m and formed the function $x_1^m v$. When only $x_2, \ldots, x_5$ were permuted, this function would attain its values from the list

$$x_1^m v_1, \ldots, x_1^m v_5. \tag{6.12}$$

ABEL let μ denote the number of different values of $x_1^m v$ when $x_2, \ldots, x_5$ were permuted in all possible ways. He then considered the different cases corresponding to different values of μ in detail and either eliminated them through a *reductio ad absurdum* or reduced them to the standard form (6.11). Throughout this procedure, it is important to keep in mind which quantities are permuted, and ABEL was not always very explicit.

35 A similar argument resembling multiplying the denominator by its conjugate is described in section 6.3.2.

The first case, in which $\mu = 5$, was eliminated, ABEL said, since that assumption would require all the values (6.12) to be different. Considering transpositions of the form $\binom{1k}{k1}$, ABEL found that $x_1^m v$ would take on another 20 different values, which would also be distinct from those in (6.12).[36] Thus in total, $x_1^m v$ would take on 25 different values, and since 25 did not divide $5! = 120$ a contradiction had been obtained.

Secondly, ABEL assumed $\mu = 1$ and found that the function v would only take on one value under all permutations of $x_2, \ldots, x_5$ and thus the case had been reduced to the one above, giving v in the form (6.11).

Thirdly, for $\mu = 4$, the function $x_1^m v$ would take on the different values $x_1^m v_1, \ldots, x_1^m v_4$, and the function v would take on the values $v_1, \ldots, v_4$ under permutations of $x_2, \ldots, x_5$. Thus, the function

$$v_1 + v_2 + v_3 + v_4$$

was a symmetric function of $x_2, \ldots, x_5$, and therefore of the form (6.11). Writing v_5 as

$$v_5 = (v_1 + \cdots + v_5) - (v_1 + \cdots + v_4),$$

ABEL concluded that the symmetric function $v_1 + \cdots + v_5$ could be incorporated in the constant term of (6.11), and therefore v_5 itself was of the form (6.11).

These first three cases were not very difficult to follow. However, the remaining two cases were subjected to much criticism from his contemporaries (see section 6.9). In a letter to the Swiss mathematician E. J. KÜLP (⋆1801),[37] who in a private correspondence had asked for clarifications, ABEL described a refined argument, which I have incorporated in the present description.

The fourth case, in which $\mu = 2$, reduced to the well known situation of a function having only two values under permutations. ABEL concluded that since $x_1^m v$ took on the two values $x_1^m v_1$ and $x_1^m v_2$ under all permutations of $x_1, \ldots, x_5$, the function v would take on only two values, say v_1 and v_2, when only $x_2, \ldots, x_5$ were permuted. Letting

$$\phi(x_1) = v_1 + v_2, \tag{6.13}$$

ABEL found that $\phi(x_1)$ was symmetric under permutations of $x_2, \ldots, x_5$ and thus of the form (6.11). The expression $\phi(x_1)$ had to take on the five different values $\phi(x_1), \ldots, \phi(x_5)$ under all permutations of $x_1, \ldots, x_5$ since only transpositions of the form $\binom{1k}{k1}$ effected the value of ϕ.

36 To see this, it suffices to realize that any permutation σ of five quantities can be written as a product of a permutation $\tilde{\sigma}$ fixing the symbol 1 and a transposition τ of the form $(1k)$. Then, if an application of σ to v gives v_u, it follows that

$$(x_1^m v) \circ \sigma = (x_1^m \circ \tilde{\sigma} \circ \tau)(v \circ \sigma) = x_{\tilde{\sigma}(k)}^m v_u.$$

37 (Abel→Külp, Paris, 1826/11/01. In Hensel, 1903, 237–240).

In the published paper, ABEL involved himself in a difficult *reductio ad absurdum* to rule out this case. However, because the proof given in the letter to KÜLP is more detailed, it is presented before the differences between the two proofs are sketched.

Besides the symmetric function (6.13), there is another obvious symmetric function under permutations of $x_2, \dots, x_5$,

$$f(x_1) = v_1 v_2.$$

The function $f(x_1)$ is of the form (6.11). ABEL introduced

$$(z - v_1)(z - v_2) = z^2 - \phi(x_1) z + f(x_1) = R, \tag{6.14}$$

and found that it must divide

$$\prod_{k=1}^{5} (z - v_k) = \sum_{k=0}^{5} p_k z^k = R',$$

in which $p_0, \dots, p_5$ were symmetric functions of $x_1, \dots, x_5$ by the theorem 1 on LAGRANGE resolvents. Since R' was unaltered by transpositions $\binom{1u}{u1}$ it followed that all the polynomials derived from (6.14) through this transposition,

$$z^2 - \phi(x_u) z + f(x_u) = \rho_u \text{ for } 1 \leq u \leq 5,$$

would divide R'. However, as R' was a polynomial of the fifth degree, some polynomials among $\rho_1, \dots, \rho_5$ had to share a common factor. Assuming that ρ_1 and ρ_2 had a factor in common ABEL concluded

$$z = \frac{f(x_1) - f(x_2)}{\phi(x_1) - \phi(x_2)}.$$

This value of z must be one of the values of v and thus the left hand side had five different values. However, the right hand side had 10 different values, and a contradiction had been reached, ruling out the case $\mu = 2$.

The published argument in *Beweis der Unmöglichkeit*[38] followed the one given in the letter to KÜLP until ABEL had demonstrated that

$$\phi(x_1) = v_1 + v_2 = \sum_{k=0}^{4} r_k x_1^k$$

and had recognized that ϕ had five different values under permutations of $x_1, \dots, x_5$. Whereas the proof in the letter then explicitly constructed the polynomials R and R', the original argument was much more roundabout. Substituting any one x_k of $x_2, \dots, x_5$ for x_1, ABEL obtained the value $\phi(x_k)$ as the sum of two of the five values of v.

38 (N. H. Abel, 1826a).

> "When x_1 is sequentially interchanged with x_2, x_3, x_4, x_5 one obtains
>
> $$\begin{aligned} v_1 + v_2 &= \phi(x_1) \\ v_2 + v_3 &= \phi(x_2) \\ &\vdots \\ v_{m-1} + v_m &= \phi(x_{m-1}) \\ v_m + v_1 &= \phi(x_m), \end{aligned}$$
>
> where m is one of the numbers $2, 3, 4, 5$."[39]

The m here is *not* the indeterminate introduced earlier, but a number introduced for this particular purpose. It is unclear to me, and probably also a point of concern to ABEL'S contemporaries, how this set of equations could be put on the circular form above. But once it had been done (assuming it could be done) it was a simple matter of contradicting the different assumptions for m. If $m = 2$, it followed that $\phi(x_1) = \phi(x_2)$ and ϕ could not have five values after all. If $m = 3$, ABEL deduced that

$$2v_1 = \phi(x_1) - \phi(x_2) + \phi(x_3),$$

whereby a contradiction was reached because the left hand side had 5 values, whereas the right hand side had $\frac{5\times4}{2} \times 3 = 30$ values. In a similar way, ABEL claimed he could prove that $m = 4$ or $m = 5$ could be ruled out as well,[40] which in turn proved that μ could not be equal to 2.

ABEL'S argument presented in the paper depended on a rather obscure sequence of functions and was severely criticized. The proof which ABEL gave in his letter to KÜLP avoided this central step and was much clearer. I conjecture that KÜLP had questioned the sequence of equations, and that ABEL had subsequently developed his new proof which he presented as an answer; I have no indication that ABEL had possessed the proof presented to KÜLP when he wrote his paper.

The final case, $\mu = 3$, was ruled out in the same way as $\mu = 2$ above. ABEL found that if $\mu = 3$, the function

$$v_1 + v_2 + v_3$$

would be symmetric under permutations of $x_2, \ldots, x_5$ and therefore

$$v_4 + v_5 = (v_1 + \cdots + v_5) - (v_1 + \cdots + v_3)$$

---

39 "*Vertauscht man der Reihe nach x_1 mit x_2, x_3, x_4, x_5, so erhält man*

$$\begin{aligned} v_1 + v_2 &= \phi(x_1) \\ v_2 + v_3 &= \phi(x_2) \\ &\vdots \\ v_{m-1} + v_m &= \phi(x_{m-1}) \\ v_m + v_1 &= \phi(x_m), \end{aligned}$$

wo m eine der Zahlen $2, 3, 4, 5$ ist." (ibid., 80).

40 HOLMBOE supplied the expressions (see section 6.9.1).

could be written in the form (6.11) as he had done in the case $\mu = 4$ above. However, ABEL had just demonstrated in the case $\mu = 2$ that no sum of two values of v could have five values under permutations of $x_1, \ldots, x_5$, whereby he reached a contradiction.

The core of ABEL'S description of functions of five quantities having five values under permutations of these consisted of two parts:

1. A direct manipulation based on LAGRANGE'S theorem 1 on resolvents, resulting in a proof that any function of five quantities $x_1, \ldots, x_5$ which is unaltered by permutations of four of these, $x_2, \ldots, x_5$, has the form of a fourth degree polynomial (6.11).

2. A meticulous study of the particular cases in which any function of five quantities which has five values under permutations of $x_1, \ldots, x_5$ is either contradicted or proved to be of the form (6.11), too.

At the conclusion of his investigations, ABEL had added a complete description of functions of five quantities having five values to the one he had obtained, in case the function had only two values. Thereby, he had obtained workable standard forms for all non-symmetric rational functions which could be involved in a supposed solution to the general quintic. All he lacked was to put the pieces together to obtain the impossibility proof.

6.6.2 The goal in sight

To combine his preliminary results into a proof of the algebraic insolubility of the general quintic

$$x^5 + a_4x^4 + a_3x^3 + a_2x^2 + a_1x + a_0 = 0, \tag{6.15}$$

ABEL assumed that an algebraic solution could be obtained. The auxiliary theorem 3 obtained earlier ensured him that he could assume that any subexpression occurring therein would be a rational function of the roots $x_1, \ldots, x_5$ of the equation (6.15). Since the quintic could not be solved by a rational expression alone, some root extraction had to occur. ABEL focused his attention on the algebraic expression of the first order in the supposed solution. Thus, he dissected the solution from the inside by focusing on this innermost non-rational function. According to ABEL'S classification, an algebraic expression of the first order contained only rational functions of the coefficients $a_0, \ldots, a_4$ and roots of the form $\sqrt[m]{R}$ where m was a prime and R was a rational function of $a_0, \ldots, a_4$. Such roots would satisfy the equation

$$v^m - R = 0, \tag{6.16}$$

and v would have to be a rational function of the roots $x_1, \ldots, x_5$. His earlier results showed that it was impossible to diminish the degree of the equation. Therefore, the link between root extractions and permutations ensured him that the function v would take on m values under all permutations of $x_1, \ldots, x_5$. Since m was a prime and had to divide 5! by LAGRANGE'S theorem, ABEL argued, the only possibilities were that m equaled 2, 3, or 5. And since no function of five quantities could have three values under permutations by the CAUCHY-RUFFINI theorem, ABEL ruled out this possibility. The two remaining cases were subsequently both brought to contradictions.

The innermost root extraction could not be a fifth root. In the simplest case, corresponding to $m = 5$, the function v had to have the form of a fourth degree polynomial, as ABEL had demonstrated:

$$v = \sqrt[5]{R} = \sum_{k=0}^{4} r_k x_1^k.$$

Through a *process of inversion* of polynomials in which the quintic equation (6.15) was used to lower the degree, ABEL found that

$$x_1 = \sum_{k=0}^{4} s_k R^{\frac{k}{5}},$$

where $s_0, \ldots, s_4$ were symmetric functions of $x_1, \ldots, x_5$. Furthermore, by use of basic properties of primitive roots of unity, he obtained

$$s_1 R^{\frac{1}{5}} = \frac{1}{5}\left(x_1 + \alpha^4 x_2 + \alpha^3 x_3 + \alpha^2 x_4 + \alpha x_5\right),$$

where α was a primitive fifth root of unity. The left hand side of the equation was a solution to a fifth degree equation, and thus had (at most) five different values, whereas the right hand side was formally altered by any permutation of $x_1, \ldots, x_5$ and thus had $5! = 120$ different values. This ruled out the case $m = 5$, and the innermost root extraction could not be a fifth root.

The innermost root extraction could not be a square root, neither. ABEL brought the case $m = 2$ to a contradiction in a similar way, although it involved studying expressions of the second order as well. He knew that the root would have to be of the form

$$\sqrt{R} = p + qs,$$

and the other value under permutations would be

$$-\sqrt{R} = p - qs.$$

Subtracting these two, ABEL concluded that $\sqrt{R}$ was of the form

$$\sqrt{R} = qs,$$

and he saw that any rational combination of such root extractions would continue to be of the same form. Therefore, any algebraic expression of the first order contained in the solution would have to be of the form

$$\alpha + \beta s$$

where α, β were symmetric functions. ABEL observed that such functions were not powerful enough to solve the general quintic (6.15), and found that such a solution would necessarily contain root extractions of the form

$$\sqrt[m']{\alpha + \beta s},$$

where m' was a prime and $\beta \neq 0$. ABEL knew that such a root, say v, was a rational function of $x_1, \ldots, x_5$. Among the values of v obtained by permuting $x_1, \ldots, x_5$ he found that two were of particular interest,

$$\begin{aligned} v_1 &= \sqrt[m']{\alpha + \beta s}, \text{ and} \\ v_2 &= \sqrt[m']{\alpha - \beta s}. \end{aligned}$$

When these two were multiplied,

$$\gamma = v_1 v_2 = \sqrt[m']{\alpha^2 - \beta^2 s^2},$$

the expression under the root sign was a symmetric function.

At this point, ABEL again considered two individual cases: either γ, itself, was a symmetric function, or it was not. In case γ was a non-symmetric function, it would be a first order algebraic expression, and ABEL had proved that for such expressions the value of m' would have to be 2. This led to a contradiction, since v then had four values under permutations of $x_1, \ldots, x_5$ because $\beta \neq 0$. However, by the CAUCHY-RUFFINI theorem no such function could exist. Consequently, $\sqrt[m']{\alpha^2 - \beta^2 s^2}$ would have to be a symmetric function. By adding v_1 and v_2, ABEL obtained a function p,

$$p = v_1 + v_2 = R^{\frac{1}{m'}} + \frac{\gamma}{R} R^{\frac{m'-1}{m'}}$$

with $R = \alpha + \beta s$. He studied the values of p resulting from substituting $\alpha^k R^{\frac{1}{m'}}$ for $R^{\frac{1}{m'}}$ and demonstrated that p had to have m' values under permutations of $x_1, \ldots, x_5$. But since $m' = 2$ had been ruled out, he concluded that $m' = 5$, and the second root extraction counted from the inside had to be a fifth root. This time ABEL obtained

$$t_1 R^{\frac{1}{5}} = \frac{1}{5}\left(x_1 + \alpha^4 x_2 + \alpha^3 x_3 + \alpha^2 x_4 + \alpha x_5\right)$$

in which t_1 was a symmetric function of the roots. The left hand side was the root of an irreducible equation of the tenth degree,[41] thus having 10 values under permutations.

41 With $y = t_1 R^{\frac{1}{5}}$ and $R = \alpha + \beta s$, the equation was $\left(y^5 - t_1^5 \alpha\right)^2 - t_1^{10} \beta^2 s^2 = 0$ in which the coefficients are symmetric functions.

The right hand side had a complete 120 values since none of the roots $x_1, \dots, x_5$ could be interchanged without altering the value of the expression. Thus, a contradiction had again been reached.

The line of ABEL'S argument in knitting together his preliminary investigations can be divided into the following steps:

1. The innermost root extraction in any supposed solution to the general quintic had to be either a fifth root ($m = 5$) or a square root ($m = 2$); any other possibilities were ruled out by the CAUCHY-RUFFINI theorem.

2. The innermost root extraction could not be a fifth root ($m \neq 5$) since this was brought to a contradiction by comparing the number of values of certain expressions.

3. Thus, the innermost root extraction had to be a square root ($m = 2$). Then the second innermost root extraction was taken into consideration. Its degree has been denoted m'.

4. The second innermost root extraction, too, had to be of degree either two ($m' = 2$) or five ($m' = 5$).

5. In case the second innermost root extraction was a square root, a function having four values under permutations would be obtained, from which a contradiction could be reached. Thus $m' \neq 2$.

6. Therefore the second innermost root extraction had to be a fifth root, but this, too, was brought to a contradiction in a way similar to step 2 above.

7. Consequently, no algebraic solution to the general quintic could exist, and the algebraic insolubility had been demonstrated.

Apparently, the argument carried out applied to the quintic equation alone. However, ABEL claimed that it also proved the insolubility of all general higher degree equations.

> "From this [the insolubility of the general quintic] it follows immediately that it is also impossible generally to solve equations of degrees above the fifth. Therefore the equations which can be generally solved are only of the four first degrees."[42]

Although he produced no further evidence, ABEL probably thought of a proof by the following argument. If the roots of the general sixth degree equation

$$x^6 + a_5x^5 + a_4x^4 + a_3x^3 + a_2x^2 + a_1x + a_0 = 0$$

[42] *"Daraus folgt unmittelbar weiter, daß es ebenfalls unmöglich ist, Gleichungen von höheren als dem fünften Grade allgemein aufzulösen. Mithin sind die Gleichungen, welche sich algebraisch allgemein auflösen lassen, nur die von den vier ersten Graden."* (N. H. Abel, 1826a, 84).

could be expressed by any algebraic formula, this formula would also provide the solution to the general fifth degree equation by inserting $a_0 = 0$ in that formula. Central to the argument is that the supposed general solution formula for sixth degree equations not only produces a single root, but can somehow be made to produce *all* the roots of the equation. This was a recurring idea in ABEL'S work on the theory of equations (see for instance theorem 10), which linked the concepts of satisfiability (a single root could be found) and solubility (all roots could be found).

6.7 ABEL and RUFFINI

According to ABEL and his commentators, ABEL was unaware of the proofs published by RUFFINI when he published his proofs of the impossibility result in 1824 and 1826.[43] Since questions of priority are a frequently recurring theme in the history of mathematics, this independence of results is noticed by most biographers of ABEL.[44] It is my firm conviction — based on the mathematical contents of his proof — that ABEL developed his proof independently of RUFFINI. However, the primary sources of information on ABEL'S independence of RUFFINI are limited. The only mention of RUFFINI made by ABEL is in his notebook entry on the theory of solubility (see chapter 8), in the introduction to which he described RUFFINI'S proof:

> "The first person, and if I am not mistaken, the only one prior to me, who has tried to prove the impossibility of the algebraic solution of the general equations, is the geometer *Ruffini*; but his memoir is so complicated that it is very difficult to judge the validity of his reasoning. It seems to me that his reasoning is not always satisfying. I think that the proof I gave in the first issue of this journal [CRELLE'S *Journal*] leaves nothing to be desired as to rigor, but it does not have all the simplicity of which it is susceptible. I have reached another proof based on the same principles, but more simple, in trying to solve a more general problem."[45]

The answer derived from "trying to solve a more general problem" was never made available in print, though. As is documented in chapter 8, such an answer was, indeed, indirectly obtainable from ABEL'S more general research on algebraic solubility which even produced an explicit example of a particular *special* equation which could not be solved.

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43 (N. H. Abel, 1824b; N. H. Abel, 1826a)

44 See for instance (Bjerknes, 1885, 22–23), (Bjerknes, 1930, 23), (Ore, 1954, 89–90), (Ore, 1957, 125), and (Stubhaug, 1996, 352–353).

45 "*Le premier, et, si je ne me trompe, le seul qui avant moi ait cherché à démontrer l'impossibilité de la résolution algébrique des équations générales, est le géomètre* Ruffini*; mais son mémoire est tellement compliqué qu'il est très difficile de juger de la justesse de son raisonnement. Il me paraît que son raisonnement n'est pas toujours satisfaisant. Je crois que la démonstration que j'ai donnée dans le premier cahier de ce journal, ne laisse rien à désirer du côté de la rigueur; mais elle n'a pas toute la simplicité dont elle est susceptible. Je suis parvenu à une autre démonstration, fondée sur les mêmes principes, mais plus simple, en cherchant à résoudre un problème plus général.*" (N. H. Abel, [1828] 1839, 218).

The notebook has been dated to 1828 by P. L. M. SYLOW (1832–1918) — a date which implies that ABEL disclosed his knowledge of RUFFINI only after returning to Christiania.[46] It is most likely that ABEL learned about RUFFINI during his European tour, and two instances are of particular importance. During his stay in Vienna in April and May 1826, ABEL became acquainted with the local astronomers K. L. VON LITTROW (1811–1877) and A. VON BURG (1797–1882). In the first volume of their journal *Zeitschrift für Physik und Mathematik*, occurring while ABEL was in town, an anonymous paper on the theory of equations was published.[47] The author,[48] who was inspired by ABEL'S proof and praised it highly, reviewed RUFFINI'S proof. Therefore it is not unlikely that ABEL learned of RUFFINI'S proof from his Viennese connections.[49]

Once in Paris, ABEL took on the duty of writing unsigned reviews for FERRUSAC'S *Bulletin des sciences mathématiques, astronomiques, physiques et chimiques* of papers published in CRELLE'S *Journal für die reine und angewandte Mathematik*. We know from one of ABEL'S letters that he, himself, wrote the review of his *Beweis der Unmöglichkeit* which gave a short exposition of the flow of the proof.[50] However, appended to the review was a short note by the editor, J. F. SAIGEY (1797–1871),[51] which drew attention to the works of RUFFINI.[52] SAIGEY mentioned CAUCHY'S favorable review of RUFFINI'S treatise and made it clear that CAUCHY'S view was not universally accepted:

> "Other geometers have not understood this demonstration and some have made the justified remark that by proving too much, Ruffini could not prove anything in a satisfactory manner; to be sure it was not known how an equation of the fifth degree, e.g., could not have *transcendental* roots, equivalent to infinite series of algebraic terms, since one demonstrates that every equation of odd degree necessarily has *some* root. By a more profound analysis, M. Abel proves that such roots cannot exist *algebraically*; but he has not solved the question of the existence of transcendental roots in the negative."[53]

Thus, at two instances in 1826, ABEL had been in close contact with journals, in which his result was linked to that of RUFFINI. A third possible source of information

46 (L. Sylow, 1902, 16).
47 (Anonymous, 1826).
48 Or authors? Unlike the review in FERRUSAC'S *Bulletin* (see below), ABEL is not likely to be the author, himself.
49 See (Ore, 1957, 125).
50 (Abel→Holmboe, Paris, 1826/10/24. N. H. Abel, 1902a, 44). The paper reviewed is, of course, (N. H. Abel, 1826a).
51 (Stubhaug, 1996, 589).
52 (N. H. Abel, 1826c, 353–354).
53 "*D'autres géomètres avouent n'avoir pas compris cette démonstration, et il y en a qui ont fait la remarque très-juste que Ruffini en prouvant trop pourrait n'avoir rien prouvé d'une manière satisfaisante; en effet, on ne conçoit pas comment une équation du cinquième degré, par exemple, n'admettrait pas de racines* transcendantes*, qui équivalent à des séries infinies de termes algébriques, puisqu'on démontre que toute équation de degré impair a nécessairement une racine* quelconque. *M. Abel, au moyen d'une analyse plus profonde, vient de prouver que de telles racines ne peuvent exister* algébriquement*; mais il n'a pas résolu négativement la question de l'existence des racines transcendantes.*" (Saigey in ibid., 354).

on RUFFINI'S research was, of course, CAUCHY whom ABEL met in Paris without any traceable interaction taking place.[54]

Although the primary information on how ABEL came to know of RUFFINI'S proofs is rather sparse, I find further support for the assumption of independence in the mathematical technicalities as documented in the preceding sections. RUFFINI'S and ABEL'S differences in notation and approach to permutations, ABEL'S definition of algebraic expressions and his careful proof of the auxiliary theorems describing them all suggest to me that ABEL'S deduction was a tailored argument for the impossibility, independent of any earlier such proofs. The common inspiration from LAGRANGE, which permeated both the works of RUFFINI and ABEL, should be evident enough to account for similarities in studying the blend of equations and permutations.

6.8 Limiting the class of solvable equations

At a conceptual level, ABEL'S proof that the general quintic could not be solved algebraically was more than just another proof added to the body of mathematics. For centuries, mathematical intuition had suggested that an algebraic solution to the fifth degree equation should exist but probably be difficult to find. ABEL had demonstrated that any supposed solution carried with it an internal contradiction and thus the result not only made the belief in general algebraic solubility tremble, it completely destroyed it.

In negating the existence of an algebraic solution, ABEL provided an instance of a *negative* result — negative in the sense that it contradicted contemporary intuition. Similar counter intuitive results abound in mathematics in the period as a result of a fundamental transition toward concept based mathematics.[55]

The outspoken reactions of the mathematical community to ABEL'S impossibility proofs can be divided in three. Some mathematicians, often belonging to the older generation or the loosely institutionalized amateur mathematicians, protested against the result and held both the statements and their proofs to be flawed. To these mathematicians, the break with their established intuition forced them into their rejection. Others accepted the result but provided refinements of the proofs and their assumptions. And yet others not only accepted the results but saw that the quintic only constituted one example of an unsolvable equation. Thereby, the more general problem of algebraic solubility could be formulated.

From the perspective of investigating the concept of solubility, the quintic helped distinguish the class of algebraically solvable equations within the class of all polynomial equations (see figure 6.1). On the other hand, in his research on the division of the circle, GAUSS had demonstrated that infinitely many equations existed which could

54 For a discussion of the relationship between CAUCHY and ABEL, see chapter 12.
55 See chapter 21.

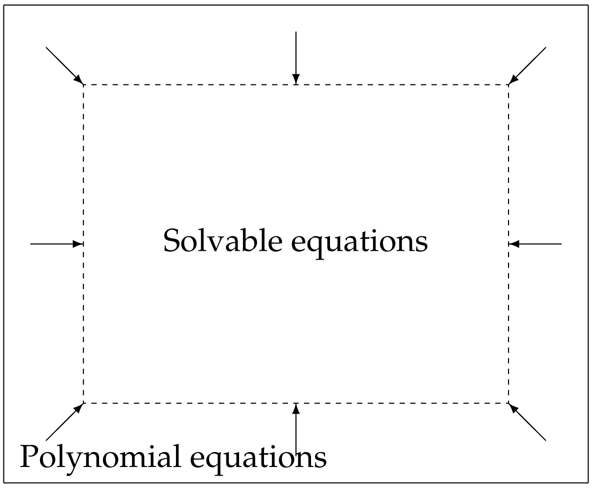


Figure 6.1: Limiting the class of solvable equations

be solved algebraically (see section 5.3). Therefore, the class of solvable equations did not collapse to a few low degree equations; soon, further solvable equations were found (see chapter 7). The search for a procedure useful in determining whether or not a given equation could be solved algebraically soon became an interesting project for mathematical research.

In the following section 6.9, I deal with the first two classes of reactions: the global and local criticism, which was advanced by ABEL'S contemporaries. In chapter 8, I describe how ABEL worked on the general problem of solubility, which was realized to its full extent and attacked shortly afterwards by GALOIS (section 8.5).

6.9 The reception of ABEL's work on the quintic

When RUFFINI published his proof of the algebraic insolubility of the quintic in Italian in 1799 the mathematical community of Europe paid little attention. Apart from a limited Italian discussion involving mathematicians outside the main stream such as P. ABBATI (1768–1842) and G. F. MALFATTI (1731–1807), only CAUCHY seems to have taken notice. Twenty-five years later, when ABEL published his proof in a brand new German mathematical journal, history could have repeated itself. However, ABEL'S proof soon became widespread knowledge and acquired a status within the mathematical community of being rigorous and close to definitive. In this section, I trace some of the events which played a role in this development, scientific and non-scientific factors, in order to describe the influence which ABEL'S research had on the subsequent evolution of the theory of equations.

Immediate reception. In his short lifetime, ABEL'S impossibility proof was published on three occasions. ABEL'S first proof — published as a pamphlet in 1824 — was, although written in French, only sparsely circulated.[56] According to HANSTEEN the copy which ABEL had sent to GAUSS in Göttingen was received with very little enthusiasm.[57] When ABEL met CRELLE in Berlin, they discussed the subject and because CRELLE and others had a hard time following the arguments, ABEL elaborated his proof. Under this procedure to clarify, ABEL produced his second proof which CRELLE subsequently found worthy of publication, translated into German, and inserted into the first issue of his *Journal für die reine und angewandte Mathematik*.[58] The immediate impact of ABEL'S paper in the mathematical community was limited. The following issue of CRELLE'S *Journal* carried a paper by the unknown mathematician L. OLIVIER on the form of roots of algebraic equations based on LAGRANGE'S research.[59] In it, OLIVIER voiced reservations concerning the solubility of the general equations which indicate that he was not an intimate member of the circle around CRELLE and had not learned of ABEL'S result prior to its publication in the *Journal für die reine und angewandte Mathematik*. The reservation might even have been inserted by CRELLE who probably also translated OLIVIER'S paper into German.

> "Furthermore, a proof of the impossibility of the solution of algebraic equations of higher degrees by radicals, should such a proof be possible, would in no way contradict the results of the above investigations on the form of the radicals, whose generality has been claimed."[60]

ABEL tried to improve the distribution of his proof as well as the reputation of CRELLE'S *Journal für die reine und angewandte Mathematik* by publishing — as a report on the paper in CRELLE'S *Journal für die reine und angewandte Mathematik* — a third version of his proof in FERRUSAC'S *Bulletin*.[61] However, in his own lifetime, ABEL was mainly known for his later work on elliptic functions (see subsequent chapters) and the impossibility proof remained less known. In corresponding with ABEL on the subject of elliptic functions, A.-M. LEGENDRE (1752–1833) urged ABEL to make public his researches on the solubility of equations which ABEL had announced in an earlier letter.[62]

> "Sir, you have announced a very beautiful work on algebraic solutions which has the purpose of giving the solution of any given numerical equation, whenever

56 (N. H. Abel, 1824b)

57 See (Hansteen, 1862, 37) and (Stubhaug, 1996, 291).

58 (N. H. Abel, 1826a)

59 Of Mr. LOUIS OLIVIER little is known. He published a total of 11 articles in the first four volumes of CRELLE'S *Journal für die reine und angewandte Mathematik* 1826–1829. OLIVIER'S mathematics and his relations to the Berlin mathematicians are the themes of a separate article under preparation.

60 "*Uebrigens würde ein Beweis der Unmöglichkeit der Auflösung höheren algebraischer Gleichungen durch Wurzelgrößen, wenn ein solcher gelänge, keinesweges den Resultaten der obigen Untersuchungen über die Form der Wurzeln, deren Allgemeinheit behauptet wurde, widersprechen.*" (Olivier, 1826a, 116).

61 (N. H. Abel, 1826c).

62 (Abel→Legendre, Christiania, 1828/11/25. N. H. Abel, 1881, 279)

> it can be developed in radicals, and to declare any equation unsolvable in this way [by radicals] which does not satisfy the required conditions; from this it follows as a necessary consequence that the general solution of the equations beyond the fourth degree is impossible. I invite you to publish this new theory as soon as you can; it would bring you much honour and generally be regarded as the biggest discovery remaining to be made in analysis."[63]

The investigations to which LEGENDRE alluded were also described in one of ABEL'S letters to HOLMBOE,[64] and were partially presented in his notebooks (see chapter 8). The above citation seems also to indicate that LEGENDRE was unaware of ABEL'S impossibility proof in CRELLE'S *Journal für die reine und angewandte Mathematik* 1826.[65]

HOLMBOE and the first edition of ABEL'S *Œuvres*. When the French mathematical community learned of ABEL'S death in 1829, the Academy sent baron J. F. T. MAURICE.[66] to condole with the Swedish envoy in Paris and suggest that the Swedish Crown Prince OSCAR undertook the publication of ABEL'S complete works.[67] In 1831, MAURICE repeated his suggestion and the editorship was delegated to ABEL'S teacher and friend HOLMBOE and the university in Christiania.[68] By 1836, HOLMBOE had completed his commentaries on the published works, but intended to include also selections from ABEL'S unpublished material in the *Œuvres*.[69] In his report to the Ministry of Ecclesiastic Affairs[70] in 1838 HOLMBOE declared that — except for a manuscript which ABEL had handed in to the French Academy[71] — he had finished collecting and commenting upon ABEL'S unpublished works.[72] Two volumes containing most of ABEL'S published works (with the noticeable exclusion of ABEL'S Parisian manuscript)[73] and some of the unpublished material from his notebooks and *Nachlass* appeared in 1839. Since most of ABEL'S papers had originally been published in French and most of his mature entries in the notebooks were in French, it had been decided that the *Œuvres* should be in French. An effort was made by HOLMBOE to dis-

63 "*Vous m'annoncez, Monsieur, un très beau travail sur les équations algébriques, qui a pour objet de donner la résolution de toute équation numérique proposée, lorsqu'elle peut être développée en radicaux, et de déclarer insoluble sous ce rapport, toute équation qui ne satisferait pas aux conditions exigées; d'où résulte comme conséquence nécessaire que la résolution générale des équations au delà du quatrième degré, est impossible. Je vous invite à publier le plutôt que vouz pourrez, cette nouvelle théorie; elle vous fera beaucoup d'honneur, et sera généralement regardée comme la plus grande dévouverte qui restait à faire dans l'analyse.*" (Legendre→Abel, Paris, 1829/01/16. N. H. Abel, 1902a, 88–89).

64 (Abel→Holmboe, Paris, 1826/10/24. ibid., 44–45).

65 (Holmboe, 1829, 349).

66 (Stubhaug, 1996, 587).

67 After a turbulent period of trembling Danish monarchy and brief independence, Norway was integrated in the Swedish monarchy 1814.

68 (Ore, 1957, 269)

69 (N. H. Abel, 1902d, 49).

70 As noted in chapter 2, the University was subsumed in the Ministry of Ecclesiastic Affairs.

71 The search for ABEL'S *Paris mémoire* is a fascinating story in its own right. See (Brun, 1949; Brun, 1953) and section 19.4, below.

72 (N. H. Abel, 1902d, 51).

73 ABEL'S Parisian manuscript known as the *Paris mémoire* is dealt with extensively in chapter 19.

tribute copies to prominent mathematicians. Therefore, ten years after ABEL'S death, the mathematical community had the opportunity to follow his arguments through HOLMBOE'S careful annotations.

> "During the revision of Abel's works it has been necessary for me to give numerous demonstrations and prove many theorems which are presented without proof by the author or whose proof is indicated so briefly that for many readers it is impossible and for almost all difficult to understand."[74]

Apart from the elaborations of vaguely suggested arguments, HOLMBOE also corrected most of the numerous misprints which had occurred in ABEL'S works published in CRELLE'S *Journal*. These, too, had served to make ABEL'S writings hard to understand.[75]

6.9.1 Local criticism of the quintic proof

Inspired by I. LAKATOS' (1922–1974) distinction between *global* and *local* counter examples, the criticism which mathematicians in the first half of the 19th century expressed toward ABEL'S proof of the insolubility of the quintic can be separated in two classes.[76] As noted, a handful of mathematicians continued to doubt or dispute the validity of the *result* that the general quintic was algebraically unsolvable. Their doubt was largely founded in an incomplete induction that equations were to be solvable; and their attitude toward ABEL'S proof ranged from ignorance to indifference. The importance of this *global* criticism is traced in section 6.9.2. On the other hand, ABEL'S *proof* was scrutinized by some of his contemporaries. Their *local* criticism picked out the vulnerable points of ABEL'S argument and sought to illuminate them or supply alternative proofs. Central to these *local* criticisms was the fact that they were based on an acceptance of the overall validity of the result but sought to secure some unclear arguments. The central parts of ABEL'S argument which was thought in need of elaboration were the classification of algebraic expressions, ABEL'S proof of the CAUCHY-RUFFINI theorem, and in particular ABEL'S study of functions of five quantities having five values under permutations.

EDMUND JACOB KÜLP. From ABEL'S correspondence with EDMUND JACOB KÜLP only ABEL'S reply to KÜLP'S questions has been preserved.[77] Therefore, we know

74 "*Under Revisionen af Abels Arbeider har det været mig nødvendigt at optegne en heel Deel Udviklinger og at bevise mange Sætninger, som hos Forfatteren ere anførte uden Beviis, eller hvis Beviis er saa kort antydet, at det for mange Læsere er umueligt og næsten for alle vanskeligt at fatte.*" (N. H. Abel, 1902d, 50).

75 (ibid., 49).

76 The distinction is inspired by (Lakatos, 1976), However, as discussed in the introduction (section 1.4.2), LAKATOS' scheme of mathematical evolution by dialectical dynamics can only be applied through largely a-historical reconstructions.

77 (Abel→Külp, Paris, 1826/11/01. In Hensel, 1903, 237–240)

Figure 6.2: WILLIAM ROWAN HAMILTON (1805–1865)

nothing of KÜLP'S attitude toward the validity of ABEL'S result. KÜLP'S criticism focused on two individual parts of ABEL'S argument. The first question was concerned with a misprint which occurred in ABEL'S proof of the CAUCHY-RUFFINI theorem. Due to the relatively new character of the theory of permutations and their notation, KÜLP apparently had trouble following ABEL'S argument and was halted by the misprint. ABEL'S notation was apparently also a problem for KÜLP; in his answer, ABEL proved the claim that any 3-cycle could be decomposed as the product of two p-cycles by writing out the substitutions in detail. I mention these objections in order to illustrate the difficulties, conceptual and technical, which nineteenth century mathematicians had in understanding and accepting ABEL'S proof.

KÜLP'S other objection concerned ABEL'S descriptive classification of rational functions of five quantities which have five values. Again, we do not have KÜLP'S formulation but only ABEL'S reply which ABEL posted from Paris less than a year after his paper had appeared in CRELLE'S *Journal*. The argument given in the letter differed substantially from the published one. As I have discussed above (in section 6.6.1), the original argument was, indeed, very hard to understand. If ABEL'S refined proof communicated to KÜLP had made it into print, ABEL'S conclusion might have been accepted at an earlier point.

William Rowan Hamilton. On the British Isles, the debate over the solubility of the general quintic took a different turn. During the years 1832–35, G. B. Jerrard (1804–1863) published his three volume work *Mathematical Researches* in which he claimed to have presented a general method of solving equations algebraically. At the 1835 meeting of the *British Association* in Dublin, William Rowan Hamilton was appointed reporter on the paper and was thus led into the theory of equations.[78] In May 1836, after having dismissed Jerrard's claim for a general solution to higher degree equations in a paper in the *Philosophical Magazine*,[79] Hamilton asked his friend J. W. Lubbock (1803–1865) to supply him with a copy of Abel's paper from Crelle's *Journal für die reine und angewandte Mathematik*. Approaching it in his very thorough and critical style, Hamilton found it somewhat unsatisfactory, and began to write his own exposition of Abel's result.[80] The following year he presented his investigations to the *Royal Irish Academy*, in whose *Transactions* they were printed.[81]

In his study of Abel's proof, Hamilton noticed two "mistakes", the first of which concerned Abel's classification of algebraic expressions (see theorem 2). After having translated Abel's proof into his own notation, Hamilton clearly expressed his objection:[82]

> "Although the whole of the foregoing argument has been suggested by that of *Abel*, and may be said to be a commentary thereon; yet it will not fail to be perceived, that there are several considerable differences between the one method of proof and the other. More particularly, in establishing the cardinal proposition that every radical in every irreducible expression for any one of the roots of any general equation is a rational function of those roots, it has appeared to the writer of this paper more satisfactory to begin by showing that the radicals of highest order will have that property, if those of lower orders have it, descending thus to radicals of the lowest order, and afterwards ascending again; than to attempt, as *Abel* has done, to prove the theorem, in the first instance, for radicals of the highest order. In fact, while following this last-mentioned method, *Abel* has been led to assume that the coefficient of the first power of some highest radical can always be rendered equal to unity, by introducing (generally) a new radical, which in the notation of the present paper may be expressed as follows:
>
> $$\sqrt[\alpha_k^{(m)}]{\left\{\sum_{\beta_i^{(m)}<\alpha_i^{(m)}} \cdot \left(b^{(m-1)}_{\beta_1^{(m)},\ldots\beta_{n^{(m)}}^{(m)}} . a_1^{(m)\beta_1^{(m)}} \ldots a_{n^{(m)}}^{(m)\beta_{n^{(m)}}^{(m)}} \right)\right\}^{\alpha_k^{(m)}}} ;$$
> $$\beta_k^{(m)} = 1$$
>
> but although the quantity under the radical sign, in this expression, is indeed free from that irrationality of the m^{th} order which was introduced by the radical $a_k^{(m)}$, it

78 Hamilton was subsequently knighted for bringing the meeting to Dublin (Hankins, 1972).

79 (W. R. Hamilton, 1836).

80 (Hankins, 1980, 277).

81 (W. R. Hamilton, 1839).

82 Hamilton's notation $\alpha_k^{(m)}$ indicates that α_k is what Abel called an algebraic expression of the m^{th} order (ibid., 171–172).

> is not, in general, free from the irrationalities of the same order introduced by the other radicals $a_1^{(m)}, \ldots$ of that order; and consequently the new radical, to which this process conducts, is in general elevated to the order $m+1$; a circumstance which *Abel* does not appear to have remarked, and which renders it difficult to judge of the validity of his subsequent reasoning."[83]

To HAMILTON, the mistake made by ABEL had obscured the validity of ABEL'S subsequent reasoning, but the validity of the impossibility result, itself, was not questioned since HAMILTON had provided it with a proof not based on ABEL'S hierarchy. Later, KÖNIGSBERGER would prove that ABEL'S hierarchy of algebraic expressions could still be rescued (see below). By the end of the century, it was eventually realized that the hierarchic structure imposed on algebraic expressions was actually superfluous for the impossibility proof.[84]

HAMILTON continued his scrutiny of ABEL'S proof by attacking ABEL'S characterization of functions of five quantities having five values under permutations:

> "And because the other chief obscurity in *Abel*'s argument (in the opinion of the present writer) is connected with the proof of the theorem, that a rational function of five independent variables cannot have five values and five only, unless it be symmetric relatively to four of its five elements; it has been thought advantageous, in this paper, as preliminary to the discussion of the forms of functions of five arbitrary quantities, to establish certain auxiliary theorems respecting functions of fewer variables; which have served also to determine *à priori* all possible solutions (by radicals and rational functions) of all general algebraic equations below the fifth degree."[85]

Thus, HAMILTON pointed his finger directly at the two weak points of ABEL'S argument. For ABEL'S flawed proof of the central auxiliary theorem — that all occurring radicals were rational functions of the roots — which he had proved by the hierarchic structure of algebraic expressions, HAMILTON substituted an argument descending and re-ascending the hierarchy of algebraic expressions.[86] The characterization of functions of five variables having five values under permutations was also carried out at length in an analysis which — following ABEL — reduced it to the study of such functions when only four of the arguments were permuted. As ABEL had done, HAMILTON completed his analysis of these functions through an extensive investigation of particular classes.[87]

HAMILTON employed a detailed style of presentation and extensive use of low degree equations as examples; nevertheless, his exposition of ABEL'S result is not particularly clear and easy to grasp.[88] The degree of detail and a complicated notation might also have obscured the main results to some of HAMILTON'S contemporaries.

83 (ibid., 248); small-caps changed into italic..
84 (J. Pierpont, 1896, 200).
85 (W. R. Hamilton, 1839, 248–249); small-caps changed into italic..
86 (ibid., 194–196).
87 (ibid., 237–246).
88 (Dickson, 1959, 179) calls it "a very complicated reconstruction of ABEL'S proof".

Neither HAMILTON'S exposition of ABEL'S proof nor his more direct criticisms of JERRARD'S works seemed to convince JERRARD of his mistake.[89] JERRARD continued to announce his claim in the *Philosophical Magazine* and in 1858 he published his *Essay on the Resolution of Equations*. By that time it was left to A. CAYLEY (1821–1895) and J. COCKLE to refute JERRARD'S claims.[90]

BERNT MICHAEL HOLMBOE. The French mathematical community mainly knew of ABEL'S work on the solubility of equations through BERNT MICHAEL HOLMBOE'S edition of ABEL'S collected works (see above).[91] HOLMBOE'S extensive annotations and elaborations were often supplying explicit calculations in places where ABEL had been brief. In terms of criticism and modification of ABEL'S proof, HOLMBOE'S annotations center on three topics: irreducibility, functions of five quantities, and an explicit description of the process of inversion which ABEL had employed (see page 119).

HOLMBOE opened with a short treatment of reducible and irreducible equations, in which he gave examples. He explicitly termed an equation *irreducible* when no root of the equation could be the root of an equation of "the same form", but of lower degree.[92] This definition was implicit in ABEL'S paper;[93] it later took on a more explicit and very central role in ABEL'S theory of solubility (see chapters 7 and 8).

Concerning ABEL'S investigations of functions of five quantities with five values, HOLMBOE'S annotations are of another character giving alternative proofs of unclear points. Remaining faithful to ABEL'S approach in the case in which $\mu = 2$ (see page 115), HOLMBOE supplied expressions with 30 and 10 different values to rule out the cases $m = 4$ and $m = 5$ which ABEL had left out. Thus, HOLMBOE sought to complete ABEL'S deduction of a contradiction. But sensing the obscure nature of ABEL'S classification of functions of five quantities with five values, HOLMBOE set out to derive his own.[94] HOLMBOE applied a general theorem, which he had proved in the *Magazin for Naturvidenskaberne*:

> "In the same way one can demonstrate that if u designates a given function of n quantities which takes on m different values when one interchanges these n quantities among themselves in all possible ways, the general form of the function of n quantities which by these mutual permutations can obtain m different values will be
>
> $$r_0 + r_1 u + r_2 u^2 + \cdots + r_{m-1} u^{m-1},$$
>
> $r_0, r_1, r_2 \ldots r_{m-1}$ being symmetric functions of the n quantities."[95]

89 Actually, HAMILTON thought highly of JERRARD'S results, which he interpreted in a restricted frame. Although JERRARD'S claim for solving general equations could not be supported, the method which he had employed was nevertheless of great importance since it — if applied to the quintic — could reduce it to the *normal trinomial form* $x^5 + px + q = 0$.

90 For instance (Cayley, 1861; Cockle, 1862; Cockle, 1863).

91 (N. H. Abel, 1839).

92 (Holmboe in ibid., 409).

93 (N. H. Abel, 1826a, 71, 82). See quotation on page 106.

94 (Holmboe in N. H. Abel, 1839, 411–413).

HOLMBOE'S proof implicitly involved LAGRANGE'S notion of *semblables* functions (functions which are altered in the same way by the same permutations), and argued *directly* that any function of five quantities, which took on five different values, must have the form of a fourth degree polynomial in which the coefficients were symmetric functions of $x_1, \ldots, x_5$.

The final noteworthy contribution by HOLMBOE to ABEL'S impossibility proof was his calculations relating to the process described as inversion of polynomials. HOLMBOE proved — through manipulations on power sums — that any fourth degree polynomial v in x

$$v = \sum_{\alpha=0}^{4} r_\alpha x^\alpha$$

could be *inverted* into

$$x = \sum_{\alpha=0}^{4} s_\alpha v^\alpha.$$ [96]

The proof is a *tour de force* dealing with symmetric functions, much in the style of E. WARING ($\sim$1736–1798), although in a clearer notational setting.

In his commentary, HOLMBOE did not penetrate to the core of the problems spotted by HAMILTON. Instead, he elaborated many of ABEL'S arguments and manipulations and supplied proofs of obscure passages. HOLMBOE'S only real reservation against ABEL'S proof concerned the classification of functions with five values, and HOLMBOE provided an alternative deduction using methods and concepts introduced by LAGRANGE and quite familiar to ABEL.

KÖNIGSBERGER. While HOLMBOE'S elaboration of ABEL'S classification of functions with five quantities might have settled HAMILTON'S unease on this objection, it took longer before HAMILTON'S other reservation was lifted. The objection raised against ABEL'S classification of algebraic expressions was lifted in two steps: In 1869, KÖNIGSBERGER demonstrated how ABEL'S classification could be rescued by modifying the claims concerning the orders and degrees of the coefficients in the representation

$$v = q_0 + p^{\frac{1}{n}} + q_2 p^{\frac{2}{n}} + \cdots + q_{n-1} p^{\frac{n-1}{n}}.$$

(see page 104).[97] The slight modification which KÖNIGSBERGER introduced revalidated ABEL'S hierarchy on algebraic expressions, and showed that ABEL'S "mistake" was of no real consequence to the proof. KÖNIGSBERGER had been stimulated to make

95 "*De la même manière on peut démontrer que, si u signifie une fonction donnée de n quantités qui prend m valeurs différentes lorsqu'on échange ces n quantités entre elles de toutes les manières possibles, la forme générale de la fonction de n quantités qui par leurs permutations mutuelles peut obtenir m valuers différentes sera*

$$r_0 + r_1 u + r_2 u^2 + \cdots + r_{m-1} u^{m-1},$$

$r_0, r_1, r_2 \ldots r_{m-1}$ étant des fonctions symétriques des n quantités." (Holmboe in ibid., 413).

97 (Königsberger, 1869).

his remedy public by the fact that such a classification was of importance by itself and the circumstance that ABEL'S flawed classification had been reproduced in J. A. SERRET'S (1819–1885) textbook *Cours d'algèbre*.[98]

The two central arguments in ABEL'S proof, to which HAMILTON raised his objections, had also stimulated other mathematicians to give alternative proofs and modifications of ABEL'S deduction. The defective classification of algebraic expressions, which for ABEL served to demonstrate that any radical in a solution was a rational function of the roots, had made HAMILTON doubt the subsequent reasoning and he supplied another deduction. With KÖNIGSBERGER, the original deduction was rescued by a slight modification, and the flaw was claimed — without detailed proof — to be of no importance in the proof. Subsequently, the classification of algebraic expressions was disbanded altogether in the impossibility proof. The other obscurity — ABEL'S classification of rational functions of five quantities which take on five different values under permutations — had been noticed in private correspondence by KÜLP, and ABEL had presented him with another more transparent deduction which, unfortunately, remained unknown to the larger mathematical public. When HAMILTON noticed the weakness of the published argument, he recast the deduction within his own framework; HOLMBOE provided it with a more general proof along the lines of other parts of ABEL'S reasoning.

For various reasons — doubt and curiosity, debate over the validity of result, and concerns for the best presentation of ABEL'S work — these mathematicians took up weak parts of ABEL'S proof and provided clearer arguments and proofs. This *local criticism* served to establish the overall validity of the impossibility of algebraically solving the quintic by examining and improving the proof.

6.9.2 Dissemination of the knowledge that the quintic was unsolvable

The controversy which raged on the British Isles concerning the insolubility of the general quintic equation seems to have been largely confined to there,[99] although HAMILTON was also called upon to refute the claim for solubility made by the Italian P. G. BADANO.[100] While HAMILTON'S penetrating local criticism of ABEL'S proof was undertaken to resolve an ongoing debate over ABEL'S statement, the Continental incorporation of ABEL'S result apparently followed another path. On the Franco-German scene, I am not aware of any *global* rejections of ABEL'S result. The style of later *local* reworkings of ABEL'S proof left little clue as to *what*, besides refinement and aesthetics, had spurred the mathematician to reformulate the argument.

98 (Königsberger, 1869, 168).

99 Besides JERRARD, MACCULLAGH also transmitted a claim to have solved the general fifth degree equation and was refuted by HAMILTON (Hankins, 1980, 438, note 22).

100 (W. R. Hamilton, 1843; W. R. Hamilton, 1844). See also (Hankins, 1980, 438, note 22). Personal data for Mr. BADANO have proved to be inaccessible.

PIERRE LAURENT WANTZEL. In a short paper published in 1845, PIERRE LAURENT WANTZEL refined ABEL'S proof by reversing the succession in which the radicals of a supposed solution is studied like HAMILTON had done.[101] Although WANTZEL deemed ABEL'S proof to be exact, he also found its presentation vague and complicated. Nevertheless, WANTZEL gave no detailed reasons for his evaluation.

> "Although his [ABEL'S] proof is basically exact it is presented in a way so complicated and so vague that it would not be generally permissible."[102]

Through a fusion of ABEL'S proof with the even vaguer and more insufficient proof by RUFFINI, WANTZEL arrived at a clear and precise proof which he thought would "lift all doubts concerning this important part of the theory of equations".[103] Unfortunately, he did not specify his "doubts".

In his fusion proof, WANTZEL took over the most important of ABEL'S preliminary arguments: the classification of algebraic expressions by orders and degrees and the auxiliary theorem derived from it (see page 104). By studying any supposed solution of the general n^{th} degree equation and permutations of the roots, WANTZEL deduced that the *outermost* root extraction would have to be a square root.[104] Continuing to the radical of second highest order, he found that it had to remain unaltered by any 3-cycle, and therefore by any 5-cycle.[105] At this point he reached a contradiction because the supposed solution would thus only have two different values under all permutations of the five roots.

WANTZEL'S proof was published in the *Nouvelles annales de mathematique* and soon became the widely accepted simplification of ABEL'S proof. It made no use of ABEL'S classification of functions of five quantities, and may thus be seen as an indirect local criticism of this classification. On the other hand, it builds directly upon ABEL'S classification of algebraic expressions.

A. E. G. ANDERSSEN. In 1848, the *Königlichen Friedrichs-Gymnasium* in Breslau invited its "protectors, sons, and friends" to be present at the annual exams. Included with the invitation was a short essay written by one of the teachers; at the time, this was not uncommon practice for German *Gymnasien*.[106] In the essay, A. E. G. ANDERSSEN [107] sought to illuminate the central arguments of ABEL'S impossibility proof. Being largely a reproduction of ABEL'S argument with some elaboration of its briefest arguments, the interesting parts of ANDERSSEN'S essay are his evaluations of ABEL'S

101 (Wantzel, 1845).
102 "*Quoique sa démonstration soit exacte au fond, elle est présentée sous une forme trop compliquée et tellement vague, qu'elle n'a pas été généralement admise.*" (ibid., 57).
103 (ibid., 57).
104 (ibid., 62).
105 (ibid., 63–64). See also CAUCHY'S proof of the CAUCHY-RUFFINI theorem, section 5.6.
106 (Anderssen, 1848).
107 No further personal information concerning this Mr. ANDERSSEN has been accessible.

proof. ANDERSSEN found the proof to be simple, coherent, and not built upon calculations but on arguments and deductions; at the same time, and possibly for the same reasons, he rated it as being difficult.

> "However simple this proof is, first of all because a single idea serves throughout as a decisive criterion, secondly because the truths by which the application of the main idea is possible, are communicated not by artificial calculations but by conclusions and deductions, it nevertheless (and even therefore) demands the most thorough contemplation in order to be understood in its entire clarity. Therefore it would not be a superfluous work to present the most important arguments of this instructive yet difficult proof by examples and further elaborations in their true spirit and full power of proof."[108]

ABEL'S classification of algebraic expressions according to orders and degrees was reproduced in an overly simplified form, in which the concept of degree has completely vanished. When it came to the classification of functions with five quantities, which HAMILTON had scrutinized, ANDERSSEN found it quite satisfactory:

> "Both these two theorems [no function of five quantities can have two or five different values under all possible interchanges of the quantities] have been proved in *Abel*'s treatise with a clarity which cannot be improved."[109]

ANDERSSEN'S essay contained no criticism of parts of ABEL'S proof nor any original modifications but only simple elaborations and some examples. However, its mere existence is evidence that ABEL'S result was becoming known to the broader circle of German mathematicians.

LEOPOLD KRONECKER. The introduction of ABEL'S work on the quintic equation into German academic circles is due to LEOPOLD KRONECKER. Much of KRONECKER'S work on algebra was inspired by ideas which he got reading ABEL and KRONECKER completed and rigorized many parts of ABEL'S research. In KRONECKER'S elegant proof of the insolubility of the general fifth (and higher) degree equation, ABEL'S proof found its final form. KRONECKER presented his simplified version of ABEL'S proof in a paper read to the *Akademie der Wissenschaften*.[110] There, he presented no criticism of ABEL'S proof but simply put forward alternative deductions preferable to ABEL'S on account of their simplicity and general nature. The validity of the *result*

108 "*So einfach dieser Beweis ist, erstens weil ein einziger Gedanke durchgehends zum entscheidenden Kriterium dient, zweitens weil diejenigen Wahrheiten, kraft deren die Anwendung des Hauptgedankens möglich ist, nicht durch künstliche Rechnungen, sondern durch Urtheile und Schlüsse vermittelt werden; so erheischt er dennoch, ja eben deswegen das gesammeltste Nachdenken, um in seiner ganzen Klarheit begriffen zu werden. Es dürfte daher keine unnöthige Arbeit sein, die wichtigsten Argumente dieses eben so lehrreichen als schwierigen Beweises durch Beispiele und weitere Ausführung in ihrem wahren Sinne und in ihrer vollständigen Beweiskraft zur Anschauung zu bringen.*" (Anderssen, 1848, 3).

109 "*Diese beiden Lehrsätze sind in* Abel*'s Abhandlung mit einer Klarheit bewiesen, welche durch Nichts erhöht werden kann.*" (ibid., 14).

110 (Kronecker, 1879).

was never questioned by KRONECKER; his improvements were *local* in the sense of replacing some of ABEL'S arguments by more apt ones.

Through a detailed reworking of ABEL'S classification, KRONECKER obtained a more precise formulation of ABEL'S auxiliary theorem on the rationality of all radicals occurring in any solution. KRONECKER let $\mathfrak{R}, \mathfrak{R}', \mathfrak{R}''$, etc. denote quantities, which were to be considered *known*, and spoke of the collection of these as "the quantities $\mathfrak{R}$". Later, this $\mathfrak{R}$ evolved into his general concept of *domains of rationality*.

> "In the described way the explicit algebraic function satisfying an equation $\Phi(x) = 0$ can be expressed as an entire function of the quantities
>
> $$W_1, W_2, \ldots W_\mu$$
>
> the coefficients of which are rational functions of the quantities $\mathfrak{R}$; the quantities W are on the one hand entire integer functions of the roots of the equation $\Phi(x) = 0$ and of roots of unity and on the other hand determined through a chain of equations
>
> $$W_\beta^{n_\beta} = G_\beta\left(W_{\beta+1}, W_{\beta+2}, \ldots W_\mu\right) \quad (\beta = 1, 2, \ldots \mu)$$
>
> $n_1, n_2, \ldots$ being prime numbers and $G_1, G_2, \ldots G_\mu$ designating entire functions of the bracketed quantities W in which the coefficients are rational functions of the quantities $\mathfrak{R}$."[111]

Although apparently formulated in a slightly different way, this theorem is very close to the one of ABEL'S auxiliary theorems ensuring the rationality of the involved radicals (theorem 3), and served KRONECKER as its equivalent. The improvements are mainly the introduction of the rationally known quantities $\mathfrak{R}$ and the explicit mention of the roots of unity. To ABEL, the roots of unity had been "known" in the common language version of this word, because he knew enough of them to handle them as simple objects. Consequently, roots of unity were not explicitly mentioned. The process of attributing technical mathematical meaning to a common language term occurred frequently in the period as is evident, for example, in the way GALOIS'S notion of groups was transformed from meaning a "collection of objects" into a term with a highly technical meaning.

111 "*In der dargelegten Weise erhält die einer Gliechung $\Phi(x) = 0$ genügende explicite algebraische Function als ganze Function von Grössen*

$$W_1, W_2, \ldots W_\mu$$

dargestellt, deren Coëfficienten rationale Functionen der Grössen $\mathfrak{R}$ sind, und die Grössen W sind einerseits ganze ganzzahlige Functionen von Wurzeln der Gleichung $\Phi(x) = 0$ und von Wurzeln der Einheit andererseits durch eine Kette von Gleichungen

$$W_\beta^{n_\beta} = G_\beta\left(W_{\beta+1}, W_{\beta+2}, \ldots W_\mu\right) \quad (\beta = 1, 2, \ldots \mu)$$

bestimmt, in denen $n_1, n_2, \ldots$ Primzahlen und $G_1, G_2, \ldots G_\mu$ ganze Functionen der eingeklammerten Grössen W bedeuten, deren Coëfficienten rationale Functionen der Grössen $\mathfrak{R}$ sind." (ibid., 77).

The succeeding part of KRONECKER'S proof concerned the substitution theoretic aspects of ABEL'S proof and consisted of an extended version of the CAUCHY-RUFFINI theorem. For $n > 4$, KRONECKER let f designate a function of quantities $x_1, \ldots, x_n$ and studied the *conjugate functions* $f_1, \ldots, f_m$; these functions were the analogous of what ABEL had called the different values of f under all permutations of $x_1, \ldots, x_n$. KRONECKER derived the result that for any non-symmetric function f, *some* permutation would exist which altered the value of one of the conjugate functions. He could even demonstrate that if only the $\frac{n!}{2}$ permutations, which left the product $\prod_{i<j} (x_i - x_j)$ unaltered, were considered, the result would still be true. These permutations are the equivalents of even permutations, i.e. they belong to the subgroup A_n of Σ_n. With this established, KRONECKER was able to deduce the theorem of CAUCHY, which ABEL had used, as a corollary.[112]

Thus, KRONECKER'S reworking of ABEL'S proof mainly consisted of a rigorization and generalization of ideas found in ABEL'S work. The general approach remained the same but the proof, concepts, and notation had undergone a dramatic evolution in the half-century which had elapsed. While ABEL'S deduction was aimed at proving the impossibility of the algebraic solution of the quintic, KRONECKER'S approach was more general and wrapped in the emerging theory of groups.

Many mathematicians of the second half of the 19$^{\text{th}}$ century were deeply occupied with understanding GALOIS' works, and subsequent to KRONECKER it became customary to deduce the insolubility of the general quintic from GALOIS theory (see section 8.5).

6.9.3 Global and local criticism

The only *global* criticism still traceable in the mathematical literature is the British controversy, at the outset of which JERRARD and HAMILTON engaged in their dispute. As a response to JERRARD'S claim of having devised a general method for reducing equations of any degree to lower degree equations, HAMILTON scrutinized ABEL'S proof in order to use it as an argument in the debate. HAMILTON'S penetrating analysis of ABEL'S argument led him to detect two points of obscurity in the classification of algebraic expressions and the classification of functions with five values. HAMILTON replaced both these arguments by his own deductions which differed slightly from ABEL'S line of argument.

The two *local* criticisms which HAMILTON raised have reemerged in many evaluations of ABEL'S proof, both independently and inspired by HAMILTON. The problem concerning the functions of five quantities was spotted by KÜLP in the same year as the original publication and ABEL responded by giving a different deduction. HOLMBOE, too, was worried about this classification and wrote one of his few mathematical papers generalizing it and providing it with an alternative proof remaining in the line

112 (Kronecker, 1879, 80).

of ABEL'S argument but superior in rigour. The classification of algebraic expressions was also a concern of some later 19^{th} century mathematicians, until it was settled by KÖNIGSBERGER and KRONECKER.

The fact that *global* criticism of ABEL'S impossibility proof was limited can be taken as a sign that the mathematical community soon came to realize the overall validity of the result. The change of attitude toward the problem, which had been facilitated by the statements of experts such as LAGRANGE and GAUSS (section 5.4) and the proofs of RUFFINI, which at least were known in some circles in Paris, had been a prerequisite for the quick acceptance. However, *local* criticism was still conducted in an effort to make ABEL'S proof clearer and more powerful. Central lemmata, on which doubt could be cast, were reexamined and new proofs were given.

6.10 Summary

As described, ABEL'S proof of the insolubility of the general quintic was a curious combination of general theorems and investigations of particular cases. Partly because of the counter intuitive nature of the result and partly because of legitimate local objections to ABEL'S argument, the result was subsequently scrutinized. Interpreted in terms of delineation of concepts, the algebraic insolubility of the general quintic distinguished the concepts of polynomial equations and algebraically solvable equations.

Chapter 7

Particular classes of equations: enlarging the class of solvable equations

If N. H. ABEL'S (1802–1829) proof of the impossibility of solving the general quintic algebraically was hampered by its brevity and obscure arguments, his only other published work on the theory of equations was more mature, beautifully lucid, and rigorous. In the *Mémoire sur une classe particulière d'équations résolubles algébriquement*, written in 1828 and published the following year, ABEL abandoned one of the central pillars of the impossibility proof — the theory of permutations — and provided a direct and affirmative proof of the algebraic solubility of a particular class of equations.[1] Focusing instead on the other pillar — the concepts of divisibility, irreducibility, and the Euclidean algorithm — this work illuminates central ideas in ABEL'S reasoning which permeate his entire work on the theory of equations.

The 1829-paper has become a classic of mathematics for its proof that the class of equations, now called *Abelian* and defined by certain properties of the roots, are always algebraically solvable. When contrasted with the contents of the impossibility proof, this result highlights a feature of the new ways of asking questions — the mechanisms of limiting and enlarging class of objects which in the nineteenth century provided the background for a new, concept based approach to mathematics. However, the paper contains more information than just this main result; in this chapter I describe some of the connections between this work and other parts of ABEL'S research as well as some of the very central concepts which ABEL put to use in it.

1 (N. H. Abel, 1829c).

7.1 Solubility of *Abelian* equations

The structure of ABEL'S *Mémoire sur une classe particulière* [2] is a descent from the general to the particular. At the outset, ABEL proposed to study *irreducible* equations in which one of the roots depended rationally on another one. The concept of *irreducible* equations took a central place in this research (see section 7.3). Part of the study was especially devoted to circular functions to which ABEL had been led by C. F. GAUSS' (1777–1855) work on the cyclotomic equation. Besides this application to circular functions, ABEL also worked on another application of the general theory to the division problem for elliptic functions. Likewise inspired by GAUSS' *Disquisitiones arithmeticae* (see section 7.2), this application was, however, not contained in the paper but had been presented the previous year in a paper on elliptic functions. ABEL was led by these two applications to an even more general result — valid for a broader class of equations having rationally related roots. In this section, I outline ABEL'S results before turning to discussions of his inspirations and methods.

7.1.1 Decomposition of the equation into lower degrees

Throughout the paper, ABEL studied polynomial equations of degree μ,

$$\phi(x) = 0,$$

in which two roots x_1, x' were related by the rational function θ,

$$x' = \theta(x_1).$$

The quantities which ABEL considered *known* in his deductions comprise all coefficients occurring in ϕ or θ. From a modern perspective, it will become clear that he also considered any required roots of unity to be known.

ABEL defined the equation $\phi(x) = 0$ to be *irreducible* when none of its roots could be expressed by a similar equation of lower degree (see section 7.3).

Employing the Euclidean division algorithm (see section 7.3) and the notation $\theta^k(x_1)$ for the k^{th} iterated application of the rational function θ to x_1, ABEL found that the set of roots of $\phi(x) = 0$ split into sequences (chains). He deduced — using the irreducibility of $\phi(x) = 0$ — that because the two roots x_1, x' of the equation $\phi(x) = 0$ were rationally related, every iteration $\theta^k(x_1)$ would also be a root of $\phi(x) = 0$. Therefore, the entire set of roots of $\phi(x) = 0$ could be collected in sequences of equal length, say n, and he wrote the roots as ($\mu = m \times n$),[3]

$$\theta^k(x_u) \text{ for } 0 \leq k \leq n-1 \text{ and } 1 \leq u \leq m. \tag{7.1}$$

[2] (N. H. Abel, 1829c).

[3] For brevity, I have added to ABEL'S notation the convention $\theta^0(x_1) = x_1$.

After ABEL had divided the roots into sequences, he proceeded to reduce the solution of the equation of degree μ to equations of lower degrees. To the first sequence $x_1, \theta(x_1), \ldots, \theta^{n-1}(x_1)$, ABEL assigned an *arbitrary* rational and symmetric function y_1 of these quantities. Since θ was also a rational function, y_1 was actually a rational function of x_1,

$$y_1 = f\left(x_1, \theta(x_1), \ldots, \theta^{n-1}(x_1)\right) = F(x_1),$$

and using the symmetry of y_1, ABEL found

$$y_1 = \frac{1}{n}\sum_{k=0}^{n-1} F\left(\theta^k(x_1)\right)$$

and more generally

$$y_1^\nu = \frac{1}{n}\sum_{k=0}^{n-1} \left(F\left(\theta^k(x_1)\right)\right)^\nu \tag{7.2}$$

for any non-negative integer ν. In the same way as ABEL formed the function y_1 from x_1, he formed an additional $m-1$ functions $y_2, \ldots, y_m$ corresponding to the other chains,

$$y_u = f\left(x_u, \theta(x_u), \ldots, \theta^{n-1}(x_u)\right) = F(x_u) \text{ for } 1 \le u \le m. \tag{7.3}$$

Each of these produced the equivalent of (7.2)

$$y_u^\nu = \frac{1}{n}\sum_{k=0}^{n-1} \left(F\left(\theta^k(x_u)\right)\right)^\nu \text{ for } 1 \le u \le m \text{ and } \nu \ge 0.$$

ABEL added these (over u) as

$$r_\nu = \sum_{u=1}^{m} y_u^\nu \quad \text{for } \nu \ge 0 \tag{7.4}$$

and obtained rational and symmetric functions of *all* the roots of $\phi(x) = 0$. These could, he noticed, therefore be expressed rationally in the coefficients of the known functions ϕ and θ. Once these power sums (7.4) were known, ABEL could determine any rational and symmetric function of $y_1, \ldots, y_m$ by the solution of an equation of degree m by E. WARING'S (∼1736–1798) result (see section 5.2.4). In particular, ABEL found that each of the coefficients of the equation

$$\prod_{u=1}^{m} (y - y_u) = 0 \tag{7.5}$$

could be determined by solving an equation of the m^{th} degree.

A central topic of ABEL'S paper is the detailed study of this decomposition of the equation of degree $\mu = m \times n$ into equations of degrees m and n. His next step was to focus attention on the equation connected with the first sequence of roots $x_1, \theta(x_1), \ldots, \theta^{n-1}(x_1)$, i.e.

$$\prod_{k=0}^{n-1} \left(x - \theta^k (x_1) \right) = 0. \tag{7.6}$$

ABEL proved that any coefficient $\psi(x_1)$ of this equation would depend rationally on y_1 and known quantities of ϕ and θ by the following nice and typical argument.

Denoting by $\psi(x_1)$ any coefficient of (7.6), ABEL formed the expressions

$$t_\nu = \sum_{u=1}^{m} y_u^\nu \cdot \psi(x_u) \text{ for } \nu \geq 0,$$

which he proved to be rational and symmetric functions of *all* the roots of $\phi(x) = 0$. Thereby, t_ν could be expressed rationally in the known quantities.

From a set of linear equations equivalent to the matrix equation

$$\begin{bmatrix} 1 1 & \ldots 1 \\ y_1 y_2 & \ldots y_m \\ \vdots \vdots & \ddots \vdots \\ y_1^{m-1} y_2^{m-1} & \ldots y_m^{m-1} \end{bmatrix} \begin{bmatrix} \psi(x_1) \\ \psi(x_2) \\ \vdots \\ \psi(x_m) \end{bmatrix} = \begin{bmatrix} t_0 \\ t_1 \\ \vdots \\ t_{m-1} \end{bmatrix},$$

ABEL deduced that $\psi(x_1)$ could be expressed as a rational function of $y_1, \ldots, y_m$. His argument is based on the possibility of attributing a non-vanishing form to the equivalent of the determinant of the matrix. This was possible because y_1 had — up to now — been an arbitrary symmetric function, and ABEL gave it the non-vanishing form

$$y_1 = \prod_{k=0}^{n-1} \left(\alpha - \theta^k (x_1) \right),$$

where α was unspecified. Furthermore, ABEL continued to show how each of the quantities $y_2, \ldots, y_m$ could be replaced by a rational function of y_1, and how $\psi(x_1)$ could be expressed as a rational function of y_1 alone. Thus, each coefficient $\psi(x_1)$ in the equation (7.6) could be determined rationally in y_1; and y_1 could be determined by solving an equation of degree m. ABEL summarized these results in an important theorem:

Theorem 5 *"The equation under consideration $\phi x = 0$ can thus be decomposed into a number m of equations of degree n in which the coefficients are rational functions of a fixed root of a single equation of degree m, respectively."*[4]

[4] *"L'équation proposée $\phi x = 0$ peut donc être décomposée en un nombre de m d'équations du degré n; donc[!] les coëfficiens sont respectivement des fonctions rationnelles d'une même racine d'une seule équation du degré m."* (N. H. Abel, 1829c, 139). The misprint "donc" has been replaced by "dont" in both editions of ABEL'S *Œuvres*.

Thus, the original problem of solving the equation $\phi(x) = 0$ of degree μ had been reduced to solving certain equations, (7.5) and (7.6), of lower degrees. Generally, the equation of degree m would not be solvable by radicals, but as ABEL went on to demonstrate, the m equations of degree n could always be solved algebraically.

7.1.2 Algebraic solubility of *Abelian* equations

If all the roots of the equation $\phi(x) = 0$ fell into the same *orbit* of θ (one chain), i.e. are of the form

$$x_1, \theta(x_1), \theta^2(x_1), \ldots, \theta^{n-1}(x_1),$$

the situation was equivalent to assuming $m = 1$ above. In this case, ABEL let α denote a primitive μ^{th} root of unity and formed the rational expression

$$\psi(x) = \left(\sum_{k=0}^{\mu-1} \alpha^k \theta^k(x)\right)^{\mu}. \tag{7.7}$$

Through direct calculations, he proved that

$$\psi\left(\theta^k(x)\right) = \psi(x) \quad \text{for all } k = 0, 1, \ldots, \mu - 1,$$

which showed that ψ was a symmetric function of the roots of $\phi(x) = 0$. Thus, $\psi(x)$ could be expressed rationally in known quantities. Next, ABEL introduced μ radicals of (7.7),

$$\sqrt[\mu]{v_u} = \sum_{k=0}^{\mu-1} \alpha_u^k \theta^k(x) \text{ for } 0 \leq u \leq \mu - 1, \tag{7.8}$$

by attributing to α_u the different μ^{th} roots of unity $1, \alpha, \alpha^2, \ldots, \alpha^{\mu-1}$. From these radicals, it was a routine procedure for ABEL to obtain the expression

$$\theta^k(x) = \frac{1}{\mu}\left(-A + \sum_{u=1}^{\mu-1} \alpha^{uk} \sqrt[\mu]{v_u}\right), k = 0, 1, \ldots, \mu - 1, \tag{7.9}$$

where A was a constant.

The expression (7.9), however, contained $\mu - 1$ extractions of roots with exponent μ which seemed to indicate that a total of $\mu^{\mu-1}$ different values could be obtained although the degree of $\phi(x) = 0$ was only μ. ABEL resolved this apparent contradiction, similar to one noticed by L. EULER (1707–1783) (see section 5.1), by an elegant argument prototypic of his approach to the theory of equations. In the deduction, ABEL proved that all the root extractions depended on one of them by considering

$$\sqrt[\mu]{v_k}\,(\sqrt[\mu]{v_1})^{\mu-k} = \left(\sum_{u=0}^{\mu-1} \alpha^{ku} \theta^u(x)\right) \times \left(\sum_{u=0}^{\mu-1} \alpha^u \theta^u(x)\right)^{\mu-k}.$$

Obviously, the form of the right hand side shows that this expression was a rational function of x. ABEL stated that it was unaffected by substituting $\theta^m(x)$ for x and considered it so obvious that he did not provide the details.[5] Thus, the expression was a rational function of the coefficients of $\phi(x) = 0$; ABEL denoted this function by a_k,

$$\sqrt[\mu]{v_k} = \frac{a_k}{v_1} \left(\sqrt[\mu]{v_1}\right)^k .$$

ABEL stated the conclusion of this investigation by giving an algebraic formula for the root x,[6]

$$x = \frac{1}{\mu}\left(-A + \sqrt[\mu]{v_1} + \frac{a_2}{v_1}\left(\sqrt[\mu]{v_1}\right)^2 + \frac{a_3}{v_1}\left(\sqrt[\mu]{v_1}\right)^3 + \cdots + \frac{a_{\mu-1}}{v_1}\left(\sqrt[\mu]{v_1}\right)^{\mu-1}\right). \tag{7.10}$$

All the other roots were contained in this formula by giving $\sqrt[\mu]{v_1}$ its μ different values $\alpha^k \sqrt[\mu]{v_1}$. ABEL expressed the implications for solubility in two theorems capturing the essence of this research. If the set of roots fell into one "orbit" of the rational expression, θ, ABEL found the equation to be solvable by radicals:

Theorem 6 *"If the roots of an algebraic equation can be represented by:*

$$x, \theta x, \theta^2 x, \ldots \theta^{\mu-1} x,$$

where $\theta^\mu x = x$ and θx denotes a rational function of x and known quantities, this equation will always be algebraically solvable."[7]

Applying this result to the particular case of irreducible equations of prime degree, which always had only one chain, ABEL found that such equations were algebraically solvable:

> "If two roots of an *irreducible* equation, of which the degree is a *prime* number, have such a relation that one can express the one *rationally* in the other, this equation will be algebraically solvable."[8]

Subsequently, ABEL refined the hypothesis that all the roots could be expressed as iterations of a rational function. That hypothesis had ensured algebraic solubility of the equation, but the same conclusion could also be established for a broader class of equations. Under the general assumption that every root of an equation, $\chi(x) = 0$,

5 The details can easily be provided by inserting into the right hand side and rearranging terms.

6 (N. H. Abel, 1829c, 142).

7 *"Si les racines d'une équation algébrique peuvent être représentées par:*

$$x, \theta x, \theta^2 x, \ldots \theta^{\mu-1} x,$$

où $\theta^\mu x = x$ et θx désigne une fonction rationelle de x et de quantités connues, cette équation sera toujours résoluble algébriquement." (ibid., 142–143).

8 *"Si deux racines d'une équation* irréductible*, dont le degré est un nombre* premier*, sont dans un tel rapport, qu'on puisse exprimer l'une* rationnellement *par l'autre, cette équation sera résoluble algébriquement."* (ibid., 143).

could be expressed rationally in a single root x, ABEL went on to assume "commutativity" of these rational dependencies, i.e. if $\theta(x)$ and $\theta_1(x)$ were any two roots of the equation $\chi(x) = 0$, written as rational expressions in x, the assumption was that

$$\theta(\theta_1(x)) = \theta_1(\theta(x)).$$

ABEL'S method of proving the algebraic solubility of $\chi(x) = 0$ under this hypothesis was to reduce the situation to the one solved above. Since all roots were known rationally once x was considered known, it sufficed to search for the root x. In order to study an *irreducible* equation, ABEL focused on the irreducible factor ϕ of χ having x as a root, repeating his concept of irreducibility (see section 7.3).

> "If the equation
>
> $$\chi x = 0$$
>
> is not irreducible, let
>
> $$\phi x = 0$$
>
> be the equation of lowest degree which the root x satisfies such that the coefficients of this equation contain nothing but known quantities."[9]

Thus, ABEL assumed that $\phi(x) = 0$ was the irreducible factor which had x as a root. By the deductions carried out above, the roots were thus expressed as (7.1), where for simplicity I write x_0 for x:

$$\theta^k(x_u) \text{ for } 0 \leq k \leq n-1 \text{ and } 0 \leq u \leq m-1.$$

The coefficients of the equation

$$\prod_{k=0}^{n-1} \left(z - \theta^k(x_0)\right) = 0 \tag{7.11}$$

could all be expressed rationally in a single quantity y_0 (above denoted y_1) which was a root of an equation (7.5) of degree m. In a footnote, ABEL demonstrated that the latter equation was irreducible. Thereby, he had reduced the determination of x to the solution of two equations of degrees n and m. Of these, he knew that the former was

9 *"Si l'équation*

$$\chi x = 0$$

n'est pas irréductible, soit

$$\phi x = 0$$

l'équation la moins élevée, à laquelle puisse satisfaire la racine x, les coëfficiens de cette équation ne contenant que des quantités connues." (ibid., 149–150).

algebraically solvable if y_0 was considered known. Although the equation of degree m

$$\prod_{u=0}^{m-1} (z - y_u) = 0 \tag{7.12}$$

giving the coefficients of (7.11) would generally not be algebraically solvable, ABEL next proved that equation (7.12) 'inherited' the property of commutative rational dependence among its roots, which $\phi(x) = 0$ possessed. Thus, a 'descent' down a string of equations was made possible.

ABEL'S proof of this 'inheritance', the commutative rational dependence among the roots of (7.12), ran as follows. The hypothesis was that all the roots were given rationally in a single root, i.e.

$$x_u = \theta_u(x_0) \text{ for } 0 \leq u \leq m-1. \tag{7.13}$$

The expression for y_u which in the previous argument was given by (7.3),

$$y_u = f\left(x_u, \theta(x_u), \ldots, \theta^{n-1}(x_u)\right) \text{ for } 0 \leq u \leq m-1,$$

under the current hypothesis became

$$y_1 = f\left(\theta_1(x_0), \theta(\theta_1(x_0)), \ldots, \theta^{n-1}(\theta_1(x_0))\right).$$

Combining this with the hypothesis of commutativity of the functions θ and θ_1, ABEL found

$$y_1 = f\left(\theta_1(x_0), \theta_1(\theta(x_0)), \ldots, \theta_1\left(\theta^{n-1}(x_0)\right)\right).$$

Therefore, y_1 was a rational and symmetric function of the sequence of roots (7.13) and could therefore be expressed rationally in y_0 and known quantities. Obviously, ABEL could carry out the same argument for any other $y_2, \ldots, y_{m-1}$. When he let $\lambda(y_0)$ and $\lambda_1(y_0)$ denote any two among the quantities $y_0, \ldots, y_{m-1}$, he found that, without loss of generality,

$$y_1 = \lambda(y_0) = f\left(\theta_1(x_0), \theta(\theta_1(x_0)), \ldots, \theta^{n-1}(\theta_1(x_0))\right) \text{ and}$$
$$y_2 = \lambda_1(y_0) = f\left(\theta_2(x_0), \theta(\theta_2(x_0)), \ldots,, \theta^{n-1}(\theta_2(x_0))\right).$$

Inserting $\theta_2(x)$ for x_0 in $\lambda(y_0)$, which transformed y_0 into y_2, ABEL obtained[10]

$$\lambda\lambda_1(y_0) = \lambda(y_2) = f\left(\theta_1\theta_2(x_0), \theta\theta_1\theta_2(x_0), \ldots, \theta^{n-1}\theta_1\theta_2(x_0)\right),$$

while inserting $\theta_1(x)$ for x_0 in $\lambda_1(y_0)$ produced

$$\lambda_1\lambda(y_0) = \lambda_1(y_1) = f\left(\theta_2\theta_1(x_0), \theta\theta_2\theta_1(x_0), \ldots, \theta^{n-1}\theta_2\theta_1(x_0)\right).$$

10 Here I deviate from my usual notation by writing the composition of functions in multiplicative mode, i.e. $\theta_1\theta_2(x_0)$ instead of $\theta_1(\theta_2(x_0))$.

Since $\theta_1\theta_2(x_0) = \theta_2\theta_1(x_0)$, ABEL concluded

$$\lambda\lambda_1(y_0) = \lambda_1\lambda(y_0),$$

and any two roots of the equation (7.12) would thus *also commute*. Therefore, the equation (7.12) determining the coefficients of (7.11) inherited this property from $\phi(x) = 0$ and could be treated in the same way as above. Since the degree was reduced by this argument, a chain of equations of strictly decreasing degrees could be constructed. At some point, where the procedure would have to terminate, the degree had to be 1 and the final equation would amount to a rational dependency.

ABEL had thus established the following important theorem on the solubility of this class of equations:

Theorem 7 *The equation $\phi(x) = 0$ is algebraically solvable* if *the following two requirements are met:*

1. *All roots of $\phi(x) = 0$ are rational expressions $\theta_1(x), \ldots, \theta_\mu(x)$ of one root*
2. *The rational expressions satisfy a requirement of commutativity $\theta_i\theta_j(x) = \theta_j\theta_i(x)$.*[11] □

Since the time of L. KRONECKER (1823–1891), equations with these properties have been named *abelian* [*abelsche*];[12] in 1932, the Springer-Verlag decided to change the first letter into a capital: *Abelian*.[13] Later, the term *Abelian* was also adopted to denote groups corresponding to *Abelian* equations, i.e. commutative groups.

In two theorems, ABEL summarized the implications for the degrees of the equations involved in the algebraic solution of the equation $\phi(x) = 0$ in which two roots were rationally related. The following theorem completely describes these degrees:

> "Supposing that the degree μ of the equation $\phi x = 0$ is decomposed as follows:
>
> $$\mu = \varepsilon_1^{v_1} \cdot \varepsilon_2^{v_2} \cdot \cdots \cdot \varepsilon_\alpha^{v_\alpha},$$
>
> where $\varepsilon_1, \varepsilon_2, \ldots, \varepsilon_\alpha$ are prime numbers, the determination of x can be effected with the help of the solution of v_1 equations of degree ε_1, v_2 equations of degree ε_2, etc., and all these equations will be algebraically solvable."[14]

11 It is remarkable and unfortunate that (Toti Rigatelli, 1994, 717) got the logic of ABEL'S reasoning wrong, reproducing the result as "he [ABEL in (N. H. Abel, 1829c)] showed that, in those equations which were solvable by radicals, all roots could be expressed as rational functions of any other root, and that these functions were permutable with respect to the four arithmetical operations. That is, if F_1 and F_2 are any two corresponding functional operations, then $F_1F_2x = F_2F_1x$."

12 (Kronecker, 1853, 6).

13 The decision prompted a brief discussion among mathematicians, see the letters (Noether→Hasse, 1932.10.29 and 1932.12.09, described at `http://www.rzuser.uni-heidelberg.de/~proquet2/HINThanoe.html`)

14 "*Supposant le degré μ de l'équation $\phi x = 0$ décomposé comme il suit:*

$$\mu = \varepsilon_1^{v_1} \cdot \varepsilon_2^{v_2} \cdot \cdots \cdot \varepsilon_\alpha^{v_\alpha},$$

où $\varepsilon_1, \varepsilon_2, \ldots \varepsilon_\alpha$ sont des nombres premiers, la détermination de x pourra s'effectuer à l'aide de la résolution de v_1 équations du degré ε_1, de v_2 équations du degré ε_2, etc., et toutes ces équations seront résolubles algébriquement." (N. H. Abel, 1829c, 152).

The resemblance to GAUSS' investigation of the cyclotomic equation is more than accidental. In multiple ways, GAUSS' work was the direct inspiration for this research. Part of the purpose of ABEL'S paper was to reproduce GAUSS'S result in this more general framework.

7.1.3 Application to circular functions and the link with GAUSS' *Disquisitiones arithmeticae*

ABEL was led to the study of *Abelian* equations by his in-depth studies of the division problem for elliptic functions (see section 7.2), which in turn were motivated by the division problem for circular functions treated by GAUSS in his *Disquisitiones arithmeticae*.[15] In the last part of the paper, ABEL incorporated GAUSS' division of the circle into his broader theory of *Abelian* equations by the following approach.

The central result of the paper *Mémoire sur une classe particulière* was contained in the second theorem (here theorem 5) on the reduction of equations of degree $m \times n$ to m solvable equations of degree n and a single equation of degree m. Originating from this theorem, ABEL deduced more particular results in various directions. Assuming that all the known quantities (i.e. coefficients) of ϕ and θ were real numbers, he studied the constructions required for the solution of the equation $\phi(x) = 0$. Considering real and imaginary parts of the radical $\sqrt[\mu]{v_1}$ (7.8), ABEL found the (non-algebraic) solution formula

$$x = \frac{1}{\mu}\left(-A + \sum_{k=1}^{\mu-1}\left(f_k + g_k\sqrt{-1}\right)\left(\sqrt{\rho}\right)^k\left(\cos\frac{k(\delta+2m\pi)}{\mu} + \sqrt{-1}\sin\frac{k(\delta+2m\pi)}{\mu}\right)\right)$$

where the quantities $\rho, A, f_1, \ldots, f_{\mu-1}, g_1, \ldots, g_{\mu-1}$ were rational functions of $\cos\frac{2\pi}{\mu}$, $\sin\frac{2\pi}{\mu}$ and the coefficients of ϕ and θ. From this, he drew the following conclusion which was intimately linked to one of GAUSS' results:

Theorem 8 *"In order to solve the equation $\phi x = 0$ it suffices:*

1) *to divide the circumference of the circle into μ equal parts,*
2) *to divide an angle δ, which can then be constructed, into μ equal parts,*
3) *to extract a square root of a single quantity ρ."*[16]

ABEL, himself, remarked that his result was an extension of one of the key results found in GAUSS' *Disquisitiones*, stating the equivalent conclusion for cyclotomic equations: That the solution of the equation $x^n = 1$ could be reduced to the following three steps:[17]

15 (C. F. Gauss, 1801).

16 *"[Q]ue pour résoudre l'équation $\phi x = 0$, il suffit:*
1) de diviser la circonférence entière du cercle en μ parties égales,
2) de diviser un angle δ, qu'on peut construire ensuite, en μ parties égales,
3) d'extraire la racine carrée d'une seule quantité ρ."
(N. H. Abel, 1829c, 144).

17 (C. F. Gauss, 1801, 454) and (C. F. Gauss, 1986, 450).

1) The division of the whole circle into $n-1$ parts ($n-1$ because the irreducible equation in GAUSS' research was $\frac{x^n-1}{x-1}=0$),
2) The division into $n-1$ parts of another arc which could be constructed after step 1 had been completed, and
3) The extraction of a square root.

The final step, the extraction of a square root, could be assumed to equal the construction of $\sqrt{n}$, GAUSS claimed without providing any proof. Later, ABEL adopted and proved the assertion.

In the fifth section of the *Mémoire sur une classe particulière*, ABEL applied his theory directly to the cyclotomic equation and the circular functions related to the division of the circle. By the addition formulae for cosine, ABEL could express $\cos ma$ rationally in $\cos a$, and assuming $\theta(\cos a) = \cos ma$ and $\theta_1(\cos a) = \cos m'a$, he obtained

$$\begin{aligned}\theta\theta_1(x) &= \theta\left(\cos m'a\right) = \cos\left(mm'a\right)\\ &= \cos\left(m'ma\right) = \theta_1(\cos ma) = \theta_1\theta(x).\end{aligned}$$

From a previously established result (here theorem 7), ABEL found that $\cos\frac{2\pi}{\mu}$ could be determined algebraically—which was a well known result.

ABEL, however, did not stop his investigations of the circular functions at this point, as he might have done had he only been interested in the algebraic solubility of the division. Assuming that $\mu = 2n+1$ was prime, ABEL studied the equation

$$\prod_{k=1}^{n}\left(X - \cos\frac{2k\pi}{2n+1}\right) = 0, \tag{7.14}$$

and used the rational dependency established above

$$\theta(\cos a) = \cos ma$$

to write

$$\theta^k(\cos a) = \cos m^k a.$$

By an argument based on GAUSS' primitive roots of the module $2n+1$, ABEL demonstrated that the roots of (7.14) were

$$x, \theta(x), \theta^2(x), \ldots, \theta^{n-1}(x) \text{ where } \theta^n(x) = x.$$

Therefore, the equation (7.14) was algebraically solvable by ABEL'S third theorem (here theorem 6), and ABEL adapted theorem 8 to this particular equation, obtaining the same result as GAUSS had found. Furthermore, ABEL presented a proof of the result, which GAUSS had only announced, that the square root extracted in step 3 could always be made to equal $\sqrt{2n+1}$ (in ABEL'S variables).

The contents of ABEL'S *Mémoire sur une classe particulière* can be summarized in the following five points depicting a descent from the general to the particular:

1. A general study of equations in which one root depended rationally on another.

2. A restriction to irreducible equations and an application of the concept of irreducibility to prove that if x and $\theta(x)$ were roots of the irreducible equation $\phi(x) = 0$, then so was $\theta^k(x)$ for all integers k.

3. A study of equations of degree $\mu = m \times n$ in which the result was obtained that the solution of such equations could be reduced to solving m algebraically solvable equations of degree n and a single (generally unsolvable) equation of degree m.

4. An application of these — and other — results to the class of *Abelian* equations, and a demonstration that these were always solvable by radicals.

5. A further application of this result to the circular functions by which GAUSS' results on the cyclotomic equation were reproduced.

ABEL had further ideas for applications of this new theory to elliptic functions, but these were not printed on this occasion (see below). In his research on *Abelian* equations, KRONECKER much later came to the conclusion that "these general Abelian equations in reality are nothing but cyclotomic equations."[18] ABEL'S paper contains, however, more than just the solubility-result for *Abelian* equations, and the general theory of the class of equations with rationally dependent roots sprung from — and had quite interesting implications for — ABEL'S approach to the theory of elliptic functions.

7.2 Elliptic functions

In his very first publication on elliptic functions entitled *Recherches sur les fonctions elliptiques*,[19] ABEL made several interesting innovations.[20] ABEL devoted a large portion of the first part of the *Recherches* to the inversion of elliptic integrals into elliptic functions, the extension of these functions into the complex domain, and the study of algebraic relations involving these functions. He derived addition formulae and studied the singularities of elliptic functions in order to address the central problem, which can be summarized in the following way:

Problem 1 (Division Problem) *Given an integer m and the value $\phi(m\beta)$ of an elliptic function of the first kind, ϕ, at $m\beta$, express $\phi(\beta)$ by radicals.* □

18 *"[...] so daß dise allgemeinen Abelschen Gleichungen im Wesentlichen nichts Anderes sind, als Kreistheilungs-Gleichungen."* (Kronecker, 1853, 11).

19 (N. H. Abel, 1827b).

20 The history of these elliptic functions and ABEL'S works on them is studied in much greater depth in part IV. For the present discussion, I am only concerned with the ideas behind ABEL'S result on the solubility of *Abelian* equations.

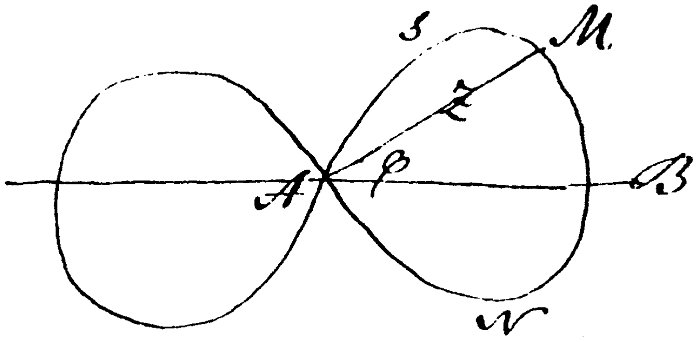


Figure 7.1: ABEL'S drawing of the lemniscate in one of his notebooks. (Stubhaug, 1996, 270)

ABEL'S inspirations for this problem were twofold. The case in which $m = 2$ and ϕ was the lemniscate function useful in measuring the arc length of the lemniscate curve (see figure 7.1)

$$\phi(x) = \int_0^x \frac{dx}{\sqrt{1-x^4}}$$

had been settled in the eighteenth century by G. C. FAGNANO DEI TOSCHI (1682–1766).[21] In his study of the equivalent problem for circular functions GAUSS had expressed his conviction that his approach would apply equally well to other transcendentals, for instance the lemniscate integral (see the quotation in section 5.3.1, p. 74).

ABEL had learned of FAGNANO DEI TOSCHI'S work and the tradition in research on elliptic integrals through his studies of the much more advanced works on the subject by EULER and A.-M. LEGENDRE (1752–1833).[22] Complementary to his generalization of FAGNANO DEI TOSCHI'S result to the bisection of elliptic functions of the first kind, ABEL gave a detailed investigation of the division of such functions into $2n+1$ parts. Reformulated in the light of the addition formulae, which he had previously developed, ABEL obtained a different version of the problem, summarized in:

Problem 2 (Division Problem) *Given n, solve the equation*

$$\phi((2n+1)\beta) = \frac{P_{2n+1}(\phi(\beta))}{Q_{2n+1}(\phi(\beta))}$$

which has degree $(2n+1)^2$. □

ABEL'S central insight was that the equation of degree $(2n+1)^2$ could be reduced to lower degree equations which were always solvable if the divisions of the periods

21 (Houzel, 1986, 298).

22 See chapter 15.

of the elliptic function were known. Addressing this division of the complete periods, ABEL demonstrated, directly inspired by GAUSS, that the roots

$$\phi^2\left(\frac{k\omega'}{2n+1}\right) \text{ for } 1 \leq k \leq n$$

could be found by solving an equation of degree $2n+2$ which might not, however, be solvable by radicals.

In the second part of the *Recherches sur les fonctions elliptiques* which appeared in 1828,[23] ABEL applied the preceding investigation to the lemniscate integral. In complete correspondence with GAUSS' result for the division of the circle, ABEL stated his result, using n in two different meanings:

> "The value of the function $\phi\left(\frac{m\omega}{n}\right)$ [the lemniscate function] can be expressed by *square roots* whenever n is a number of the form 2^n or $1+2^n$, the latter number being prime, or a product of multiple numbers of these two forms."[24]

Therefore, the division of the lemniscate into n equal parts could always be constructed by ruler and compass if n was a number of the described form.

In the *Recherches sur les fonctions elliptiques*, ABEL used direct methods to reduce the degrees and prove the solubility of the involved equations. However, as he soon realized, these properties depended on a deeper relation between the roots of the equations, and in his letters he considered the division of the lemniscate as a by-product of his research in the theory of equations.[25] As ABEL indicated in the introduction to the *Mémoire sur une classe particulière*, he had planned to apply the theory concerning these equations to elliptic functions:

> "After having presented this theory in its generality, I will apply it to circular and elliptic functions."[26]

Although no explicit application ever appeared in print (see section 7.2.1), it is not hard to see that for instance the equation

$$\prod_{k=1}^{n}\left(X-\phi^2\left(\frac{k\omega'}{2n+1}\right)\right)=0$$

falls into the category studied in the general theory because of the rational dependency expressed by the addition formulae for ϕ.

23 (N. H. Abel, 1828b).

24 "*La valeur de la fonction* $\phi\left(\frac{m\omega}{n}\right)$ *peut être exprimée par* des racines carrées *toutes les fois que* n *est un nombre de la forme* 2^n *ou* $1+2^n$*, le dernier nombre étant premier, ou même un produit de plusieurs nombres de ces deux formes.*" (ibid., 168).

25 (Abel→Holmboe, Paris, 1826/12. N. H. Abel, 1902a, 52) and (Abel→Holmboe, Berlin, 1827/03/04. ibid., 57).

26 "*Après avoir presenté généralement cette théorie, je l'appliquerai aux fonctions circulaires et elliptiques.*" (N. H. Abel, 1829c, 132).

7.2.1 The lost sections

The paper *Mémoire sur une classe particulière*,[27] which was published in the second issue of the fourth volume of A. L. CRELLE'S (1780–1855) *Journal* appearing on March 28th 1829, i.e. a few days before ABEL'S death, was not complete. At the end of the published part, following the application to circular functions, CRELLE added a footnote:

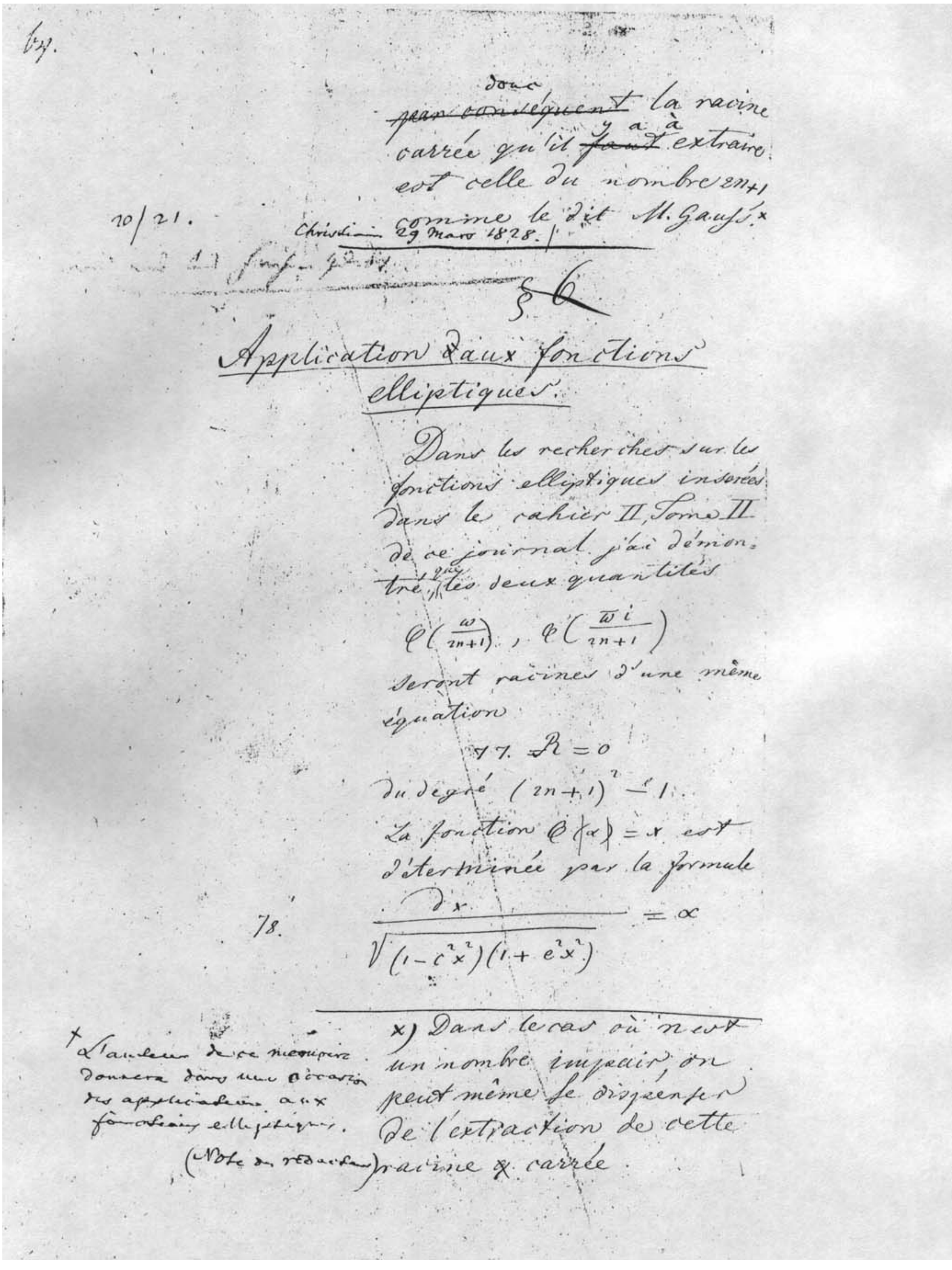
> "The author of this treatise will, on another occasion, present applications to elliptic functions."[28]

At the end of ABEL'S manuscript for the *Mémoire sur une classe particulière*,[29] the opening page of a sixth — not printed — section entitled "*Application aux fonctions elliptiques*" can still be found (see figure 7.2). In the limited space of this one page, ABEL outlined the link with the *Recherches sur les fonctions elliptiques*. Its purpose was to facilitate the application of his newly developed theory to the division problem. From a letter to CRELLE — which ABEL wrote in October 1828 — it becomes clear that ABEL *had* sent a manuscript including the application to elliptic functions to CRELLE for publication in the *Journal*.[30] Because of his intense competition with C. G. J. JACOBI (1804–1851) on elliptic functions,[31] ABEL urged CRELLE to rush publication of his sketch of his general theory of elliptic functions, the *Précis d'une théorie des fonctions elliptiques*.[32] ABEL wanted CRELLE to delay the publication of the *Mémoire sur une classe particulière*, which had been scheduled for publication in the first issue of the fourth volume, and to leave out the part concerning the application to elliptic functions.[33] CRELLE followed ABEL'S desire and published the *Mémoire sur une classe particulière* in the second issue. The *Précis d'une théorie des fonctions elliptiques* was published in the third and fourth issues of the fourth volume of the *Journal*, concluding a volume in which ABEL had published repeatedly on elliptic functions.

Unfortunately, CRELLE'S correspondence and *Nachlass* appears to have been lost.[34] Therefore, little hope remains of finding the lost sections of ABEL'S paper. Nevertheless, some information on their contents can be reconstructed from two sources: a notebook entry and the paper *Précis d'une théorie des fonctions elliptiques*.

In one of ABEL'S notebooks, a brief list of contents of the manuscript *Mémoire sur une classe particulière* was found.[35] It was intended for ABEL'S own use and carried correctly numbered references to central formulae and results, both in the published and

27 (ibid.), described above.
28 "*L'auteur de ce mémoire donnera dans une autre occasion des applications aux fonctions elliptiques.*" (ibid., 156, footnote).
29 (Abel, MS:592, 64).
30 (Abel→Crelle, Christiania, 1828/10/18. Biermann, 1967, 28–29)
31 See part IV.
32 (N. H. Abel, 1829d)
33 This letter published in 1967 thus settles the speculation of SYLOW as to why these parts were not published (L. Sylow, 1902, 18).
34 See footnote 54 on 33.
35 (Abel, MS:351:C, 52). See also (N. H. Abel, 1881, II, 310–311) and (L. Sylow, 1902, 7–8).


64.

~~par conséquent~~ donc la racine carrée qu'il ~~faut~~ y a à extraire est celle du nombre $2n+1$ comme le dit M. Gauss.×

10/21. Christiania 29 Mars 1828.

~~§ 6.~~

~~Application aux fonctions elliptiques.~~

~~Dans les recherches sur les fonctions elliptiques insérées dans le cahier II, Tome II de ce journal, j'ai démontré que les deux quantités~~

$$\varphi\left(\frac{\omega}{2n+1}\right), \quad \varphi\left(\frac{\varpi i}{2n+1}\right)$$

~~seront racines d'une même équation~~

$$77. \quad R = 0$$

~~du degré~~ $(2n+1)^2 - 1$.

~~La fonction~~ $\varphi(\alpha) = x$ ~~est déterminée par la formule~~

$$78. \quad \frac{dx}{\sqrt{(1-c^2x^2)(1+e^2x^2)}} = \alpha$$

×) Dans le cas où n est un nombre impair, on peut même se dispenser de l'extraction de cette racine × carrée

+ L'auteur de ce mémoire donnera dans une occasion des applications aux fonctions elliptiques. (Note du rédacteur)

Figure 7.2: The last page of ABEL'S manuscript for *Mémoire sur une classe particulière* (Abel, MS:592, 94) with the crossed out beginning of a sixth section. See also (N. H. Abel, 1881, II, 313).

in the missing sections. Thus it must have been produced shortly before the manuscript was sent to CRELLE. Apparently, the manuscript contained two further sections besides the five published ones. In the sixth section, concerning the application to elliptic functions, ABEL listed the result that if

$$\frac{m^2+2n+1}{2\mu+1}$$

was integral, the complex division

$$\phi\left(m-\alpha i\right)\frac{\omega}{2n+1}$$

could be effected by solving a μ^{th} degree equation.[36] This is a generalized version of the division problem treated above. The seventh section concerned the transformations of elliptic functions; a topic which also constituted a major part of ABEL'S competition with JACOBI. In the notebook, ABEL listed a number of formulae capturing central results. In order to produce a reliable interpretation, ABEL'S works in the transformation theory of elliptic functions have to be taken into consideration.[37]

7.3 The concept of irreducibility at work

Of central importance to ABEL'S research in the theory of equations was his use of the concepts of irreducibility and divisibility. In his *Disquisitiones arithmeticae*, GAUSS devoted a paragraph to the following result concerning the equation $X = 0$ which corresponded to the system Ω of imaginary n^{th} roots of unity:

> "Theory of the roots of the equation $x^n - 1 = 0$ (where n is assumed to be prime).
>
> Except for the root 1, the remaining roots contained in (Ω) are included in the equation $X = x^{n-1} + x^{n-2}$+etc.$+x+1=0$.
>
> The function X cannot be decomposed into lower factors in which all the coefficients are rational."[38]

GAUSS demonstrated the indecomposibility of X by an *ad hoc* argument and did not put it to central use later in the proof (section 5.3). In ABEL'S impossibility proof, numerous allusions to irreducibility had been made; however, they all served as simplifications and not as central concepts (see section 6.3.3).[39] By 1829 ABEL promoted the concept into a fundamental one on which theorems could be built. ABEL'S definition of irreducibility was intended to capture the same property as GAUSS had demonstrated for X, although ABEL spoke of *irreducible equations* where GAUSS had spoken

36 (Abel, MS:351:C, 52).

37 This theme is taken up in part IV.

38 "*Theoria radicum aequationis* $x^n - 1 = 0$ *(ubi supponitur,* n *esse numerum primum).*
Omittendo radicem 1*, reliquae* (Ω) *continentur in aequatione* $X = x^{n-1} + x^{n-2} +$ *etc.* $+x+1=0$.
Functio X *resolvi nequit in factores inferiores, in quibus omnes coefficients sint rationales.*" (C. F. Gauss, 1801, 417); English translation from (C. F. Gauss, 1986, 412).

39 (N. H. Abel, 1826a, 71).

of *indecomposable functions*. This switch from polynomial functions to their associated equations was not uncommon, and is mainly a distinction in *terms*. ABEL gave his first definition of irreducibility in a footnote in the paper on *Abelian* equations:

> "An equation $\phi x = 0$, in which the coefficients are rational functions of a certain number of known quantities $a, b, c, \ldots$, is called *irreducible* when it is impossible to express any of its roots by an equation of lower degree, in which the coefficients are also rational functions of $a, b, c, \ldots$."[40]

The first — and highly useful — theorem which ABEL demonstrated with this definition was that no equation could share a root with an irreducible one without having all the roots of the irreducible equation as roots. In the following section, I describe ABEL'S proof and use of this important theorem.

7.3.1 EUCLID's division algorithm

Formulated in the terminology of the *Mémoire sur une classe particulière*, the central theorem on irreducible equations was the following one expressing the property described above:

Theorem 9 *"If one of the roots of an irreducible equation, $\phi x = 0$, satisfies another equation, $fx = 0$, where fx denotes a rational function of x and known quantities which are supposed contained in ϕx, this latter equation will also be satisfied if instead of x any other root of the equation $\phi x = 0$ is inserted."*[41]

ABEL gave a proof of this theorem — again relegated to a footnote — which is a beautiful application of the division algorithm much along the lines of a modern argument. Because f was a rational function, ABEL could write it as

$$f = \frac{M}{N}, \tag{7.15}$$

where M and N were entire functions of x. But, as ABEL noticed, "any [polynomial] function of x can always be put on the form $P + Q \cdot \phi x$ where P and Q are entire functions such that the degree of P is less than that of the function ϕx."[42] This application of the division algorithm with remainder was well known to ABEL and received no

[40] "*Une équation $\phi x = 0$, dont les coefficients sont des fonctions rationnelles d'un certain nombre de quantités connues $a, b, c, \ldots$ s'appelle* irréductible*, lorsqu'il est impossible d'exprimer aucune de ses racines par une équation moins élevée, dont les coefficiens soient également des fonctions rationnelles de $a, b, c, \ldots$.*" (N. H. Abel, 1829c, 132, footnote).

[41] "*Si une des racines d'une équation irréductible $\phi x = 0$ satisfait à une autre équation $fx = 0$, où fx désigne une fonction rationnelle de x et des quantités connues qu'on suppose contenues dans ϕx; cette dernière équation se trouvera encore satisfaite en mettant au lieu de x une racine quelconque de l'équation $\phi x = 0$.*" (ibid., 133).

[42] "*mais une fonction de x peut toujours être mise sous la forme $P + Q \cdot \phi x$, ou P et Q sont des fonctions entières, telles, que le degré de P soit moindre que celui de la fonction ϕx.*" (ibid., 132–133, footnote).

further comment.[43] By inserting into (7.15), ABEL found

$$f(x) = \frac{P + Q \cdot \phi(x)}{N}. \tag{7.16}$$

Next, he let x denote a common root of ϕ and f and concluded that x would also be a root of $P = 0$. However, if P were not identically zero, "this equation gives x as a root of an equation of degree less than that of $\phi x = 0$; which is a contradiction of the hypothesis. Therefore, $P = 0$ and it follows that $fx = \phi x \cdot \frac{Q}{N}$."[44] Thus, it was obvious that f would vanish whenever ϕ did and, therefore, that any root of $\phi(x) = 0$ would also be a root of $f(x) = 0$.

ABEL put this important theorem to use in the very first description of the equations treated in the *Mémoire sur une classe particulière*. If x' and x were two roots of the *irreducible* equation $\phi(x) = 0$ among which a rational dependency existed,

$$x' = \theta(x),$$

then every iterated application of θ to x would also be a root of this equation. ABEL'S demonstration followed directly from the theorem above. He argued that since it followed from the hypothesis that the equations

$$\phi(\theta(x)) = 0 \text{ and } \phi(x) = 0$$

had a root, x, in common, theorem 7.3 stated that for any root, y, of $\phi(x) = 0$, $\theta(y)$ would also be a root of that equation. Once he had established this result, the argument of ABEL'S paper was on its way, and the complex of conclusions described above could be obtained.

ABEL turned the concept of irreducibility of equations, which had existed as an *ad hoc* tool before into a central foundation upon which a building of theorems could be established.[45] The irreducibility in ABEL'S sense was defined as minimality of the equation expressing the roots under the restriction that the coefficients must depend rationally on the same quantities as the original equation. From this definition, generalizations were later made toward the general concept of *domain of rationality*. But working with this definition — and the division algorithm of EUCLID ($\sim$295 B.C.) — ABEL demonstrated the important theorem 9 of divisibility, which in turn established the basic property of the class of equations studied in the *Mémoire sur une classe particulière*.[46]

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43 It had been explicitly employed in GAUSS' second proof of the *Fundamental Theorem of Algebra* (see section 5.7).

44 "*cette équation donnera x, comme racine d'une équation d'un degré moindre que celui de $\phi x = 0$; ce qui est contre l'hypothèse; donc $P = 0$ et par suite $fx = \phi x \cdot \frac{Q}{N}$.*" (ibid., 133,footnote).

45 See also (L. Sylow, 1902, 23–24).

46 (N. H. Abel, 1829c).

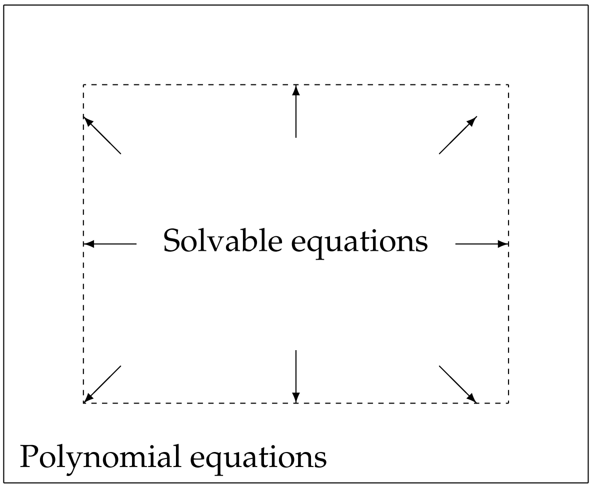

Figure 7.3: Extending the class of solvable equations: *Abelian* equations

7.4 Enlarging the class of solvable equations

ABEL considered the positive result demonstrating the solubility of certain equations as a counterpart to the insolubility of higher degree general equations. In the introduction to the *Mémoire sur une classe particulière*, ABEL wrote:

> "It is true that the algebraic equations are not generally solvable, but there is a particular class of each degree for which the algebraic solution is possible."[47]

To this class of solvable equations belonged the equations of the form $x^n - 1 = 0$ studied by GAUSS and the generalizations of these obtained by ABEL in the paper. Only few other equations were explicitly known to be solvable, and ABEL'S result can thus be seen to provide a demonstration that the total class of solvable equations had a certain range. In the limitation-enlargement model suggested in section 6.8, the situation can be described by figure 7.3 and much of ABEL'S research to describe the precise extent of *solubility* can be interpreted in this context. In a letter to B. M. HOLMBOE (1795–1850) written during his stay in Paris, ABEL described the problem and his progress:

> "I am currently working on the theory of equations, which is my favorite theme, and have finally reached a point where I see a way to solve the following general problem: To determine the form of all algebraic equations which can be solved algebraically. I have found an infinitude of the fifth, sixth, seventh, etc. degree which had never been smelled before."[48]

47 "*Il est vrai que les équations algébriques ne sont pas résolubles généralement; mais il y en a une classe particulière de tous les degrés dont la résolution algébrique est possible.*" (N. H. Abel, 1829c, 131).

Thus, ABEL'S two publications on the theory of equations which appeared during his lifetime contributed a negative, limiting result of insolubility of the *general* higher degree equations and a positive, enlarging result of the solubility of a *certain* class of equations of all degrees. The program set out above in the letter to HOLMBOE was pursued by ABEL from his time in Paris, and traces of it can be found in his notebooks. However, his correspondence also announced further and far-reaching results for which no detailed studies or proofs have been recovered. The determination of the exact extension of the concept of algebraic solubility was approached by ABEL through a theory largely based on the same tools as his published works, but never completed nor published in his lifetime. Therefore, the solution to this fundamental problem is rightfully attributed to E. GALOIS (1811–1832). In the next chapter, ABEL'S steps toward a general theory of solubility are analyzed against the background of his other works and GALOIS' contemporary ideas.

[48] "*Jeg arbeider nu paa Ligningernes Theorie, mit Yndlingsthema og er endelig kommen saa vidt at jeg seer Udvei til at løse følgende alm: Problem. Determiner la forme de toutes les équation algébriques qui peuvent être resolues algebriquement. Jeg har fundet en uendelig Mængde af 5te, 6te, 7de etc. Grad som man ikke har lugtet indtil nu.*" (Abel→Holmboe, Paris, 1826/10/24. N. H. Abel, 1902a, 44).

Chapter 8

A grand theory *in spe*: algebraic solubility

In his correspondence with A. L. CRELLE (1780–1855) and B. M. HOLMBOE (1795–1850), N. H. ABEL (1802–1829) announced numerous results in the theory of equations beyond the impossibility of solving the quintic and the study of *Abelian* equations. Some concerned the form of solutions to algebraically solvable equations of the fifth degree,[1] others dealt with solubility results for broader classes of equations,[2] and yet others testify to ABEL'S general progress in his program of determining the form of solvable equations.[3] The information provided in the letters is complemented by a notebook entry dating from 1828 which has been included in both editions of the *Œuvres* under the title *Sur la résolution algébrique des équations*.[4] The entry begins as a manuscript almost ready for press, but after some introductory remarks, a few theorems and some deductions it turns from its initial thoroughness and clarity to nothing but bare calculations. Nevertheless, when considered together, these sources give an impression of the methods and extent of the general theory of algebraic solubility which ABEL set out to develop in the last years of his life.

8.1 Inverting the approach once again

The notebook manuscript dealt with the general form of algebraically solvable equations. In one of the two lengthy introductions which ABEL wrote for this work the problem was clearly set out:

> "Given an equation of any given degree, to determine whether or not it could be satisfied algebraically."[5]

1 (Abel→Crelle, Freyberg, 1826/03/14. N. H. Abel, 1902a, 21–22).
2 (Abel→Crelle, Christiania, 1828/08/18. ibid., 72–73).
3 (Abel→Holmboe, Paris, 1826/10/24. ibid., 44–45).
4 (N. H. Abel, [1828] 1839).
5 "*Une équation d'un degré quelconque étant proposée, reconnaître si elle pourra être satisfaite algébriquement, ou non.*" (N. H. Abel, 1881, vol. 2, 330).

ABEL'S initial step in solving this general problem was to reformulate it in the following program which he described in the introduction to the other version of the manuscript.

> "From this, the following two problems stem naturally whose complete solution comprises the entire theory of the algebraic solution of equations, namely:
> 1) To find all equations of any determinate degree which are algebraically solvable.
> 2) To decide whether or not a given equation is algebraically solvable."[6]

Thus, the problem of determining the algebraic solubility of equations had been inverted once again. In principle, ABEL'S program amounted to listing — by some descriptive form — *all* equations of a certain degree which could be solved algebraically and then deducing whether any given equation was in this list. In pursuing this problem, ABEL focused on a given algebraic expression, a radical, and sought to describe the *irreducible* equation which it satisfied. In doing so, the concept of *irreducibility* acquired its second importance in ABEL'S research as a means of obtaining *the* equation linked to a given radical. This shift from working with equations of which some representation was known, either as a fifth degree polynomial or as relations among its roots, to general equations which were only characterized by their external structure as being irreducible is what I consider a second inversion of approach.

ABEL'S inversion was intimately connected to a general consideration on mathematical methodology. In his introduction, he described this inversion of approach in a much quoted paragraph:

> "To solve these equations [of the first four degrees], a uniform method was discovered which, it was thought, was applicable to an equation of any degree; but in spite of all the efforts of a *Lagrange* and other distinguished geometers, the proposed goal could not be reached. This led to the assumption that the solution of the general equation was algebraically impossible; but this could not be decided since the adopted method had only been able to lead to reliable conclusions in the case in which the equations were solvable. In fact, one proposed to solve the equations without knowing if that was possible. In this case, one might come to the solution although that was not certain at all; but if by misfortune the solution was impossible, one might search an eternity without finding it. To infallibly reach anything in this matter, it is necessary to follow another route. One should give the problem such a form that it will always be possible to solve it, which can always be done for any problem. Instead of demanding a relation, of which the existence is unknown, one should ask whether such a relation is possible at all."[7]

---

6 *"De là dérivent naturellement les deux problèmes suivans, dont la solution complète comprend toute la théorie de la résolution algébrique des équations, savoir:*
1) Trouver toutes les équations d'un degré déterminé quelconque qui soient résolubles algébriquement.
2) Juger si une équation donnée est résoluble algébriquement, ou non."
(N. H. Abel, [1828] 1839, 218–219).

7 *"On découvrit pour résoudre ces équations une méthode uniforme et qu'on croyait pouvoir appliquer à une équation d'un degré quelconque; mais malgré tous les efforts d'un* Lagrange *et d'autres*

As previously noted, ABEL'S belief that any problem could be converted into a solvable one was held by most mathematicians throughout the 19th century. It became prominent in the so-called *Hilbert Programme* before the development of axiomatics stressed that decidability could only be asked and answered relatively to the (axiomatic) system in which the problem was embedded.

With ABEL'S new driving question, modified from the ones motivating the impossibility proof and the study of *Abelian* equations, it was his intention to explore the grey area between the entire set of equations and the ones known to be solvable (see figures 6.1 and 7.3). ABEL'S hope was to delineate the border line between solvable and unsolvable equations by some external characteristic.

8.2 The construction of the irreducible equation satisfied by a given expression

The general program of ABEL'S research was to construct a list of all irreducible solvable equations and subsequently match any given equation against this list. His attempt at implementing this scheme consisted of a construction of the irreducible equation satisfied by a given algebraic expression. After establishing certain properties of this equation from the expression which satisfies it, ABEL returned to the problem of determining whether a given equation was solvable or not.

The first part of ABEL'S notebook manuscript contained theorems and results presented in a clear and deductive manner. Their contents showed frequent similarities with the opening studies of the form of algebraic expressions satisfying an equation as carried out in the impossibility proof (see section 6.3.3). If anything, the 1828 notebook lacked — by comparison to the impossibility proof — the clear, albeit defective, classification of algebraic expressions of which only reminiscences were given. The classification established in the notebook was insufficient to cover some of the required deductions, and it is possible that ABEL, himself, had noticed this deficiency (see below).

Basic concepts. In the opening section of the manuscript proper (following a lengthy introduction), ABEL outlined his own characterization of algebraic expressions which

---

géomètres distingués on ne put parvenir au but proposé. Cela fit présumer que la résolution des équations générales était impossible algébriquement; mais c'est ce qu'on ne pouvait pas décider, attendu que la méthode adoptée n'aurait pu conduire à des conclusions certaines que dans le cas où les équations étaient résolubles. En effet on se proposait de résoudre les équations, sans savoir si cela était possible. Dans ce cas, on pourrait bien parvenir à la résolution, quoique cela ne fût nullement certain; mais si par malheur la résolution était impossible, on aurait pu la chercher une éternité, sans la trouver. Pour parvenir infailliblement à quelque chose dans cette matière, il faut donc prendre une autre route. On doit donner au problème une forme telle qu'il soit toujours possible de le résoudre, ce qu'on peut toujours faire d'une problème quelconque. Au lieu de demander une relation dont on ne sait pas si elle existe ou non, il faut demander si une telle relation est en effet possible." (ibid., 217).

could occur in the solution of a solvable equation. This characterization had been one of the points of objection to his impossibility proof of 1826 (see section 6.9.1). However, only the objections raised by E. J. KÜLP (⋆1801) were known to ABEL and ABEL did not react directly to them in the notebook. The characterization which ABEL presented in the notebook was only a limited version of the one found in the impossibility proof. In the notebook, ABEL described the radicals from the outer-most one inward in the following form

$$y = P_0 + P_1 \cdot R_1^{\frac{1}{\mu_1}} + P_2 \cdot R_1^{\frac{2}{\mu_1}} + \cdots + P_{\mu_1-1} \cdot R_1^{\frac{\mu_1-1}{\mu_1}}, \tag{8.1}$$

in which $P_0, \ldots, P_{\mu_1-1}$ and R_1 were rational expressions in known quantities and the other radicals $R_2^{\frac{1}{\mu_2}}, R_3^{\frac{1}{\mu_3}}, \ldots$. In relation to the route he had taken in the impossibility proof, he abandoned the concept of degree of algebraic expressions and imposed only the hierarchy from the concept of order which counted the number of nested root extractions of prime degree.

ABEL introduced three notational concepts which he used throughout the preliminary part of the manuscript to simplify his notation:

1. He chose to denote algebraic expressions by writing their order as subscripts, for instance writing A_m for an algebraic expression A of order m.

2. With y being of the form (8.1) and $\phi(y) = 0$ an equation satisfied by y, ABEL chose to write the equation as $\phi(y, m) = 0$ if all the coefficients of $\phi(y)$ were algebraic expressions of order m. Furthermore, he denoted the degree of the equation by $\delta\phi(y, m)$.

3. Most importantly, he introduced a symbol $\prod A_m$ for the product of all values of A_m obtained from attributing to the outermost radical in A_m, $R^{\frac{1}{\mu}}$, all its possible values, $R^{\frac{1}{\mu}}, \omega R^{\frac{1}{\mu}}, \ldots, \omega^{\mu-1} R^{\frac{1}{\mu}}$ (ω a μ^{th} root of unity). Thus, if

$$A_m = \sum_{k=0}^{\mu-1} p_k R^{\frac{k}{\mu}},$$

the new symbol denoted the expression

$$\prod A_m = \prod_{u=0}^{\mu-1} \left(\sum_{k=0}^{\mu-1} p_k \omega^{uk} R^{\frac{k}{\mu}} \right).$$

Using these concepts and a number of immediate consequences derived from them, ABEL constructed and characterized the irreducible equation associated with a given algebraic expression.

Lemmata. In the first lemma, ABEL obtained a result which had played a central role in his impossibility proof. It stated that if the equation

$$\sum_{u=0}^{\mu-1} t_u y_1^{\frac{u}{\mu_1}} = 0 \tag{8.2}$$

could be satisfied, in which the coefficients $t_0, \ldots, t_{\mu-1}$ were rational functions of ω, known quantities (i.e. coefficients of the equation $\phi(y) = 0$), and lower order radicals, then all the coefficients had to vanish, i.e. $t_0 = t_1 = \cdots = t_{\mu-1} = 0$ (cmp. lemma 1). By and large, the proof resembled the one given in 1826 (see section 6.3.3) but differed when ABEL had to eliminate the possibility of a first degree irreducible factor. Letting $z = y^{\frac{1}{\mu_1}}$, ABEL assumed that the irreducible factor (corresponding to an irreducible equation) $\sum_{u=0}^{\kappa} s_n z^n$ divided (8.2) and excluded the possibility of $k \geq 2$. The case of a first degree irreducible factor was briefly dismissed by the following argument:

> "Thus, it is necessary that $k = 1$, but that gives
>
> $$s_0 + z = 0$$
>
> from which
>
> $$z = \sqrt[\mu_1]{y_1} = -s_0,$$
>
> which is similarly impossible."[8]

As P. L. M. SYLOW (1832–1918) has remarked, the conclusion that $z = -s_0$ is impossible is essentially correct,[9] it can be supported if an improved hierarchy is imposed on the radicals.[10] Again, ABEL'S notebook does not contain all the technical details of his deductions.

ABEL put forward another important proposition when he claimed that the roots of satisfiable equations come in "bundles". He stated that if the equation

$$\phi(y, m) = 0 \tag{8.3}$$

was satisfied by an algebraic expression of order n

$$y = \sum_{k=0}^{\mu-1} p_k y_1^{\frac{k}{\mu_1}},$$

8 *"Il faut donc que $k = 1$, or cela donne*

$$s_0 + z = 0$$

d'où

$$z = \sqrt[\mu_1]{y_1} = -s_0,$$

ce qui est de même impossible." (N. H. Abel, [1828] 1839, 229).

9 (Sylow in N. H. Abel, 1881, vol. 2, 332).

10 As shown by (Holmboe in N. H. Abel, 1839, vol. 2, 289) and (Maser in Abel and Galois, 1889, 149).

it would also be satisfied if $\omega^u y_1^{\frac{1}{\mu}}$ were inserted for $y_1^{\frac{1}{\mu}}$ (ω a μ^{th} root of unity). He gave no explicit proof of this result, which is a simple consequence of the vanishing of the coefficients of (8.2).[11] The result provided the important connection that any root of $\prod \phi(y, m) = 0$ would also be a root of $\phi(y, m) = 0$.

The manuscript also contains the fundamental characterization of irreducible equations that no equation can share a root with an irreducible equation without the latter dividing the former (cmp. theorem 7.3). ABEL derived this along the lines described in section 7.3 but applied the terminology developed in the manuscript. By implicit application of the Euclidean division algorithm, ABEL demonstrated that if the equations

$$\phi(y, m) = 0 \text{ and } \phi_1(y, n) = 0$$

had a common root, ϕ was assumed to be irreducible, and $n \leq m$, then

$$\phi_1(y, n) = \phi(y, m) \cdot f(y, m).$$

Properties of $\prod \phi(y, m)$. In his subsequent argument, ABEL sought to describe the irreducible equation satisfied by a given algebraic expression. The central tool employed was his *construction* of this equation based on the construction of $\prod \phi(y, m)$ and the demonstration of its properties. The construction which ABEL gave was mainly existential; it amounted to proving the existence of an equation having specific useful properties.

Continuing to build upon the fundamental result on irreducible equations, ABEL proved the following theorem.

Theorem 10 *If*

$$\phi_1(y, n) = f(y, m) \cdot \phi(y, m),$$

then for some m'

$$\phi_1(y, n) = f_1(y, m') \cdot \prod \phi(y, m). \qquad \square$$

ABEL'S proof was elegant and made prototypical usage of the previously established theorems and the concept of the outer-most radical. Denoting by $\sqrt[\mu]{y_1}$ the outermost root extraction of $\phi(y, m) = 0$, c.f. (8.1), this equation would also be satisfied if $\omega^k \sqrt[\mu]{y_1}$ were substituted for $\sqrt[\mu]{y_1}$ where ω was a μ^{th} root of unity. Consequently, $\omega^k \sqrt[\mu]{y_1}$ was a root of ϕ and, therefore, also of ϕ_1. Thus, ϕ_1 would have the different roots of ϕ corresponding to different values of k as roots. As ABEL noticed, if these factors corresponding to different values of k had no common factors (were relatively prime), their product would also be a factor of ϕ and the proof had been completed. In the impossibility proof of 1826, ABEL had stated this result, which translated into the notation of the manuscript concludes that "it is clear that the given equation must

11 See for instance (Holmboe in N. H. Abel, 1839, vol. 2, 289).

be satisfied by all values of y which are obtained by attributing to $y_1^{\frac{1}{\mu}}$ all the values $\omega y_1^{\frac{1}{\mu}}, \omega^2 y_1^{\frac{1}{\mu}}, \ldots, \omega^{n-1} y_1^{\frac{1}{\mu}}$"[12] (see section 6.3.3). In 1826, it had been given no proof, but in the notebook, ABEL provided the proof as an easy and elegant application of the fundamental concepts and tools.

ABEL proceeded by establishing a central link between the irreducibility of $\phi(y,m) = 0$ and that of $\prod \phi(y,m) = 0$.

Theorem 11 *If the equation*

$$\phi(y,m) = 0$$

is irreducible, then so is the equation

$$\phi_1(y,m) = \prod \phi(y,m) = 0. \qquad \square$$

ABEL argued for this theorem by a *reductio ad absurdum* proof against which SYLOW later raised well founded objections. ABEL assumed that ϕ_1 was reducible and that $\phi_2(y,m')$ was an irreducible[13] factor of $\prod \phi(y,m) = 0$. Under these assumptions, ϕ_2 and ϕ would have a common root since all the roots of $\prod \phi$ were also roots of ϕ. The assumed irreducibility of ϕ then enabled ABEL to conclude that because the irreducible ϕ and ϕ_2 had a root in common, ϕ would be a factor of ϕ_2,

$$\phi_2(y,m') = f(y,m) \cdot \phi(y,m).$$

This in turn implied (by theorem 10)

$$\phi_2(y,m') = f_1(y,m'') \cdot \underbrace{\prod \phi(y,m)}_{=\phi_1(y,m)}. \tag{8.4}$$

On the other hand, ϕ_2 had been assumed to be an irreducible factor of ϕ_1 implying $\deg \phi_2 < \deg \phi_1$, which contradicted (8.4).

SYLOW'S objections concerned the properties of $\prod \phi$. Besides certain points, at which ABEL left out assumptions of irreducibility, SYLOW noticed that ABEL tacitly assumed that $\phi(y,m)$ did not have factors in which all the coefficients were rational expressions in inner radicals and known quantities. If such factors were involved, the equation $\prod \phi(y,m) = 0$ might turn out to be a *power* of an irreducible equation.[14] SYLOW repaired ABEL'S argument by refining his hierarchy of algebraic expressions.

---

12 "*[...] so ist klar, daß der gegebenen Gleichung durch alle die Werthe von y genug werden muß, welche man findet, wenn man der Größe $p^{\frac{1}{n}}$ alle die Werthe $\alpha p^{\frac{1}{n}}, \alpha^2 p^{\frac{1}{n}}, \ldots, \alpha^{n-1} p^{\frac{1}{n}}$ beilegt.*" (N. H. Abel, 1826a, 72).

13 Actually, ABEL did not, presumably inadvertently, state the condition of irreducibility of ϕ_2.

14 (Sylow in N. H. Abel, 1881, vol. 2, 332).

Construction of the irreducible equation. With the first theorems and the lemmata described above, ABEL was in a position to give a construction of the irreducible equation which a given algebraic expression satisfied. More importantly, this construction allowed him to demonstrate that central properties of this equation could be deduced from properties of the initially given algebraic expression. ABEL let

$$a_m = f\left(\sqrt[\mu_m]{y_m}, \sqrt[\mu_{m-1}]{y_{m-1}}, \dots \right)$$

denote a given algebraic expression and constructed the *irreducible* equation $\psi(y) = 0$ which would have a_m as a root in the following way.

Since a_m was to satisfy $\psi(y) = 0$, it would be necessary that $y - a_m$ was a factor of ψ. By the theorem 10, it followed that

$$\phi_1(y, m_1) = \prod (y - a_m)$$

would also be a factor. Because $y - a_m$ was a first degree polynomial and, therefore, irreducible, it followed that ϕ_1 was also irreducible (by theorem 11). Consequently, ϕ_1 was an irreducible factor of $\psi(y)$ and the procedure could be repeated yielding a sequence of irreducible factors

$$\phi_n(y, m_n) = \prod \phi_{n-1}(y, m_{n-1}),$$

in which the radicals of a_m were sequentially removed by the analogue of multiplying with the complex conjugate (c.f. section 6.3.2).

ABEL claimed that the sequence of positive integers $m_1, m_2, \dots$ was decreasing but gave no explicit argument. However, by J. L. LAGRANGE'S (1736–1813) theorem (section 5.2.3) it is not hard to see that $\prod a_m$ is a rational function of y_m and the inner radicals involved. Therefore, the order of $\prod a_m$ is less than the order of a_m. Thus, at a certain point (after, say, u steps) the sequence $m_1, m_2, \dots$ had to vanish, and an equation would be obtained in which all the coefficients were rationally known. This equation was the sought-for $\psi(y) = 0$,

$$\psi(y) = \phi_u(y, 0) = \prod \phi_{u-1}(y, m_{u-1}).$$

Directly from this construction, ABEL deduced his characterization of the irreducible equation satisfied by a given algebraic expression, laying the foundations for his further reasoning. He summarized the properties in the following four points:[15]

Proposition 1 *The following four results link properties of the irreducible equation $\psi(y) = 0$ satisfied by a given algebraic expression a_m to properties of the expression itself:*

1. *The degree of ψ is the product of certain exponents of root extractions occurring in a_m. Among these exponents, the one of the outer-most root extractions is always present.*

15 (N. H. Abel, [1828] 1839, 232–233)

2. *The exponent of the outer-most root extraction divides the degree of ψ [actually contained in 1, above].*

3. *If ψ can be algebraically satisfied, it is also algebraically solvable. All its roots are obtained by attributing to the root extractions $y_{m_u}^{\frac{1}{\mu_u}}$ all their possible values.*

4. *If the degree of ψ is μ, the expression a_m may have μ, and no more than μ, values.* □

ABEL'S deduction of these properties was straightforward from considerations on the exponents of involved root extractions and the construction described. A formal consideration of the uniqueness of the irreducible equation constructed was not carried out, but must have seemed obvious to ABEL.

8.3 Refocusing on the equation

The first theorems and the construction of the irreducible equation connected to a given algebraic expression are fascinating pieces of mathematics revealing traces of ABEL'S profound ideas. Whereas the presentation of these fundamental results was lucid — and basically acceptable to present day mathematicians — ABEL'S following investigations in the notebook took another form. As he progressed farther from the well established results founded in the theory of LAGRANGE, his explanatory remarks and general narrative became ever more sparse until they finally ceased altogether. However, ABEL'S notebook is the only source illustrating how he planned to proceed, and I will try to reconstruct the central result of these investigations, which was never presented in a form intended for publication.

Because ABEL'S argument, from this point onward, consists of little but equations, I have reconstructed how he *could*, with his tools and methods, have argued. In limiting myself to ABEL'S argument for the reduction of the general problem to *Abelian* equations, I remain close to the sources. ABEL'S unfinished manuscript inspired mathematicians of the nineteenth century — such as C. J. MALMSTEN (1814–1886), SYLOW, and L. KRONECKER (1823–1891) — to elaborate and extend the investigation;[16] recently, L GÅRDING and C. SKAU have taken up the problem anew.[17]

SYLOW has speculated that ABEL recognized the insufficiency of his description of algebraic expressions. In response to his realization, ABEL should, according to SYLOW, have abandoned his attempt at presenting a manuscript ready for printing and instead recorded his further findings in the order and form in which he came to them.[18] As also noted by SYLOW, this change in style of presentation was not uncommon. In his notebooks, ABEL frequently started out writing coherent manuscripts,

16 (Malmsten, 1847), (Sylow, 1861), (L. Sylow, 1902, 18–22), and (Kronecker, 1856).
17 (Gårding, 1992) and (Gårding and Skau, 1994).
18 (L. Sylow, 1902, 19).

which gradually turned into a sequence of formulae.[19] At a later time, when the ideas had matured and proofs had been improved, the results emerged in another manuscript or in print.

In the third section of the *Sur la résolution algébrique des équations*, which was entitled "On the form of algebraic expressions which can satisfy an irreducible equation of a given degree", ABEL reverted his approach once again. In the impossibility proof, he had fixed the equation (the general quintic) and sought to describe any algebraic expression which could satisfy it. In the opening part of the notebook manuscript, he had reversed this approach in order to describe the *simplest* equation which a given algebraic expression could satisfy; ABEL'S concept of *simplicity* was, of course, that of *irreducibility*. But in this third section, ABEL once again fixed the *equation*

$$\phi(y) = 0 \tag{8.5}$$

of degree μ and tried to analyse the form of any algebraic expression a_m of order m which could satisfy it.

ABEL'S attention restricted to equations of prime degree. ABEL'S ambition had been to treat — in all its generality — all degrees μ. From his correspondence there is some indication that he made some progress in solving this general problem.[20] However, the notebook manuscript only contains conclusive arguments concerning the simpler case in which μ was a prime. The pivotal tools in ABEL'S investigations were the results on the constructed irreducible equation, summed up in the proposition 1 above, and his penetrating knowledge of properties of *Abelian* equations (see chapter 7).

For ABEL, the first — and most important — consequence of assuming μ prime was to rewrite a_m in accordance with proposition 1:2. Writing s in place of y_m above, he found

$$a_m = \sum_{k=0}^{\mu-1} p_k s^{\frac{k}{\mu}},$$

which follows from the fact that the exponent of the outer-most root extraction in a_m had to divide μ. The proposition 1:3 furthermore stated that the other roots of (8.5) could be obtained by inserting $\omega^u s^{\frac{1}{\mu}}$ for $s^{\frac{1}{\mu}}$. ABEL denoted[21] these μ roots $z_0, \ldots, z_{\mu-1}$,

$$z_u = \sum_{k=0}^{\mu-1} p_k \omega^{uk} s^{\frac{k}{\mu}} \text{ for } 0 \le u \le \mu - 1.$$

Since each of these was a root of the equation (8.5), they had to remain unaltered when all the root extractions in $p_0, \ldots, p_{\mu-1}, s$ were given all their respective possible values, ABEL argued from proposition 1:3.

19 (L. Sylow, 1902, 8).
20 (Abel→Holmboe, Berlin, 1827/03/04. N. H. Abel, 1902a, 57).
21 I have chosen to enumerate them starting from zero, whereas ABEL began with the number 1. The benefit of my enumeration is simplicity of the subsequent formulae.

The coefficients $p_0, \ldots, p_{\mu-1}$ depended rationally upon s. In the following, ABEL investigated the dependency of the coefficients $p_0, \ldots, p_{\mu-1}$ upon s. ABEL linked the choice of other root extractions[22] in the expressions for $p_0, \ldots, p_{\mu-1}$ to permutations of the roots $z_0, \ldots, z_{\mu-1}$ in a way resembling the auxiliary theorem 3 of the impossibility proof. There, ABEL had used results obtained from permuting the roots to demonstrate that any radical occurring in a supposed solution formula would have to be a rational function of the roots of the equation. A similar result was needed in this context which was more general than the quintic studied in 1826. Although his explicit calculations took another form, the underlying ideas of the reworking remain the same.

ABEL based his argument on letting[23] $\hat{p}_0, \ldots, \hat{p}_{\mu-1}, \hat{s}$ denote any set of values of $p_0, \ldots, p_{\mu-1}, s$ corresponding to choosing other roots of unity in the algebraic expressions for $p_0, \ldots, p_{\mu-1}, s$. The above argument ensuring that z_0 was unaltered by other choices of root extractions, was summarized by ABEL as

$$\sum_{k=0}^{\mu-1} p_k \omega^{uk} s^{\frac{k}{\mu}} = \sum_{k=0}^{\mu-1} \hat{p}_k \hat{\omega}^{uk} \hat{s}^{\frac{k}{\mu}} \text{ for } 0 \leq u \leq \mu - 1.$$

Through a simple interchange of the order of summation, ABEL found that the first coefficient p_0 was *unaltered* if another root extraction $\hat{s}$ of s was chosen. Turning his attention to the quantities s and $\hat{s}$, he then — by a sequence of formulae — demonstrated that there existed an integer ν such that these quantities were related by the equation

$$\hat{s} = p_\nu^\mu s^\nu. \tag{8.6}$$

In the course of his deductions, ABEL introduced the further simplification $p_1 = 1$ which earlier led him into the mistaken assumptions on the degrees and orders of the coefficients in the impossibility proof (see sections 6.3.2 and 6.9.1). In the present situation, it had no negative implications, though. With this simplification, the roots $z_0, \ldots, z_{\mu-1}$ could be expressed as

$$z_u = p_0 + \omega^u s^{\frac{1}{\mu}} + \sum_{k=2}^{\mu-1} p_k \omega^{uk} s^{\frac{k}{\mu}} \text{ for } 0 \leq u \leq \mu - 1.$$

Summing over the roots and using basic properties of primitive roots of unity, ABEL obtained

$$s^{\frac{1}{\mu}} = \frac{1}{\mu} \sum_{k=0}^{\mu-1} \omega^{-k} z_k, \text{ and}$$
$$p_u s^{\frac{u}{\mu}} = \frac{1}{\mu} \sum_{k=0}^{\mu-1} \omega^{-ku} z_k \text{ for } 2 \leq u \leq \mu - 1.$$

22 By "choosing another root extraction", I mean (in a general setup) choosing $\alpha \sqrt[n]{y}$ for $\sqrt[n]{y}$ where α is an n^{th} root of unity.

23 ABEL wrote $s', w', p'_0, \ldots, p'_{\mu-1}$ for the quantities I have denoted $\hat{s}, \hat{\omega}, \hat{p}_0, \ldots, \hat{p}_{\mu-1}$. I have altered his notation to make powers such as $\hat{s}^{\frac{k}{\mu}}$ more readable.

For any $u > 1$, ABEL had, therefore, explicitly demonstrated that $p_u s$ was a rational function of the roots $z_0, \ldots, z_{\mu-1}$.

The irreducible equation for s was *Abelian*. The ultimate result of ABEL'S studies of the solubility of equations amounted to a characterization of the *irreducible* equation $P = 0$ which the quantity s satisfied. By arguments founded in C. F. GAUSS' (1777–1855) theory of primitive roots, ABEL found that $P = 0$ had the property of having all its roots representable as the "orbit" of a rational function (see page 145) whereby the equation fell into the category studied in the *Mémoire sur une classe particulière*.[24]

Denoting the degree of the irreducible equation $P = 0$ by ν, ABEL could express its ν roots in one of the two forms

$$s \text{ or } p^{\mu}_{m_{k_1}} s^{m_{k_1}} \text{ for } 1 \leq k_1 \leq \nu - 1$$

where $m_{k_1} \in \{2, 3, \ldots, \mu - 1\}$. He deduced this from (8.6) described above, since choosing any other root extraction would give an $\hat{s}$ of the form $p^{\mu}_{\theta} s^{\theta}$. Fixing some m, a sequence could be constructed, possibly renumbering the coefficients $p_0, \ldots, p_{k_1-1}$,

$$\begin{aligned} s_1 &= p^{\mu}_0 s^m, \\ s_2 &= p^{\mu}_1 s^m_1, \\ &\vdots \\ s_{k_1} &= p^{\mu}_{k_1-1} s^m_{k_1-1}. \end{aligned}$$

At some point, the sequence would stabilize because only finitely many different roots of $P = 0$ could be listed. Assuming this to have occurred after the k_1^{th} iteration, at which point the value could be assumed to be s again, ABEL wrote

$$s = s_{k_1} = p^{\mu}_{k_1-1} s^m_{k_1-1} = s^{m^{k_1}} \prod_{u=0}^{k_1-1} p^{\mu m^u}_{k_1-(u+1)}.$$

Dividing this equation by s and extracting the μ^{th} root, he obtained the relation

$$s^{\frac{m^{k_1-1}}{\mu}} \prod_{u=0}^{k_1-1} p^{m^u}_{k_1-u-1} = 1.$$

Since the product was a rational function of s by the previous result, ABEL concluded that the exponent of s would have to be integral

$$\begin{aligned} \frac{m^{k_1} - 1}{\mu} &= \text{integer}, \\ \text{or } m^{k_1} &\equiv 1 \pmod{\mu}. \end{aligned}$$

24 (N. H. Abel, 1829c)

The central question of this part of the paper was whether $k_1 = \nu$, i.e. whether *all* the roots of $P = 0$ were found in the sequence above. ABEL answered this important question by a nice application of GAUSS' primitive roots, although his presentation in the notebook becomes increasingly obscure (see figure 8.1). Eventually, nothing but a sequence of equations can be found. However, ABEL'S intended argument can be inferred and reconstructed. In the following, I add some explanation to ABEL'S equations based on arguments by HOLMBOE and SYLOW.[25]

In order to demonstrate that $s_1^{\frac{1}{\mu}}, \ldots, s_{k_1}^{\frac{1}{\mu}}$ were rational functions of $s^{\frac{1}{\mu}}$, ABEL let m denote a primitive root of the modulus μ and recast the procedure described above as

$$\begin{aligned} s_1^{\frac{1}{\mu}} &= p_0 s^{\frac{m^\alpha}{\mu}}, \\ s_2^{\frac{1}{\mu}} &= p_1 s_1^{\frac{m^\alpha}{\mu}}, \\ &\vdots \\ s_k^{\frac{1}{\mu}} &= p_{k-1} s^{\frac{m^\alpha}{\mu}}. \end{aligned} \tag{8.7}$$

At some point, say after the k^{th} iteration, the procedure would stabilize and give

$$s^{\frac{1}{\mu}} = s^{\frac{m^{\alpha k}}{\mu}} \times \prod_{u=0}^{k-1} p_{k-u-1}^{m^{u\alpha}}.$$

By the same argument as above, ABEL could write

$$\frac{m^{\alpha k} - 1}{\mu} = \text{integer}, \tag{8.8}$$

and he concluded that k divided $\mu - 1$. This conclusion can be seen to impose a minimality condition upon k with respect to (8.8). However, in ABEL'S equations no mention of such a minimality requirement can be found. The congruence (8.8)

$$m^{\alpha k} \equiv 1 \pmod{\mu}$$

led ABEL to introduce n such that

$$\alpha k = (\mu - 1)\, n.$$

In subsequent reasoning, ABEL repeatedly used the fact that $(k, n) = 1$ without going into details. However, it is a consequence of the minimality of k mentioned above. Through a sequence of deductions based on primitive roots and congruences inspired by GAUSS, ABEL could link a number β to the sequence (8.7) such that

$$\beta k = \mu - 1.$$

25 (Holmboe in N. H. Abel, 1839, vol. 2, 288–293), (Sylow in N. H. Abel, 1881, vol. 2, 329–338), and (L. Sylow, 1902, 18–22).

If any root existed outside the sequence (8.7), a sequence could be based on this root, and a similar deduction would produce another pair of integers β', k' related by

$$\beta' k' = \mu - 1.$$

However, as ABEL demonstrated, from two such sequences a third one corresponding to $\beta'' = \gcd(\beta, \beta')$ could also be constructed with the same property

$$\beta'' k'' = \mu - 1.$$

ABEL knew that if $\beta = \beta'$, the two initial sequences were not distinct. If the two initial sequences were assumed to be maximal, a contraction was obtained, since the sequence corresponding to β'' was longer than both the initial sequences.

Thus, ABEL had demonstrated that the assumption of a root existing outside the maximal sequence (8.7) led to a contradiction, and therefore all the roots were located in a single chain. Using the same notation as in the *Mémoire sur une classe particulière*, ABEL wrote the set of roots of $P = 0$ as

$$s, \theta(s), \theta^2(s), \ldots, \theta^{\nu-1}(s), \text{ where } \theta^{\nu}(s) = s,$$

and the equation $P = 0$ was seen to be a specimen of the class of equations which have become known as *Abelian* equations (see chapter 7).

The first result of ABEL'S research had been to reduce the search for algebraic expressions satisfying an *arbitrary* equation to the search for expressions satisfying an *irreducible* one. As SYLOW remarks,[26] the present investigation had led to the further restriction to studying only the possible solutions to *irreducible Abelian* equations whose degree divided $\mu - 1$. The desired complete characterization of expressions solving irreducible *Abelian* equations was, however, not undertaken in the notebook study.

8.4 Further ideas on the theory of equations

Besides the described reduction to *Abelian* equations, the notebook manuscript and ABEL'S letters contain other interesting results one of which addressed the form of roots of solvable equations. This result can be seen as an elaboration and rigorization of one of L. EULER'S (1707–1783) claims.

The form of roots of solvable equations: rigorizing EULER. At the end of the investigation of possible solutions in the notebook, ABEL found that if an equation was solvable by radicals, its solution would be based on the relationship

$$s_k^{\frac{1}{\mu}} = A_i \prod_{u=0}^{\nu-1} a_u^{\frac{m^{ku\alpha}}{\mu}} \text{ for } 0 \leq k \leq \nu - 1,$$

26 (L. Sylow, 1902, 21).

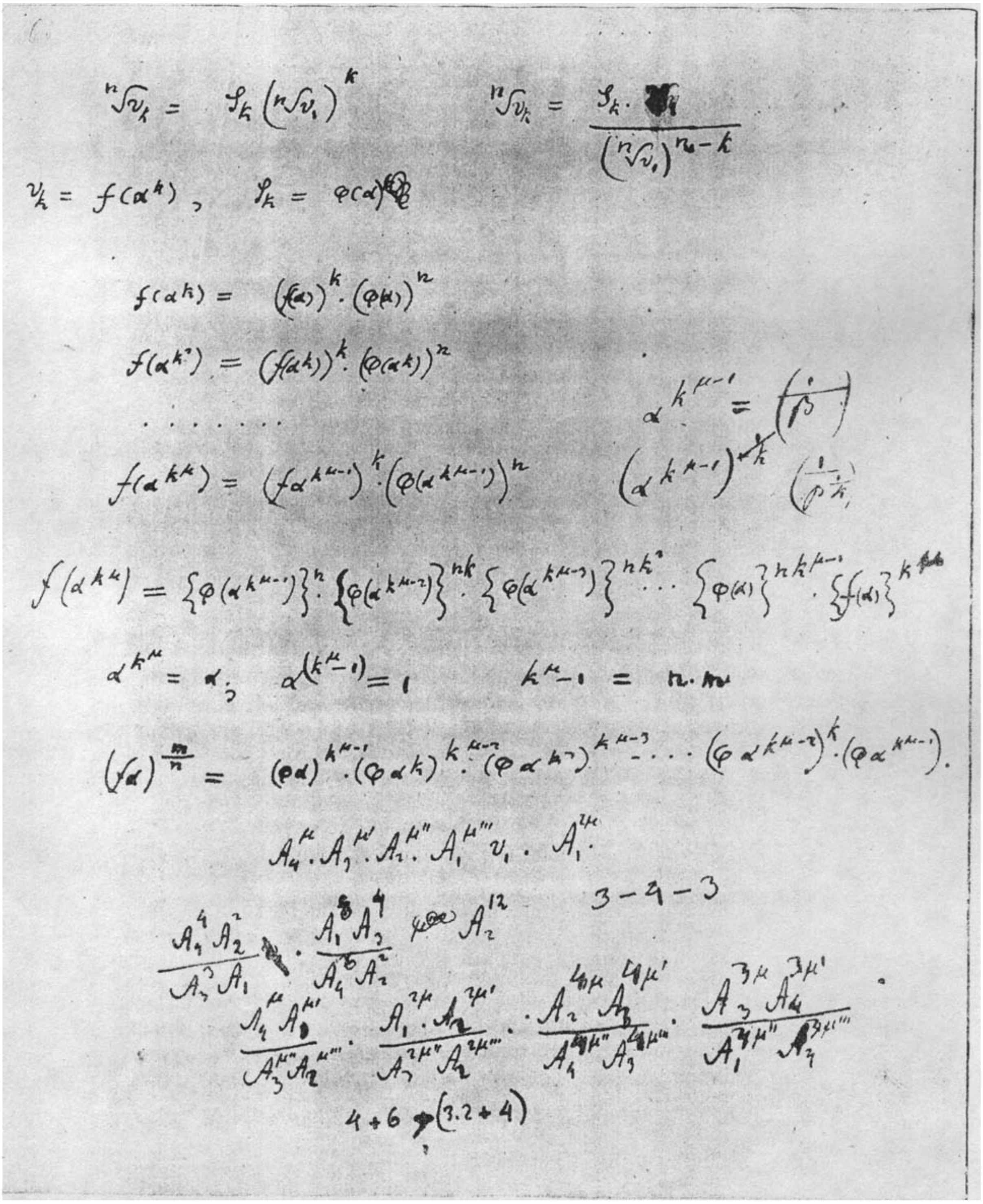

Figure 8.1: One of the last pages from ABEL'S notebook manuscript on algebraic solubility (Abel, MS:696, 66). Reproduced from (N. H. Abel, 1902e, facsimile III)

where $a_0, \ldots, a_{\nu-1}$ were roots of an irreducible *Abelian* equation of degree ν and the coefficients A_i were rational expressions in s. The root z_0 of the initial equation was in turn given from the sequence $s_0^{\frac{1}{\mu}}, \ldots, s_{\nu-1}^{\frac{1}{\mu}}$ by a relationship of the form

$$z_0 = p_0 + \sum_{u=0}^{\nu-1} \sum_{k=0}^{\nu-1} \phi_u(s_k) \cdot s_k^{\frac{m^u}{\mu}},$$

where $\phi_0, \ldots, \phi_{\nu-1}$ were rational functions. In a letter to CRELLE dated 1826, ABEL had announced a result for equations of the fifth degree which was a particular case of the above.

> "When an equation of the fifth degree, whose coefficients are *rational numbers*, is algebraically solvable, one can always give its roots the following form:
>
> $$x = c + A \cdot a^{\frac{1}{5}} \cdot a_1^{\frac{2}{5}} \cdot a_2^{\frac{4}{5}} \cdot a_3^{\frac{3}{5}} + A_1 \cdot a_1^{\frac{1}{5}} \cdot a_2^{\frac{2}{5}} \cdot a_3^{\frac{4}{5}} \cdot a^{\frac{3}{5}} + A_2 \cdot a_2^{\frac{1}{5}} \cdot a_3^{\frac{2}{5}} \cdot a^{\frac{4}{5}} \cdot a_1^{\frac{3}{5}} + A_3 \cdot a_3^{\frac{1}{5}} \cdot a^{\frac{2}{5}} \cdot a_1^{\frac{4}{5}} \cdot a_2^{\frac{3}{5}}$$
>
> where
>
> $$\begin{aligned} a &= m + n\sqrt{1+e^2} + \sqrt{h\left(1+e^2+\sqrt{1+e^2}\right)}, \\ a_1 &= m - n\sqrt{1+e^2} + \sqrt{h\left(1+e^2-\sqrt{1+e^2}\right)}, \\ a_2 &= m + n\sqrt{1+e^2} - \sqrt{h\left(1+e^2+\sqrt{1+e^2}\right)}, \\ a_3 &= m - n\sqrt{1+e^2} + \sqrt{h\left(1+e^2-\sqrt{1+e^2}\right)}, \\ A &= K + K'a + K''a_2 + K'''aa_2, \\ A_1 &= K + K'a_1 + K''a_3 + K'''a_1a_3, \\ A_2 &= K + K'a_2 + K''a + K'''aa_2, \\ A_3 &= K + K'a_3 + K''a_1 + K'''a_1a_3. \end{aligned}$$
>
> The quantities c, b [h], e, m, n, K, K', K'', K''' are all *rational* numbers.
>
> In this way, however, the equation $x^5 + ax + b = 0$ cannot be solved as long as a and b are arbitrary quantities."[27]

Probably from his realization that all quantities involved in the solution are rationals, square roots of rationals, or fifth roots of rationals, ABEL concluded that there were values of a and b for which the equation $x^5 + ax + b = 0$ could not be solvable by

27 "*Wenn eine Gleichung des fünften Grades, deren Coëfficienten* rationale Zahlen *sind, algebraisch auflösbar ist, so kann man immer den Wurzeln folgende Gestalt geben:*

$$x = c + A \cdot a^{\frac{1}{5}} \cdot a_1^{\frac{2}{5}} \cdot a_2^{\frac{4}{5}} \cdot a_3^{\frac{3}{5}} + A_1 \cdot a_1^{\frac{1}{5}} \cdot a_2^{\frac{2}{5}} \cdot a_3^{\frac{4}{5}} \cdot a^{\frac{3}{5}} + A_2 \cdot a_2^{\frac{1}{5}} \cdot a_3^{\frac{2}{5}} \cdot a^{\frac{4}{5}} \cdot a_1^{\frac{3}{5}} + A_3 \cdot a_3^{\frac{1}{5}} \cdot a^{\frac{2}{5}} \cdot a_1^{\frac{4}{5}} \cdot a_2^{\frac{3}{5}}$$

radicals. In this way, the insolubility of fifth degree equations of the standard form[28] $x^5 + ax + b = 0$ was demonstrated directly: If the equation had been solvable, ABEL possessed a solution formula, which he saw was not powerful enough to give the solution of *arbitrary* equations.

In a letter to HOLMBOE from the same year, the result on the form of roots was given another twist.

> "Concerning equations of the 5$^{\text{th}}$ degree I have found that whenever such an equation can be solved algebraically, the root must have the following form:
>
> $$x = A + \sqrt[5]{R} + \sqrt[5]{R'} + \sqrt[5]{R''} + \sqrt[5]{R'''}$$
>
> where R, R', R'', R''' are the 4 roots of an equation of the 4$^{\text{th}}$ degree and have the property that they can be expressed with help of only square roots. — It has been a difficult task for me with respect to expressions and notation."[29]

In this form, the statement is a refined version of EULER'S "conjecture" that the solution of the fifth degree equation should be of the form

$$A + \sqrt[5]{R} + \sqrt[5]{R'} + \sqrt[5]{R''} + \sqrt[5]{R'''} \tag{8.9}$$

where R, R', R'', R''' were solutions to an equation of the fourth degree (see section 5.1).

---

wo

$$\begin{aligned}
a &= m + n\sqrt{1+e^2} + \sqrt{h\left(1+e^2+\sqrt{1+e^2}\right)},\\
a_1 &= m - n\sqrt{1+e^2} + \sqrt{h\left(1+e^2-\sqrt{1+e^2}\right)},\\
a_2 &= m + n\sqrt{1+e^2} - \sqrt{h\left(1+e^2+\sqrt{1+e^2}\right)},\\
a_3 &= m - n\sqrt{1+e^2} + \sqrt{h\left(1+e^2-\sqrt{1+e^2}\right)},\\
A &= K + K'a + K''a_2 + K'''aa_2,\\
A_1 &= K + K'a_1 + K''a_3 + K'''a_1a_3,\\
A_2 &= K + K'a_2 + K''a + K'''aa_2,\\
A_3 &= K + K'a_3 + K''a_1 + K'''a_1a_3.
\end{aligned}$$

Die Grössen $c, b, e, m, n, K, K', K'', K'''$ sind alle rationale *Zahlen.*
Auf diese Weise lässt sich aber die Gleichung $x^5 + ax + b = 0$ nicht auflösen, so lange a und b beliebige Grössen sind." (Abel→Crelle, Freyberg, 1826/03/14. N. H. Abel, 1902a, 21–22).

28 If formulated in positive way, the researches of JERRARD (see section 6.9.1) demonstrated that every fifth degree equation could be transformed to this normal trinomial form. (W. R. Hamilton, 1839, 251)

29 *"Med Hensyn til Ligninger af 5th Grad har jeg faaet at naar en saadan Ligning lader sig løse algebraisk maa Roden have følgende Form:*

$$x = A + \sqrt[5]{R} + \sqrt[5]{R'} + \sqrt[5]{R''} + \sqrt[5]{R'''}$$

hvor R, R', R'', R''' ere de 4 Rødder af en Ligning af 4de Grad, og som lade sig udtrykke blot ved Hjelp af Qvadratrødder. — Det har været mig en vanskelig Opgave med Hensyn til Udtryk og Tegn." (Abel→Holmboe, Paris, 1826/10/24. N. H. Abel, 1902a, 45).

ABEL had turned the argument around and demonstrated that, although not all fifth degree equations were algebraically solvable, those which were all had solutions of the form (8.9). A particular instance of this result had already been obtained for *Abelian* equations in the *Mémoire sur une classe particulière* as recorded in (7.10). As a result of this inversion of argument, EULER'S hypothesis can be seen as a bold conjecture which ABEL later turned into a proof through a restriction on the class of objects dealt with. Where EULER had been concerned with the class of *all* fifth degree equations, ABEL restricted (barred) his results on the form of roots to only those equations which were *algebraically solvable*.

An extension of the class of *Abelian* equations. In a later letter to CRELLE, written around the same time as the notebook entry, i.e. 1828, ABEL announced further results in the theory of equations. Generalizing the assumptions on the rational correspondences between roots of an irreducible equation sufficient to guarantee solubility, ABEL had found:

> "If three roots of an irreducible equation of a certain *prime* degree have such a relation between them that one can express one of the roots *rationally* in the two others, the equation under consideration will always be solvable by *radicals*."[30]

As SYLOW has noticed, the assumption on the rational relationship among the three roots is not quite clear: The mathematical correct assumption is that *all* the roots of the equation can be expressed rationally if any two among them are considered known.[31] In the form of a corollary to his result, ABEL gave the result contained in the *Mémoire sur une classe particulière* that if two roots of an irreducible equation of prime degree were rationally related, the equation would be algebraically solvable. Although this indicates that ABEL had, at the time of writing the *Mémoire sur une classe particulière*, the result on the solubility of irreducible equations of prime degree in which any root can be written as

$$x_i = \theta_i\,(x_0, x_1)$$

at his disposal, he never made the more general result public in print.

This class of equations, which ABEL saw contained the so-called *Abelian* ones, was taken up by E. GALOIS (1811–1832) after whom they are now named. Within his theory (see chapter 8.5), GALOIS stated the theorem that it was a necessary and sufficient condition for algebraic solubility that "if some two of the roots of an irreducible equation of prime degree are considered known, the others can be expressed rationally."[32]

30 "*Si trois racines d'une équation quelconque irreductible d'un degré marqué par un nombre* premier *sont liées entre elles de la manière que l'on pourra exprimer l'une de ces racines* rationellement *en les deux autres, l'équation en question sera toujours resoluble à l'aide de* radicaux." (Abel→Crelle, Christiania, 1828/08/18. N. H. Abel, 1902a, 73).

31 (L. Sylow, 1902, 17).

32 "Théorème. *Pour qu'une équation irréductible de degré premier soit soluble par radicaux, il faut et il suffit que deux quelconques des racines étant connues, les autres s'en déduisent rationnellement.*" (Galois, 1831c, 69).

The major parts of ABEL'S research on equations which can be rendered intelligible have been presented above. Nevertheless, ABEL'S notebooks are filled with notes and scribbles for additional research which he never translated into a finished form suitable for presentation. During the few remaining years of his life, ABEL became preoccupied with other mathematical topics. Thus, we can only wonder what he might have achieved, had he returned to the theory of solubility *per se*.

8.5 General resolution of the problem by E. GALOIS

ABEL'S attempt at a general theory of the algebraic solubility of equations was not published until the first edition of the *Œuvres* 1839. Hence, it is most likely that GALOIS was unaware of ABEL'S general research when he wrote down his theory in the early 1830s. GALOIS knew the published works of LAGRANGE and A.-L. CAUCHY (1789–1857), and he had probably read ABEL'S two publications on the theory of equations — the impossibility proof of 1826 and the *Mémoire sur une classe particulière* published 1829[33] — as well as ABEL'S more widely known works on the theory of elliptic functions, the *Recherches sur les fonctions elliptiques*[34] and the *Précis d'une théorie des fonctions elliptiques*[35].[36] GALOIS "vehemently denied"[37] dependence on ABEL as can be seen from the fragmentary *Note sur Abel*,[38] but undeniably they share many of their inspirations. In section 8.5.1, I briefly describe GALOIS' unified theory before I comment upon the common inspiration and central problems shared in the works of ABEL and GALOIS (section 8.5.2).

The turbulent life of EVARISTE GALOIS as well as the interplay between his life and the fate of his mathematics have been studied intensively.[39] GALOIS' theory of algebraic solubility was not made public to the mathematical community except for a small group of members of the *Institut de France* until J. LIOUVILLE (1809–1882) published selections from GALOIS' mathematical manuscripts in the *Journal de mathématiques pures et appliquées* in 1846.[40] Subsequently, many mathematicians in the second half of the nineteenth century invested great efforts in incorporating GALOIS' at times fragmentary and non-rigorous mathematics into the new standards of clarity and rigour. The process made mathematicians like KRONECKER return to ABEL'S works and manuscripts (see section 6.9.2), but was largely an enterprise of digesting GALOIS' work. Therefore, the reception of GALOIS' theory is not the primary concern

33 (N. H. Abel, 1826a; N. H. Abel, 1829c)
34 (N. H. Abel, 1827b; N. H. Abel, 1828b)
35 (N. H. Abel, 1829d)
36 (Wussing, 1969, 75).
37 (Kiernan, 1971, 90).
38 (Galois, 1831b).
39 For instance (Wussing, 1975), (Rothman, 1982), or (Toti Rigatelli, 1996).
40 (Lützen, 1990, 559–580).

Figure 8.2: EVARISTE GALOIS (1811–1832)

in the present context,[41] which focuses on the differences and similarities between the almost concurrent works of ABEL and GALOIS.

8.5.1 The emergence of a general theory of solubility

In a sequence of manuscripts, GALOIS attacked the same two problems as ABEL had suggested in order to describe the extension of algebraic solubility (see section 8.1). ABEL had attempted to solve the first problem — that of finding all solvable equations of a given degree — in his notebook manuscript described in this chapter. ABEL'S second question concerning the determination of whether a given equation was algebraically solvable or not was the direct purpose of GALOIS' theory. GALOIS intended to give characterizations of solubility which *could*, at least in principle, be used to decide the solubility of any given equation, but the machinery needed for actually determining the solubility of given equations was of lesser interest to him.[42]

The important feature of GALOIS' theory was to associate a structure called a *group* to any given equation such that the question of solubility of equations could be translated into questions concerning these structures. Although the concept of group only saw its first *instances* and was not a developed abstract concept in the works of GA-

41 It has been dealt with extensively in the literature, for instance (J. Pierpont, 1898), (Kiernan, 1971), (Hirano, 1984), (Scholz, 1990), or (Martini, 1999).

42 (Kiernan, 1971, 83).

LOIS, he was instrumental in bringing about the structural approach to mathematics, which came to dominate much of 20th century mathematics.[43]

GALOIS' work was, as he himself somewhat laconically remarked,[44] founded in the theory of permutations most of which he had taken over from CAUCHY. GALOIS considered an equation of degree m

$$\phi(x) = 0$$

having the roots $x_1, \ldots, x_m$, and claimed that *the group of the equation* G — later called the *Galois group* — could always be found, which had the following two properties:

1. that every function of the roots $x_1, \ldots, x_m$ which was (numerically) invariant under the substitutions of G was rationally known, and conversely,

2. that every rational function of the roots $x_1, \ldots, x_m$ was invariant under the substitutions of G.

GALOIS took over the concept of *rationally known* from LAGRANGE but changed the notion of *invariant* to stress *numerical invariance* instead of LAGRANGE'S *formal invariance* in order to deal with special (i.e. non-general) equations. However, GALOIS' proof of the existence of the group of the equation suffered from the unclear character of his concept of invariance.[45]

Although the concepts of *permutation* and *substitution* underwent some uncompleted changes in GALOIS' manuscripts, he clearly perceived the multiplicative nature of substitutions — understood as transitions from one arrangement (permutation) to another — as well as the multiplicative closure of the GALOIS group.

> "It is clear in the group of permutations under consideration, the arrangement of letters is not important, but only the substitutions on the letters, by which we move from one permutation to another. Thus, if in similar group one has the substitutions S and T, one is also certain to have the substitution ST."[46]

The second component of GALOIS' theory addressed the reduction of the group of an equation by the adjunction of quantities to the set of *rationally known quantities*. By adjoining to the rationally known quantities a single root of an irreducible auxiliary equation, GALOIS could decompose the group of the equation into a number, p, of subgroups. These had the remarkable property that applying a substitution to

43 These aspects of GALOIS' work have been studied by, for instance, (Wussing, 1969) and (Kiernan, 1971).

44 (Galois, 1830, 165).

45 (Kiernan, 1971, 80–81).

46 "*Comme il s'agit toujours de questions où la disposition primitive des lettres n'influe en rien, dans les groupes que nous considérons, on devra avoir les mêmes substitutions quelle que soit la permutation d'où l'on sera parti. Donc si dans un pareil groupe on a les substitutions S et T, on est sûr d'avoir la substitution ST.*" (Galois, 1831c, 47). I have extended the translation found in (Kiernan, 1971, 80).

permutations in one of the subgroups gave the permutations of another subgroup.[47] When GALOIS adjoined the entire set of roots of the irreducible auxiliary equation, he obtained an even more remarkable result:

> "*Theorem*. If one adjoins to an equation *all* the roots of an auxiliary equation, the groups in question in theorem II [i.e. the p subgroups mentioned above] will furthermore have the property that the substitutions are the same in each group."[48]

Of this important theorem GALOIS gave no proof, but hastily remarked "the proof will be found."[49] The contents of the theorem is GALOIS' characterization of the defining property of what was be called normal subgroups, since GALOIS' statement corresponds to saying that all the conjugate classes of a subgroup U are identical.[50]

The link between properties of the decomposition into normal subgroups of the group of the equation and the algebraic solubility of the equation was provided in the far-reaching fifth problem of the manuscript. Using modern concepts and terms, it can be summarized as follows. Assuming that the equation under consideration had the group G, and that p was the smallest prime divisor of the number of permutations in G, GALOIS argued that the equation could be reduced to another equation having a smaller group G' whenever a normal subgroup N existed in G with index p. Furthermore, the link with algebraic solubility was provided when GALOIS stated that the equation would be solvable in radicals precisely when its group could be decomposed into the trivial group by iterated applications of the preceding principle.[51]

GALOIS applied the general result on algebraic solubility in two ways to obtain important characterizations of solubility of equations. First, he sought criteria for solubility of irreducible equations of prime degree and found the following:

> "Thus, for an irreducible equation of prime degree to be solvable by radicals it is *necessary* and *sufficient* that any function which is invariant under the substitutions
>
> $$x_k \quad x_{ak+b}$$
>
> [a and b are integer constants] is rationally known."[52]

---

47 (Galois, 1831c, 55).

48 "Théorème. *Si l'on adjoint à une équation* toutes *les racines d'une équation auxiliaire, les groupes dont il est question dans le théorème II jouiront de plus de cette propriété que les substitutions sont les mêmes dans chaque groupe.*" (ibid., 57).

49 "*On trouvera la démonstration.*" (ibid., 57).

50 (Scholz, 1990, 384).

51 (ibid., 384–385).

52 "*Ainsi, pour qu'une équation irréductible de degré premier soit soluble par radicaux, il* faut *et il* suffit *que toute fonction invariable par les substitutions*

$$x_k \quad x_{ak+b}$$

soit rationnellement connue." (Galois, 1831c, 69).

Thus, GALOIS had characterized solvable irreducible equations of prime degree p by the necessary and sufficient requirement that their GALOIS group contained nothing but permutations corresponding to the linear congruences[53]

$$i \to ai + b \pmod{p} \text{ where } p \nmid a. \tag{8.10}$$

From this characterization of solubility, GALOIS deduced a second one which ABEL had also hit upon (see section 8.4), when he demonstrated his eighth proposition:

> "*Theorem.* For an equation of prime degree to be solvable by radicals it is necessary and sufficient that any two of its roots being known, the others can be deduced rationally from them."[54]

The character of GALOIS' reasoning often left quite a lot to be desired. When LIOUVILLE eventually published GALOIS' manuscripts, he accompanied them with an evaluation of GALOIS' clarity and rigour:

> "Clarity is indeed an absolute necessity. [...] Galois too often neglected this precept."[55]

In making GALOIS' new ideas available to the mathematical community and in providing proofs and elaborations of obscure points, mathematicians of the second half of the nineteenth century invested much effort in the theory of equations, permutations, and groups. Although GALOIS had found out how the solubility of a given equation could be determined by inspecting the decomposability of its associated group into a tower of normal subgroups, a number of points were left open for further research. To mathematicians around 1850, three problems were of primary concern: GALOIS' construction of the group of an equation was considered to be unrigorous, no characterization of the important *solvable groups* had been carried out, and a certain arbitrariness of the order of decomposition also remained. These matters were cleared, one by one, until the theory ultimately found its mature form in the abstract field theoretic formulation of H. WEBER (1842–1913) and E. ARTIN (1898–1962).[56]

8.5.2 Common inspiration and common problems

As mentioned earlier (p. 181), GALOIS and ABEL drew extensively on common sources. The ideas of invariance under permutations of the roots, founded in LAGRANGE'S work,[57] were important to both of them; and they both relied on the general theory

53 (Scholz, 1990, 385).

54 "Théorème. *Pour qu'une équation de degré premier soit soluble par radicaux, il faut et il suffit que deux quelconques des racines étant connues, les autres s'en déduisent rationnellement.*" (Galois, 1831c, 69).

55 (Liouville quoted from Kiernan, 1971, 77).

56 (ibid.) and (Scholz, 1990, 392–398).

57 (Lagrange, 1770–1771)

of permutations and notations which CAUCHY had developed.[58] GALOIS' investigations, however, took a different approach from the one ABEL had employed even in his attempt at a general theory of solubility. GALOIS' decisive step of relating a certain group to an equation and transforming the investigation of solubility of the equation into investigating properties of the group was as distant from ABEL as it was from LAGRANGE. But although ABEL'S research on algebraic solubility did not appear until 1839, it might have helped the mathematical community understand the purposes and intentions of GALOIS' difficult manuscripts. As H. WUSSING has put it, ABEL would probably have been the only person with the capacity to immediately understand GALOIS' works, but unfortunately ABEL had died before GALOIS ever wrote down his manuscripts.[59]

A common component of central importance to both ABEL and GALOIS was the concept of *irreducibility* (see section 7.3). In one of his manuscripts, GALOIS defined an equation to be *reducible* whenever it had rational divisors, and *irreducible* otherwise.[60] This definition closely resembles the one given by ABEL, who had been more explicit about the rationality of the divisor, though. The first theorem on irreducible equations, which ABEL proved, can also be found in GALOIS' manuscripts:

> "Lemma I. An irreducible equation cannot have any root in common with another rational equation without dividing it."[61]

Of this lemma GALOIS gave no proof, but he used the concept and lemma extensively. Through GALOIS the concept of irreducibility in its present sense finally entered algebra as a central concept upon which deductions could be built.

ABEL'S investigations had led to abstract and not easily applicable results concerning solubility. The same is true for GALOIS' approach — even to a larger extent. ABEL'S positive criteria of solubility of, for instance, *Abelian* equations concerned certain relationships existing among the unknown roots of an equation. In case nothing but the coefficients of the equation was known, this approach had no chance of producing an answer to the question of the solubility of the equation. In GALOIS' concept of the group of an equation, this non-constructive approach is carried to an extreme. GALOIS had tried to prove that such a group always existed, but did not address the question of how to construct it. He had presented his thoughts in a sequence of memoirs, one of which he had handed in to the *Institut de France* in January 1831. The reviewers, S. F. LACROIX (1765–1843) and S.-D. POISSON (1781–1840), immediately noticed this "deficiency" and allowed it to play a role in their refusal:

> "[...] it should be noted that [the theorem] does not contain, as the title would have the reader believe, the condition of solubility of equations by radicals. [...]

58 (A.-L. Cauchy, 1815a; A.-L. Cauchy, 1815b)
59 (Wussing, 1975, 397).
60 (Galois, 1831c, 45).
61 "*Lemme I. Une équation irréductible ne peut avoir aucune racine commune avec une équation rationnelle sans la diviser.*" (ibid., 47).

> This condition, if it exists, should have an external character, that can be tested by examining the coefficients of a given equation, or, at most, by solving other equations of lesser degree than that proposed. We made all possible efforts to understand M. Galois' evidence. His thesis is neither clear enough, nor sufficiently developed to enable us to judge its rigour."[62]

The interplay between the theory of equations and the flourishing theory of elliptic functions had been essential in ABEL'S approach (see section 7.2). The division of elliptic functions had given rise to certain classes of equations described by relations among the roots, and ABEL had pursued his favorite subject, the theory of equations (see the quotation on page 160), in investigating the question of algebraic solubility of these equations. Although not to the same extent engaged in research on elliptic functions, GALOIS also saw the modular equations of elliptic functions as an important application of and inspiration for his theory of solubility. After GALOIS was expelled from the *École Normale* in 1831, he offered classes on, among other subjects of algebra, "elliptic functions treated as pure algebra",[63] presumably dealing with the subject in a way similar to ABEL'S approach. In the 1831-manuscript,[64] GALOIS gave a general solution to the division problem concerning the division of an elliptic function of the first kind into p^n equal parts, where p was a prime. The central step of the proof was given by his result that any rational function which is unaltered by *linear congruence* substitutions of the form (8.10) is known. Just as ABEL had generalized his interest in elliptic functions into the integration theory of algebraic functions, GALOIS' investigations took a similar turn, and a large part of his manuscripts concerned this theory.

The creation of *Galois Theory* in many ways marked the transition into modern mathematics. The concept of group was implicitly introduced by GALOIS, and he explicitly gave it its name; but more importantly, GALOIS' revolutionary attitude toward explicit arguments in mathematics marked a transition from arguments based on manipulations of formal expressions to more *concept* based deductions. To many nineteenth century mathematicians this transition — together with the fragmentary and hasty character of GALOIS' arguments — rendered the new results "vague", faulty, or at least in need of elaboration and proof.[65] The transition proved to be irreversible, though, and concept based mathematics was the mathematics of the future.

62 (Toti Rigatelli, 1996, 90).
63 (ibid., 79–80).
64 (Galois, 1831a)
65 (Kiernan, 1971, 59).

Part III

Interlude: ABEL and the 'new rigor'

Chapter 9

The nineteenth-century change in epistemic techniques

Of the numerous transitions in epistemic techniques — changes in *how* mathematics was conducted — which took place in the 1820s, few were as far reaching as the initiation of the movement aiming at rigorizing analysis through arithmetization. The rigorization of analysis involved fundamental changes in the basic concepts of the discipline and also manifested itself on the technical level. The causal events leading to the rigorization are varied and span both external and internal factors. However, it is no coincidence that the rigorization was originally promoted in textbooks which were needed for the large-scale instruction in mathematics brought about by external events.

Critical revision: A change in epistemic techniques. Central to the replacement of existing practice was the prominence given by leading research mathematicians to the rigorization program and the critical revision. J. L. LAGRANGE'S (1736–1813) textbooks marked a new awareness concerning the foundations of the calculus — later rigorization built upon the Lagrangian program.

In the 1820s, A.-L. CAUCHY (1789–1857) presented his revision of the foundations of analysis which meant ingeniously revising the basic notions of the discipline. At the core of the change, CAUCHY discarded the eighteenth century conception of formal equality between expressions in favor of a new concept of arithmetical equality between functions. This change had implications for most of the other basic notions: limits, convergence, continuity, and differentiability to name but a few. For instance, CAUCHY was led by his new rigor to abandon attributing meaning to sums of divergent series and to promote tests of convergence into central positions within his theoretical framework.

By the mid-1820s, N. H. ABEL (1802–1829) expressed severe concerns for the contemporary state of the calculus: he felt that it lacked system and rigor. Simultaneously, ABEL revealed his interest in finding out how the previous generations could have obtained correct results from their "unrigorous" foundations. This question represents another aspect of the critical revision; an aspect which is intimately tied to the perception of cumulativity in mathematics.

Toward a concept based version of analysis. One of the main achievements of CAUCHY'S new rigor was the new internal relationship between definitions, theorems and proofs. Not only did CAUCHY promote notions such as *limits* into the core concepts of analysis he also put his definitions into direct use in forming new concepts, e.g. convergence and continuity, and in proving theorems about these concepts. Thus, CAUCHY'S definitions in certain senses continued existing trends but were more concrete and applicable in stating and demonstrating theorems. Furthermore, CAUCHY'S critical revision forced him to restructure the network of definitions and theorems changing the internal fabric of the theory.

Interpreted in the framework of the nineteenth century transition toward concept based mathematics, the rigorization of analysis thus provides an important example in which both the structure and the techniques of a discipline underwent deep changes.

In the following chapters, the general transition is described and analyzed from the perspective of ABEL. ABEL'S impact on the rigorization program mainly consists of three themes:

1. A very critical attitude which was mainly expressed in letters.

2. A proof of the binomial theorem which surpassed its predecessors in generality and rigor.

3. A discussion on the existence of general criteria of convergence.

All three themes fall in the changing standard of analysis which was brought about by CAUCHY'S revision of the discipline. Therefore, important aspects of this context must first be described.

Chapter 10

Toward rigorization of analysis

From the time the calculus emerged in the 17th century until the end of the 18th century, mathematicians and philosophers were wary when confronted with questions concerning its foundations. To some extent ignoring foundational questions, mathematicians focused on creating new results which could be useful in answering interesting questions, for instance in the field of mathematical physics. To some mathematicians working toward the end of the 18th century, rigorously founding the calculus remained one of the few open problems; but one of relatively lesser importance than the development of new analytical results.[1] To others, primarily J. L. LAGRANGE (1736–1813), the foundations of the calculus became a prestigious mathematical research problem.[2]

The transformation of concepts, theorems, and proofs in the process of rigorization in analysis have been subject to a variety of historical enquiries; in the following, emphasis is given to establishing and illustrating certain ideas and developments which are of importance in subsequent chapters.[3]

10.1 EULER's vision of analysis

To understand the revision and the contents of the refocus on rigor, some aspects of eighteenth century analysis are of key importance. In particular, the results and techniques of L. EULER (1707–1783) dominated the way mathematicians worked in the field for half a century.

Focus on functions and formal equality. Beginning with his influential monograph *Introductio in analysin infinitorum*,[4] EULER promoted *functions* to become the basic ob-

1 See for instance the quotations in section 3.3 frequently invoked to document a belief in the stagnation of the mathematical sciences.

2 For LAGRANGE'S algebraic approach to the calculus, see e.g. (Grabiner, 1990); for its influence on CAUCHY, see (Grabiner, 1981b). The best general presentation of the development of analysis in the nineteenth century is, I think, (Bottazzini, 1986).

3 For the evolution of rigorization in analysis, see e.g. (Bottazzini, 1986; Jahnke, 1999; Lützen, 1999).

4 (L. Euler, 1748).

jects of analysis. EULER'S definitions and use of functions have attracted the interest of historians of mathematics.[5] In the present context, the two most important aspects of EULER'S approach are:

1. EULER'S variable quantities were universal in the sense that they would "comprise all determinate values" including positive and negative, rational and irrational, and real and imaginary values.

2. EULER defined a function of a variable quantity to be an "analytic expression composed in any way from the variable quantity and numbers or constant quantities". The operations allowed to form *analytic expressions* were algebraic operations, both finite and infinite.

Together, these two aspects entail an important interpretation of the concept of equality between functions. To EULER, two analytic expressions were considered equal if one could be transformed into the other by a sequence of (formal) manipulations. For instance, in developing methods for expanding rational functions into power series, EULER described — in the *Introductio* — a method by which the two expressions

$$\frac{1}{1-x} \text{ and } \sum_{n=0}^{\infty} x^n$$

should be considered equal because the latter could be obtained by (formally) carrying out the division.[6] Of course, EULER was aware that peculiar results would emerge if certain numerical values were inserted for x and the equality was believed to apply to this numerical case as well. The proper interpretation of the sum

$$1 - 1 + 1 - 1 + \dots$$

had been a controversial subject throughout the first half of the eighteenth century. To EULER, its sum would be $\frac{1}{2}$ by the formal equality above. Generally, EULER chose to focus on the formal aspect of functional equalities ignoring the "paradoxes" which might occur if numerical values were inserted.

To a modern reader, EULER'S disregard for numerical convergence may seem odd. However, it corresponds to a paradigm in analysis — the Euclidean paradigm — which focused on the fruitful manipulations of finite or infinite expressions; the un-problematic transition from one such representation to another constituted a cornerstone of EULER'S skillful investigations in analysis.

5 See e.g. (Jahnke, 1999; Lützen, 1978; Youschkevitch, 1976).

6 (L. Euler, 1748, §60–61).

10.1.1 The binomial theorem

In its various forms and various degrees of specialization, i.e. various restrictions on m and x, the binomial theorem asserts the equality

$$(1+x)^m = 1 + \frac{m}{1}x + \frac{m(m-1)}{1\cdot 2}x^2 + \frac{m(m-1)(m-2)}{1\cdot 2\cdot 3}x^3 + \dots.$$

The theorem became one of the pivotal points of analysis since it was first employed as a heuristic tool by I. NEWTON (1642–1727) to obtain series expansions for expressions such as $(1+x)^{\frac{1}{2}}$. For integral exponents ($m \in \mathbb{N}$), the binomial theorem reduced to the well known — and firmly established — binomial *formula*

$$(1+x)^m = \sum_{n=0}^{m} \binom{m}{n} x^n \text{ for } m \in \mathbb{N}.$$

NEWTON used extrapolation from the cases of integral exponents to obtain the equality of the finite and infinite expressions in situations corresponding to fractional exponents (e.g. $m = \frac{1}{2}$, above). In the eighteenth century, the binomial theorem was provided with various proofs.[7]

To further illustrate the Eulerian paradigm in analysis, the role played by the binomial theorem within EULER'S structuring of analysis provides many interesting hints; furthermore, that theorem is of direct importance in understanding the way analysis was reorganized in the early nineteenth century.

EULER'S first proof of the binomial theorem: the link with Taylor series. In the *Introductio*, EULER gave no general proof of the binomial theorem but repeatedly used a particular version in which he let $n \to \infty$ in the binomial *formula*. Later, he presented two different proofs of this highly important tool. The first proof, published in his sequel textbook *Institutiones calculi integralis*,[8] highlighted the intimate connection between the binomial theorem and the Taylor expansion theorem which in modern notation stated that any function f (later with certain restrictions) could be expanded as

$$f(x+a) = f(x) + \frac{f'(x)}{1}a + \frac{f''(x)}{1\cdot 2}a^2 + \dots.$$

For EULER, the binomial theorem was a rather easy consequence of the Taylor expansion provided the relation

$$\frac{d}{dx}x^\mu = \mu x^{\mu-1}$$

had previously been established for exponents μ. However, as EULER later realized, the binomial theorem was central to the differentiation of such monomials if μ was not an integer. Therefore, proving the binomial theorem from the Taylor series expansion had created a vicious circle in the argument. Nevertheless, proofs of the binomial

7 The history of the binomial theorem has attracted the interest of many scholars, see e.g. (Dhombres and Pensivy, 1988; Pensivy, 1994).

8 (L. Euler, 1755, 276–279).

theorem from Taylor theorem recurred throughout the century and even into the nineteenth century.[9]

EULER'S second proof of the binomial theorem based on functional equations. In his second proof of the binomial theorem, published in 1775,[10] EULER devised his proof following an outline which would recur in most subsequent "rigorous" proofs. EULER introduced the notation

$$[m] = 1 + \frac{m}{1}x + \frac{m\,(m-1)}{1\cdot 2}x^2 + \ldots$$

to denote the binomial series associated with the exponent m. Thus, proving the binomial theorem thus amounted to proving the equality $[m] = (1+x)^m$. The central step in the proof was the realization that the brackets satisfied a functional equation[11]

$$[m+n] = [m]\cdot[n]\,.$$

EULER'S proof of the functional equation was based on formally multiplying the corresponding infinite series. Once EULER had obtained the above functional equation and the binomial formula secured the equality $[m] = (1+x)^m$ for integral m, he extended the domain for m by the computation

$$[1] = \left[m\cdot\frac{1}{m}\right] = \left[\frac{1}{m}\right]^m \underset{m,n\in\mathbb{N}}{\Rightarrow} \left[\frac{n}{m}\right] = [n]^{\frac{1}{m}} = (1+x)^{\frac{n}{m}}\,.$$

Thus, EULER proved the binomial theorem for all fractional exponents and claimed — without giving any proof — that it extended to all real exponents by way of continuity (see below). In summary, the central steps of EULER'S second proof of the binomial theorem are:

1. The binomial formula, $[m] = (1+x)^m$ for $m \in \mathbb{N}$.
2. The functional equation $[m+n] = [m]\cdot[n]$ proved by manipulating the associated power series.
3. An extension to rational exponents.
4. A further extension to real exponents by continuity arguments.

In complete correspondence with his views on formal equality, EULER did not venture into considerations of the convergence of the infinite expression contained in the binomial theorem. To him, the theorem simply stated a formal equivalence of two different representations of the same function (expression).

9 On the proof by WALLACE, see (Craik, 1999, 252–253).
10 (L. Euler, 1775).
11 For the history of functional equations, mainly with CAUCHY, see (J. Dhombres, 1992).

1797	Théorie des fonctions analytiques
1806	Leçons sur le calcul des fonctions
1813	Théorie des fonctions analytiques, nouvelle édition

Table 10.1: LAGRANGE's monographs on his algebraic analysis

10.2 LAGRANGE's new focus on rigor

The Eulerian approach to analysis based on functions and series representations proved highly productive for mathematicians with right kinds of intuitions and understanding. Toward the end of the century, a number of events — in particular the external influence of mass instruction in mathematics and the change of generations — introduced a different view on the status of analysis. Its fruitfulness was admired but its lack of strict logical order was realized by some of its most distinguished practitioners. More so than anybody else, JOSEPH LOUIS LAGRANGE was instrumental in fertilizing the ground for a fundamental revision of the Eulerian paradigm.

LAGRANGE presented his new algebraic theory of functions in three important monographs (see table 10.1). In what follows, references are made to the second edition of the *Théorie des fonctions analytiques* which was the latest of the three and was included in LAGRANGE'S collected works.[12]

Importantly, LAGRANGE believed he could prove that any function could be expanded "by the theory of series"[13] into a series of the form

$$f(x+i) = f(x) + ip(x) + i^2 q(x) + i^3 r(x) + \dots.$$

The functions $p, q, r, \dots$ were called the 'derived' functions of f, and it was the crux of the theory to show that they corresponded to the ordinary differentials obtained in the usual — less rigorous — way.

Thus, at the very center of the Lagrangian system laid the expansion of a function into a power series. As J. V. GRABINER has convincingly described in her thesis, the expansion into power series was not an *assumption* in the Lagrangian system but was provided with an *algebraic* proof using one of EULER'S ideas.[14] As a consequence, the general expansion of any function into power series was made into a general principle replacing the important *tool* for obtaining such expansions which EULER had used to such a high effect, the binomial theorem.

LAGRANGE'S contribution to the rigorization of the calculus was at least twofold:

1. The mere fact that LAGRANGE — "a most illustrious mathematician" — devoted

12 (Lagrange, 1813).
13 (ibid., 7–8).
14 (Grabiner, 1990, 93ff).

so much attention to the foundational questions raised the prestige of such questions; rigorization became a legitimate mathematical research topic.

2. Just as importantly, LAGRANGE'S work on rigorization provided a revolutionary new synthesis of the formal interpretation of series at the heart of the calculus. Furthermore, the binomial theorem and the expansion into power series (Taylor series) changed their internal relationship and dependency (see below).

10.3 Early rigorization of theory of series

In the first decades of the 19th century, a number of mathematicians responded to the call for rigorization in the theory of infinite series. Two of the most interesting reactions to the state of rigor in the theory of infinite series were made by C. F. GAUSS (1777–1855) and B. BOLZANO (1781–1848).

To GAUSS, BOLZANO, and their contemporaries, analysis was a conglomerate of various methods, key results, and foundations. An interesting illustration of the concurrently existing approaches to the discipline can be found in the textbooks written by S. F. LACROIX (1765–1843) just before the turn of the century.[15] In three volumes, LACROIX presented much of the key material of analysis adapting various approaches and foundations to suit his needs.

Both GAUSS and BOLZANO reacted inspired partly by philosophical arguments; in the following, some of the relevant aspects of their contributions are outlined.

10.3.1 GAUSS' hypergeometric series

GAUSS' main contribution to the rigorization of the theory of series consisted of a paper concerning the so-called *hypergeometric series* .[16] The paper was presented in 1812 and published the following year.[17] To GAUSS, the hypergeometric series

$$F(\alpha, \beta, \gamma, x) = 1 + \sum_{n=1}^{\infty} \frac{x^n}{n!} \prod_{m=0}^{n} \frac{(\alpha + m)(\beta + m)}{(\gamma + m)}$$

constituted a preferred representation of a vast range of functions including logarithmic, elliptic, and other transcendental functions. By studying this series in its generality, GAUSS obtained knowledge of the functions which it could represent. GAUSS never conducted a full investigation of *which* functions it could represent but the study of the series remained an interesting topic, in particular for Göttingen mathematicians. GAUSS' research represents an intermediate between the old direct and more specialized representations and the modern concept based approach to analysis.[18] More im-

15 (Lacroix, 1797; Lacroix, 1798; Lacroix, 1800).
16 The name *hypergeometric series* is a later invention, see (Wussing, 1982, 299).
17 (C. F. Gauss, 1813).
18 See also chapter 21.

portantly, GAUSS' investigations of the hypergeometric series also contained a number of very interesting results. In particular, GAUSS' attitude toward convergence of series and criteria for deciding the convergence is relevant to the present analyses.

Concepts of convergence and a criterion of convergence. Before he could advance to deeper questions, GAUSS emphasized that the convergence or divergence of the hypergeometric series had to be investigated. In his paper on the hypergeometric series, GAUSS gave no explicit definition of convergence. However, by the following argument, GAUSS claimed that the series converged for $|x| < 1$ and diverged for $|x| > 1$. As was customary, these requirements were stated verbally without the notation of numerical values (see, e.g., quotation below).

GAUSS compared the coefficients of two sequential powers of x, say the coefficients of x^m and x^{m+1}, and found that their ratio

$$\frac{1 + \frac{\gamma+1}{m} + \frac{\gamma}{m^2}}{1 + \frac{\alpha+\beta}{m} + \frac{\alpha\beta}{m^2}}$$

approached the value 1 when m was taken to be increasingly large. GAUSS then concluded that for any complex value of x with $|x| < 1$, the series would be convergent "at least from some point onward" and lead to a determinate finite sum. In case $|x| > 1$, the series would necessarily diverge and it could not have a sum.[19] GAUSS summarized his position:

> "Since our function is defined as the sum of a series, it is obvious, that our investigations are naturally confined to the cases in which the series actually converges and that it is absurd to ask for the value of the series whenever x has a value greater than unity."[20]

Of the cases with $|x| = 1$, GAUSS only investigated $x = 1$ and found that under the condition $\alpha + \beta - \gamma < 0$, the series would have a finite sum.[21]

In the above context, GAUSS appears to have employed a concept of series convergence which corresponded to the partial sums approaching a finite limit. We are easily led to believe that GAUSS' familiar looking notions such as *convergent* and *sum* meant the same to him as they do to us. However, another concept of convergence was also in use at GAUSS' time and even appeared later in his manuscripts (see below). Therefore, it is worth re-examining the evidence to see if it appears different with this added information.

Originating with J. LE R. D'ALEMBERT (1717–1783) in the mid-eighteenth century, the term *convergent* was used by mathematicians within the formal paradigm to denote

19 (ibid., 126).

20 "*Patet itaque, quatenus functio nostra tamquam summa seriei definita sit, disquisitionem natura sua restrictam esse ad casus eos, ubi series revera convergat, adeoque quaestionem ineptam esse, quinam sit valor seriei pro valore ipsius x unitate maiori.*" (ibid., 126). For a German translation, see (C. F. Gauss, 1888, 10).

21 (C. F. Gauss, 1813, 139, 142–143).

series in which the numerical value of the general term vanished monotonically, i.e. series $\sum a_n$ for which the sequence $|a_n|$ was monotonically decreasing and approached zero.[22] The vanishing of terms, clearly contrasted to the convergence of the partial sums, can be found in an unpublished manuscript written by GAUSS probably after 1831.[23]

> "By *convergence* of an infinite series, I will simply understand nothing but the infinite approaching of its terms toward 0 when the series is infinitely continued. The convergence of a series in itself is thus to be distinguished from the convergence of its summation toward a finite limit; however, the latter implies the former but not the other way around."[24]

Exactly which concept of convergence, GAUSS had in mind in his research on the hypergeometric series can seem unclear. From a modern perspective, we are tempted to assume that GAUSS interpreted *convergence* as convergence of the partial sums and interpret GAUSS' comparison of subsequent terms as an implicit quotient criterion. However, GAUSS' reasoning can equally well be interpreted within the older concept of D'ALEMBERT-convergence.[25]

In terms of the development described in the next chapter, GAUSS' investigation on the hypergeometric series is important in three respects:

1. GAUSS' investigation was confined to a particular series, albeit one with three parameters which enabled GAUSS to model a number of transcendental functions using it.

2. GAUSS insisted on establishing the convergence of the series before speaking of its sum. He used an implicit theorem — apparently equivalent to the *ratio test*[26] (see subsequent chapters) — to determine restrictions on the variable x.

3. Despite aiming at "the rigorous methods of the ancient geometers"[27], GAUSS' theory of infinite series as expressed in the paper on the hypergeometric series was rudimentary and not spelled out in much detail. For instance, it is not completely clear precisely what his basic notions meant.

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22 (Grabiner, 1981b, 60).

23 (Schneider, 1981, 55–56).

24 "*Ich werde unter* Convergenz*, einer unendlichen Reihe schlechthin beigelegt, nichts anders verstehen als die beim unendlichen Fortschreiten der Reihe eintretende unendliche Annäherung ihrer Glieder an die* 0*. Die Convergenz einer Reihe an sich ist also wohl zu unterscheiden von der Convergenz ihrer Summirung zu einem endlichen Grenzwerthe; letztere schliesst zwar die erstere ein, aber nicht umgekehrt.*" (C. F. Gauss, Fa, Kapsel 46a, A1–A13, 400).

25 In most of the (earlier) secondary literature, e.g. (Pringsheim, 1898–1904, 79), GAUSS' emphasis on establishing the convergence of the hypergeometric series and his use of the quotient comparison have been taken as precursors of the rigorization program (see next chapter). SCHNEIDER has aptly interpreted GAUSS' concept of convergence in terms of the *sequence* of terms (Schneider, 1981, 56).

26 The *ratio test* is also sometimes called the *quotient test* but I will use the term *ratio test*, throughout.

27 "*Ostendemus autem, et quidem, in gratiam eorum, qui methodis rigorosis antiquorum geometrarum favent, omni rigore.*" (C. F. Gauss, 1813, 139).

To summarize the debate, we have to emphasize three dates. In 1812, GAUSS' presented his research on hypergeometric series in which his concept of convergence remains undefined; in 1821, A.-L. CAUCHY (1789–1857) promoted the convergence of the partial sums into the only acceptable definition of convergence; but as late as 1831, GAUSS employed a D'ALEMBERT-like concept of convergence which entailed the vanishing of the terms and did not provide convergence of the series. To believe that GAUSS had anticipated CAUCHY'S notion of convergence and the ratio test in 1812 thus seems to be the least efficient interpretation. GAUSS may very well have held the same conceptions about convergence in 1812 as he evidently did in 1831. Instead, it seems that until CAUCHY'S work, different notions of convergence were co-existing and the position of definitions and tests of convergence within the structure of the theory of series floated.

10.3.2 BOLZANO's rigorization of the binomial theorem

Contrary to GAUSS, the Czech priest and mathematician BOLZANO did not have the ear of the international mathematical community although his ideas and visions for the foundation of the calculus reached even further than GAUSS'. To promote interest in his work, BOLZANO published critical investigations and new proofs of key theorems of analysis. He hoped that mathematicians would pay more attention to a broader philosophical program which he was developing.

In 1816 and in Prague, BOLZANO published a book entitled *Der binomische Lehrsatz* which is of particular relevance to the current purpose.[28] In that book, BOLZANO scrutinized existing derivations of the binomial theorem before going on to present his own proof. As noted, N. H. ABEL (1802–1829) once praised BOLZANO'S cleverness (see p. 42); important aspects of ABEL'S criticism may well have their origins with BOLZANO.

BOLZANO'S critical attitude. In the introduction of his book, BOLZANO reviewed the structures of previous proofs of the binomial theorem. In the process, BOLZANO developed a penetrating criticism of the accepted methods of reasoning with infinite series. Soon, others would repeat BOLZANO'S criticism — at least, ABEL'S judgement of eighteenth century epistemic techniques in analysis resembled some of BOLZANO'S points.

A number of interesting themes were raised in BOLZANO'S introduction. BOLZANO observed that the foundation of the entire "higher analysis" (calculus) rested on *Taylor's Theorem* and that this theorem in turn relied on the binomial theorem. Consequently, the obscure status of the proof of the latter theorem had severe implications for the entire discipline.

28 (Bolzano, 1816). BOLZANO'S titles are often very precise and very long; here the abridged version is used throughout.

Figure 10.1: BERNARD BOLZANO (1781–1848)

Indeed, from B. TAYLOR'S (1685–1731) days, proofs of *Taylor's Theorem* had relied on an analogy between repeated differences and the binomial formula.[29] However, the relevant step in the proof of *Taylor's Theorem* seems to have been a limit process based on the binomial *formula* in which the exponent n increased to infinity and thus did not rely on the full binomial theorem. This distinction between the binomial theorem and the indicated limit process does not seem to have been undertaken by eighteenth and nineteenth century mathematicians, though.

Next, BOLZANO criticized previous proofs for operating with (completed) infinite series, i.e. working with series as if they were polynomials. Instead, he proposed a concept of numerical limit processes based on (variable) quantities (*Größen*) ω which could be assumed positive but less than any given value. He also described these quantities as "quantities which can be made as small as one desires."[30] Importantly, BOLZANO'S ω was not a completed infinitesimal but a variable quantity which depended on a limit process.

In continuation of the previous point, BOLZANO insisted that restrictions be imposed on the binomial such that the series was (arithmetically) convergent. He claimed that previous proofs had "proved to much" by not taking such restrictions on x into account and forbade application of the theorem outside the domains of convergence of the series. In the argument, BOLZANO employed a counter example which based

29 See e.g. (Jahnke, 1999, 139–142).

30 "*Größen, welche so klein werden können, als man nur immer will.*" (Bolzano, 1816, v).

on uncritical use of the binomial theorem

$$\sqrt{-1} = (1-2)^{\frac{1}{2}} = 1 - \frac{1}{2}\cdot 2 - \frac{1}{8}\cdot 4 - \frac{1}{16}\cdot 8 - \dots \tag{10.1}$$

exhibited the imaginary unit as an infinite sum of real numbers.[31]

Following his program, BOLZANO rejected all previous proofs: NEWTON'S proof because it had been based on extrapolation and not "everything which corresponds to known truths is necessarily true";[32] the proofs based on the expansion into *Taylor series* because they introduced a vicious circle and the binomial theorem should be the more basic of the two theorems; and even EULER'S second proof because it operated with completed infinite series and did not consider the convergence of the series.

Revision of EULER'S proof. Subsequent to his critical remarks, BOLZANO presented his own new rigorized proof of the binomial theorem. He based it on the outline of EULER'S second proof but replaced the way in which EULER handed infinite series with his new concept of numerical equality and limits. Overturning EULER'S manipulations of completed infinite series, BOLZANO worked with the partial sums and limit arguments. Expressed in EULER'S notation, BOLZANO proved by multiplying the first s terms of $[m]$ with the first t terms of $[n]$, that the first $\min(s,t)$ terms of the product corresponded to the first $\min(s,t)$ terms of $[m+n]$.[33] If we introduce the notation $[m]_s^t$ to denote the sum of the terms ranging from s to t in the series $[m]$,[34] the result can be expressed as

$$\left([m]_1^s\,[n]_1^t\right)_1^{\min(s,t)} = [m+n]_1^{\min(s,t)}\,.$$

However, the terms after $\min(s,t)$ would not always be equal in the two expressions but BOLZANO found that the difference

$$\left([m]_1^s\,[n]_1^t\right)_{\min(s,t)}^{r+s} - [m+n]_{\min(s,t)}^{s+t}$$

could be made smaller than any given positive value by taking $\min(s,t)$ sufficiently large provided that $|x| < 1$.[35] Thus, BOLZANO obtained his proof of the functional equation

$$[m]\cdot[n] = [m+n]$$

under the important assumption $|x| < 1$.

Extension to real exponents. EULER'S (second) proof of the binomial theorem had focused on rational exponents. At the end of the argument, he suggested that other (positive) exponents could also be considered:

31 (ibid., vi).
32 (ibid., xi).
33 (ibid., §38).
34 This notation has been adapted from (Hauch, 1997).
35 (Bolzano, 1816, §40).

"[...] and thus it shows that

$$\left[\frac{i}{a}\right] = (1+x)^{\frac{i}{a}},$$

which demonstrates that our theorem is true when for the exponent n any fraction $\frac{i}{a}$ is taken, from this its truth is evident for all positive numbers taken in place of the exponent n."[36]

BOLZANO was slightly more specific on binomial expansion for irrational exponents. As a consequence of the meaning of $(1+x)^i$ for i an irrational number, BOLZANO claimed, $(1+x)^i$ could be approached as closely as desired by $(1+x)^{\frac{m}{n}}$ for m, n integers. Inserting $\frac{m}{n}$ for i everywhere in the series and letting $\frac{m}{n}$ approach i, the sum would approach $(1+x)^i$ as closely as desired. Thus, BOLZANO alluded to his concept of continuity applied to the exponentiation and to power series in order to obtain the binomial theorem for all real exponents.[37]

10.4 New types of series

The series discussed thus far have all been power series but in the early nineteenth century, this situation changed. Series which were not power series had emerged in various contexts in the eighteenth century but became very important in the first decades of the nineteenth century, mainly through investigations in the theory of heat conducted by J. B. J. FOURIER (1768–1830).[38]

FOURIER'S term-wise integration. From the first decade of the nineteenth century, FOURIER had begun representing physical phenomena — mainly heat conduction — by trigonometric series. In 1822, his investigations were published as a monograph.[39] One of FOURIER'S central tricks was the term-wise integration of an infinite series employed to obtain the *Fourier coefficients* in the following way. Assuming that a function $\phi(x)$ could be expanded as

$$\phi(x) = \sum_{i=1}^{\infty} a_i \sin ix, \tag{10.2}$$

36 *"[...] atque hinc in genere manifestum fore*

$$\left[\frac{i}{a}\right] = (1+x)^{\frac{i}{a}},$$

ita ut iam demonstratum sit theorema nostrum verum esse, si pro exponente n fractio quaecunque $\frac{i}{a}$ accipiatur, unde veritas iam est evicta pro omnibus numeris positivis loco exponentis n accipiendis." (L. Euler, 1775, 215–216).

37 (Bolzano, 1816, §46).

38 FOURIER and his works leading to Fourier series have been widely studied, see e.g. (Bottazzini, 1986; I. Grattan-Guinness, 1972).

39 (Fourier, 1822).

Figure 10.2: JEAN BAPTISTE JOSEPH FOURIER (1768–1830)

FOURIER multiplied both sides of the equation by $\sin nx$ and integrated from 0 to π,

$$\int_0^\pi \phi\,(x) \sin nx\, dx = \sum_{i=1}^{\infty} a_i \int \sin ix \sin nx\, dx,$$

where the integration of the sum was carried out term-wise. By the orthogonality,

$$\int \sin ix \sin nx\, dx = \begin{cases} \frac{\pi}{2} \text{if } n = i, \\ 0 \text{otherwise,} \end{cases}$$

FOURIER found the coefficients of the expansion (10.2) to be

$$\frac{\pi}{2} a_i = \int_0^\pi \phi\,(x) \sin ix\, dx.$$

The interchange of summation and integration would soon become a point of objection against FOURIER'S rigor.

SIMÉON-DENIS POISSON'S peculiar example. Almost simultaneous with FOURIER'S first investigations, a problem arose which also involved series which were not power-series. The problem was raised by SIMÉON-DENIS POISSON in 1811 and was intensively debated for the next decades, in particular in the French journal *Annales de mathématiques pures et appliquées*.[40]

40 This is well described in (Jahnke, 1987, 105–117) and (Bottazzini, 1990, lx–lxiii).

It all began when POISSON noticed that a peculiar situation arose from letting $m = \frac{1}{3}$ and $x = \pi$ in the binomial expansion of $(2\cos x)^m$,[41]

$$(2\cos x)^m = \sum_{n=0}^{\infty} \frac{\prod_{k=0}^{n-1}(m-k)}{n!} \cos\left((m-2n)\,x\right).$$

In a short paper, POISSON observed that the left hand side had the three values

$$-\sqrt[3]{2},\ \sqrt[3]{2}\left(\frac{1+\sqrt{-3}}{2}\right), \text{ and } \sqrt[3]{2}\left(\frac{1-\sqrt{-3}}{2}\right)$$

although the right hand side was a single-valued function,

$$\cos\left(\frac{\pi}{3}\right)\sum_{n=0}^{\infty}\frac{\prod_{k=0}^{n-1}\left(\frac{1}{3}-k\right)}{n!} = \frac{1}{2}\times(1+1)^{\frac{1}{3}} = \frac{\sqrt[3]{2}}{2}.$$

Thus, the sum of the series on the right hand side corresponded to neither of the values of the expression on the left hand side but was the average of its two complex values.

Poisson's example is a particular example of the kind of strange relations which could result by interpreting formal equalities in situations outside the domain of numerical equality. In this sense, it is similar to the peculiar formal equality

$$\frac{1}{2} = 1 - 1 + 1 - 1 + \dots$$

which had puzzled mathematicians in the eighteenth century. However, *Poisson's example* was a *convergent* series and the problem was that it did not agree with its *true value*.

Upon POISSON'S publication, mathematicians sought to understand how and why this peculiarity emerged, and the debate also spread to Berlin. In Berlin, A. L. CRELLE (1780–1855) and M. OHM (1792–1872) became interested in the explanation of this phenomenon — as did the mysterious L. OLIVIER who will appear prominently in chapter 13.[42] ABEL also became acquainted with the problem and it provoked him into producing a new proof of the binomial theorem. Before attention is focused on ABEL'S work in the theory of series, the following chapter is devoted to his greatest inspiration in the field: CAUCHY'S *Cours d'analyse*.

41 (Poisson, 1811). POISSON wrote $x = 200^\circ$ and thus adhered to the new radian system.
42 See e.g. (Jahnke, 1987, 105–117).

Chapter 11

CAUCHY's new foundation for analysis

Against the background of J. L. LAGRANGE'S (1736–1813) algebraic foundation for the calculus, another and radically different program of rigorization emerged when A.-L. CAUCHY (1789–1857) set out to write a textbook suitable for his courses at the *École Polytechnique*. In a sense, CAUCHY'S famous textbook *Cours d'analyse* continues the Lagrangian *program* as its subtitle *Analyse algébrique* testifies,[1] but its contents constituted a remarkable break with the Lagrangian *system*. In the *Cours d'analyse*, CAUCHY reformulated and revised the theory of infinite series from his novel viewpoint based on a shift in the conception of equality (see below).[2] Later, CAUCHY continued the reworking of the foundations of the calculus in two further textbooks dealing with the differential and integral calculus (see table 11.1).

The Lagrangian foundation for the calculus relied on a notion of equality between functions (expressions) which was largely *formal* and had been inherited from L. EULER (1707–1783) (see chapter 10). In CAUCHY'S hands, the concept of equality shifted toward focusing on numerical or arithmetical equality: to CAUCHY, two functions were *only* equal if they produced equal numerical results for equal numerical values of the arguments. By way of a few central examples, I will document how this change of approach was implemented and what its consequences were.

11.1 Programmatic focus on arithmetical equality

Generality of algebra. Describing the methods used in the *Cours d'analyse*, CAUCHY stressed the way in which he had fought to obtain the standard of rigor which is characteristic of geometry by denouncing arguments relying on the "generality of algebra". Such arguments could be, CAUCHY admitted, suitable inductions for obtaining

1 (A.-L. Cauchy, 1821b).

2 CAUCHY'S *Cours d'analyse* marks a turning point in the history of the calculus and has been given due attention by historians of mathematics. The most comprehensive presentation is probably BOTTAZZINI'S introduction to a photographic reproduction of the *Cours d'analyse* (Bottazzini, 1990). In particular, the section entitled *The "Generality of Algebra"* (ibid., xliv–xcvii) is of direct relevance to the present discussion.

1821	Cours d'analyse d'École Royale Polytechnique. Première partie. Analyse algébrique
1823	Résumé des leçons données a l'École Royale Polytechnique sur le calcul infinitésimal
1829	Leçons sur le calcul différentielle

Table 11.1: CAUCHY's textbooks on the calculus

the truth but should never be allowed to act as exact proofs. In particular, CAUCHY mentioned how arguments by the generality of algebra had been leading mathematicians into unfounded passages "from convergent to divergent series, from real quantities to imaginary expressions".[3] CAUCHY continued,

> "Similarly, one should realize that they [arguments by the generality of algebra] tend to attribute to algebraic formulae an indefinite extension whereas in reality, the majority of these formulae only subsists under certain conditions and for certain values of the quantities which they contain."[4]

Important examples of the problems which this requirement addressed was the relationship between formulae such as

$$\frac{1}{1-x} \text{ and } \sum_{n=0}^{\infty} x^n \tag{11.1}$$

which have been described above. Mathematicians unknowingly adhering to the formal concept of equality had been aware that counter-intuitive results could emerge if numerical values were inserted into the two expressions and their equality was extended to cover numerical values. For instance,

$$\frac{1}{1-(-1)} = \frac{1}{2}, \text{but } \sum_{n=0}^{\infty} (-1)^n = 1 - 1 + 1 - 1 + \ldots$$

and the sum did certainly not represent the value $\frac{1}{2}$ in any numerical sense.

CAUCHY removed these anomalies by dismissing the concept of formal equality and carefully analyzing the conditions under which a numerical equality between expressions such as (11.1) would hold. Thus, he found and emphasized that for numerical equality it was required that $|x| < 1$, and consequently the peculiar results were all explained away.

3 (A.-L. Cauchy, 1821a, iii).

4 "*On doit même observer qu'elles tendent à faire attribuer aux formules algébriques une étendue indéfinie, tandis que, dans la réalité, la plupart de ces formules subsistent uniquement sous certaines conditions, et pour certaines valeurs des quantités qu'elles renferment.*" (ibid., iii).

11.2 CAUCHY's concepts of limits and infinitesimals

In the preliminaries, CAUCHY defined what he understood under the terms *limit* and *infinitely small*. The two concepts are closely related in CAUCHY'S book although their interrelation is not trivial. The proper interpretation of the concepts has sparked some controversy in the historical literature which has sometimes chosen to focus on one of the concepts and neglecting the other. To be fair to the source and to facilitate a discussion of N. H. ABEL'S (1802–1829) reading of it, both CAUCHY'S definitions are reproduced and translated here.[5]

First, CAUCHY defined his concept of *limit*:

> "Whenever the values successively attributed to one and the same variable approach a fixed value indefinitely in such a way that it eventually differs from it by as little as one desires, the latter is called the *limit* of all the others."[6]

A few sentences later, CAUCHY gave his definition of *infinitely small quantities* which he based on the notion of limits:

> "Whenever the successive numerical values of one and the same variable decrease indefinitely in such a way that they become less than any given number, this variable becomes what is called *infinitely small* or an *infinitely small quantity*. A variable of this kind has zero for limit."[7]

Various conceptions of limits and infinitesimals had previously been suggested as the foundations for the calculus and EULER called the calculus the "algebra of zeros" because of his prolific use of infinitesimals. However, CAUCHY gave a process-based definition of *limits* and — more importantly — showed how to work with it. Correspondingly, to CAUCHY, infinitesimals were *variable quantities* which were involved in limit processes and could be made as small as desired by particular choices of the variable of the limit process.

It has puzzled certain historians of mathematics why CAUCHY simultaneously employed limits and retained the older concept of (completed) infinitesimals.[8] The fact remains that CAUCHY employed both concepts in different proofs and probably thought of them as equivalent but suited for different purposes.[9]

Importantly, CAUCHY sometimes used symbols to denote infinitely small quantities which were really (according to the definition) variables which tended toward the

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5 A good interpretation is provided in (Grabiner, 1981b, 80–81).

6 "*Lorsque les valeurs successivement attribuées à une même variable s'approchent indéfiniment d'une valeur fixe, de manière à finir par en différer aussi peu que l'on voudra, cette dernière est appelée la* limite *de toutes les autres.*" (A.-L. Cauchy, 1821a, 4).

7 "*Lorsque les valeurs numériques successives d'une même variable décroissent indéfiniment, de manière à s'abaisser au-dessous de tout nombre donné, cette variable devient ce qu'on nomme un* infiniment petit *ou une quantité* infiniment petite. *Une variable de cette espèce a zéro pour limite.*" (ibid., 4).

8 See discussion in (Lützen, 1999, 198–211).

9 See e.g. (Grabiner, 1981b, 87).

limit zero. However, by introducing symbols, the process under which the variable vanished was obscured and the order in which limit processes were conducted was not explicit.

11.3 Divergent series have no sum

When CAUCHY extended the procedure of analyzing the requirements for numerical equality involving series, he was led to a conclusion which he knew would be painful for his contemporaries to accept.

> "It is true that in order to always remain faithful to these principles, I see myself forced to accept multiple propositions which may appear a bit harsh at first sight. For instance, in the sixth chapter, I announce that *a divergent series has no sum*."[10]

CAUCHY'S treatment of series began with series of positive real terms (section VI.2), was then extended to series of general real terms (section VI.3), before he went on to treat series with complex terms (chapter IX). In the sixth chapter, CAUCHY elaborated his definition of convergence and his attitude toward divergent series.

> "Let
>
> $$s_n = u_0 + u_1 + u_2 + \cdots + u_{n-1}$$
>
> be the sum of the first n terms where n designates any integer. If, for ever increasing values of n, the sum s_n approaches a certain limit s indefinitely, the series is said to be *convergent* and the above mentioned limit is called the *sum* of the series. In the contrary case, if the sum s_n does not approach any fixed limit when n increases indefinitely, the series is *divergent* and no longer has a sum."[11]

As the quotations demonstrate, CAUCHY sought to limit the concept of "sum of a series" to apply only to *convergent* series. This position was radicalized by ABEL in his correspondence as we will see in section 12.3: ABEL wanted an outright ban on divergent series and saw them as the creation of the Devil. To CAUCHY, who was also a very creative mathematician outside fundamental issues, divergent series remained of interest in asymptotic mathematics; only in questions of foundational nature, they were not attributed any sum.

---

10 "*Il est vrai que, pour rester constamment fidèle à ces principes, je me suis vu forcé d'admettre plusieurs propositions qui paraîtront peut-être un peu dures au premier abord. Par exemple, j'énonce dans le chapitre VI, qu'*un série divergente n'a pas de somme *[...]*" (A.-L. Cauchy, 1821a, iv).

11 "*Soit*

$$s_n = u_0 + u_1 + u_2 + \cdots + u_{n-1}$$

la somme des n premiers termes, n désignant un nombre entier quelconque. Si, pour des valeurs de n toujours croissantes, la somme s_n s'approche indéfiniment d'une certaine limite s; la série sera dite convergente, *et la limite en question s'appellera la somme de la série. Au contraire, si, tandis que n croît indéfiniment, la somme s_n ne s'approche d'aucune limite fixe, la série sera divergente, et n'aura plus de somme.*" (ibid., 123).

For the moment, focus will be given to another intrinsic aspect of CAUCHY'S definition: the position of certain concepts and theorems within the theoretical framework. If only convergent series were allowed to have a sum, and knowledge of the *value* of the sum was required to apply the definition to determine whether the series was convergent or not, a problem emerged. Phrased in modern notation, it is not practical to attempt establishing that some s exists such that

$$|s_n - s| \to 0 \text{ as } n \to \infty$$

without knowing *which* s could be a candidate; for complicated or general series, such candidates might not be available. Therefore, CAUCHY'S theoretical complex — in an essential way — required a means of investigating convergence without any prior knowledge of the purported sum. This important problem was met by a number of criteria of convergence which were also among the chief innovations in the *Cours d'analyse*.

After giving his definition of convergence, CAUCHY gave a first characterization of his new concept, which was, however, of little use in practically establishing convergence (see below).

> "Thus, for the series $u_0 + u_1 + u_2 + \cdots + u_n + \ldots$ to be convergent, it is first necessary that the general term u_n decreases indefinitely when n grows. However, this condition does not suffice and it must also be so that for increasing values of n the different sums
>
> $$\begin{gathered} u_n + u_{n+1}, \\ u_n + u_{n+1} + u_{n+2}, \\ \text{etc.,} \end{gathered}$$
>
> i.e. the sums of the quantities
>
> $$u_n, u_{n+1}, u_{n+2}, \text{ etc.}$$
>
> taken starting from the first and to whatever number one may wish, eventually always produce numerical values less than any assignable limit. Conversely, whenever these two conditions are fulfilled, the convergence of the series is assured."[12]

12 "*Donc, pour que la série (1) soit convergente, il est d'abord nécessaire que le terme général u_n décroisse indéfiniment, tandis que n augmente; mais cette condition ne suffit pas, et il faut encore que, pour des valeurs croissantes de n, les différentes sommes*

$$\begin{gathered} u_n + u_{n+1}, \\ u_n + u_{n+1} + u_{n+2}, \\ \&c\ldots \end{gathered}$$

c'est-à-dire, les sommes des quantités

$$u_n, u_{n+1}, u_{n+2}, \&c\ldots$$

prises, à partir de la première, en tel nombre que l'on voudra, finissent par obtenir constamment des valeurs numériques inférieures à toute limite assignable. Réciproquement, lorsque ces diverses conditions sont remplies, la convergence de la série est assurée." (ibid., 125–126).

In the second half of the nineteenth century, CAUCHY'S characterization of convergence by means of the so-called *Cauchy criterion* became even more important when it was realized, that no proof of CAUCHY'S last assertion could be given. The solution devised by mathematicians such as J. W. R. DEDEKIND (1831–1916) and G. CANTOR (1845–1918) to secure the validity of CAUCHY'S second claim was to *construct* the real numbers in such ways that they possessed this property of *completeness*.

11.4 Means of testing for convergence of series

With his new emphasis on convergence, CAUCHY'S theory needed criteria of convergence which would operate simply from the general terms without any information about the sum. Based on previous observations, CAUCHY established three important such tests which are still all important today. First, he proved the root test to the effect that if $\sqrt[n]{u_n}$ has a limit k as $n \to \infty$, the series $\sum u_n$ will be convergent if $k < 1$ and divergent if $k > 1$. In case $k = 1$, nothing could be said of the convergence by this criterion. CAUCHY then transformed the root test to obtain the ratio test (see below) and a logarithmic criterion by comparison with the harmonic series.

It is a general feature of these criteria of convergence that there are cases for which they do not provide any information concerning convergence. As we will see in chapter 13, the search for a complete test of convergence which could *always* determine the convergence or divergence of series from its general term was also actively pursued in the 1820s.

CAUCHY'S proof of the ratio test. CAUCHY'S proved his criteria of convergence by an ingenious route albeit complicated.[13] First, he proved the root test directly from the assumptions and the previously established convergence of the geometric progression. Next, he referred to a previously established theorem to the effect that if the sequences $\{\sqrt[n]{u_n}\}$ and $\left\{\frac{u_{n+1}}{u_n}\right\}$ were both convergent, their limits would be equal. Ultimately, this theorem provided the proof of the ratio test. To get a grasp of the way CAUCHY reasoned with his concepts and the way in which he obtained his criteria, the details of his proof are considered.[14] Later, after ABEL'S way of commencing the theory of series has been described, the role of the ratio test in the two theories can be discussed.

CAUCHY proved the convergence part of the root test by letting U denote a number $k < U < 1$. Then, he observed that a large integer n had to exist such that for any larger number, say $N \geq n$,

$$\sqrt[N]{u_N} < U, \text{ i.e. } u_N < U^N.$$

13 (A.-L. Cauchy, 1821a, 132–135).

14 Today, this theorem is standard textbook material in basic calculus courses. The modern proof closely resembles CAUCHY'S proof.

Thus, the tail of the series was term-wise less than a convergent geometric progression, and the convergence of the series $\sum u_n$ was concluded. Similarly, CAUCHY proved the divergence part of the root test by comparing with a divergent geometric progression.

CAUCHY based his proof of the ratio test on the following result which he had previously obtained.

> "2nd theorem. If the function $f(x)$ is positive for large values of x and the ratio
> $$\frac{f(x+1)}{f(x)}$$
> converges toward the limit k when x increases indefinitely, the expression
> $$[f(x)]^{\frac{1}{x}}$$
> will converge at the same time toward the same limit."[15]

In his proof, CAUCHY distinguished between two cases namely k finite or not; we shall here only be concerned with the case where k is finite. CAUCHY let ε denote an as yet unspecified number which was presumably very small. By assuming that for $x \geq h$,

$$\frac{f(x+1)}{f(x)} \in [k-\varepsilon, k+\varepsilon],$$

CAUCHY found by the theory of means which he developed in a note[16] that the geometric mean[17] of

$$\frac{f(h+1)}{f(h)}, \frac{f(h+2)}{f(h+1)}, \ldots, \frac{f(h+n)}{f(h+n-1)},$$

would also belong to this interval, i.e.

$$\sqrt[n]{\frac{f(h+n)}{f(h)}} = k+\alpha, \quad \alpha \in [-\varepsilon, \varepsilon].$$

Then, CAUCHY found by inserting $x = h+n$

$$f(x) = f(h) \cdot (k+\alpha)^{x-h}$$

which meant

$$f(x)^{\frac{1}{x}} = f(h)^{\frac{1}{x}} \cdot (k+\alpha)^{1-\frac{h}{x}} \underset{x\to\infty}{\to} k+\alpha.$$

15 *"2.e Théorème. Si, la fonction $f(x)$ étant positive pour de très-grandes valeurs de x, le rapport*

$$\frac{f(x+1)}{f(x)}$$

converge, tandis que x croit indéfiniment, vers la limite k, l'expression

$$[f(x)]^{\frac{1}{x}}$$

convergera en même temps vers la même limite." (ibid., 53–54).

16 (ibid., note II).

17 The geometric mean of the quantities $a_1, \ldots, a_n$ was the quantity $\sqrt[n]{\prod_{k=1}^{n} a_k}$.

Thus, the limit of $f(x)^{\frac{1}{x}}$ belonged to the same arbitrarily small interval as the limit of $\frac{f(x+1)}{f(x)}$, and thus the two limits were equal. Now, the ratio test followed by letting $f(n) = u_n$ and observing that the limits of $\sqrt[n]{u_n}$ and $\frac{u_{n+1}}{u_n}$ coincided.

11.5 CAUCHY's proof of the binomial theorem

CAUCHY agreed with his predecessors in considering the binomial theorem a corner stone of the calculus. His proof of it relied on and promoted two of his new techniques and concepts in analysis: those of functional equations and continuous functions.[18] As EULER had done, CAUCHY considered the functional equation

$$f(m) f(n) = f(m+n) \tag{11.2}$$

of which he knew that the binomial

$$f(m) = (1+x)^m \tag{11.3}$$

was a continuous solution for all m provided x was fixed. On the other hand, the function defined by the infinite series

$$\sum_{k=0}^{\infty} \binom{m}{k} x^k \tag{11.4}$$

was also a solution to the functional equation (11.2) under the assumptions that it converged and m was a rational number.

To demonstrate that the series (11.4) satisfied the functional equation, CAUCHY had to be able to multiply infinite series. He invented a way of multiplying *absolutely convergent* series which rigorously established the convergence of the product.[19] Based on the argument which EULER had also used, CAUCHY then knew that the series (11.4) coincided with $f(m)$ for all rational values of m. Therefore, the general equality of (11.3) and (11.4) would be proved if the series was a continuous function of m. In order to prove the continuity of the series (11.4), CAUCHY devised and proved a general theorem to the effect that a convergent sum of continuous functions was always a continuous function. Later, this theorem would arouse much controversy (see below).

CAUCHY'S way of multiplying infinite series. In the *Cours d'analyse*, CAUCHY invented a way of multiplying two absolutely convergent series such that the product would be a new convergent series. As he had done throughout, CAUCHY developed his theory of infinite series in three steps:

1. Series of real, positive terms (section VI.2)

18 For CAUCHY'S theory of functional equations, see (J. Dhombres, 1992).
19 (A.-L. Cauchy, 1821a, 157).

2. Series of general real terms (section VI.3)

3. Series of complex terms (chapter IX)

In each of these three theories, CAUCHY developed a product theorem,[20] and CAUCHY'S proofs will be relevant when compared with ABEL'S subsequent proofs. The multiplication theorems applying to series of real terms (the first and second) are the most interesting for the present study. In his theory of series of positive terms, CAUCHY had stated and proved that if two series $\sum u_n$ and $\sum v_n$ were convergent and converged toward s and s', the series whose general term was

$$w_k = \sum_{n+m=k} u_n v_m \tag{11.5}$$

would be convergent and converge toward the product ss'. When he wanted to generalize this theorem to general series of real terms, CAUCHY imposed the restriction that each of the series $\sum u_n$ and $\sum v_n$ was to be convergent when their terms were replaced by their absolute values, i.e. both factors were to be *absolutely convergent*, although the term and an elaborate concept was only invented some years later (see section 12.7).

In the first case, in which all terms were positive quantities, CAUCHY proved the theorem by a direct argument. He let s''_n designate the sum of the first n terms of the purported product series (11.5) and defined

$$m = \begin{cases} \dfrac{n-1}{2} \text{if } n \text{ is odd} \\ \dfrac{n-2}{2} \text{if } n \text{ is even} \end{cases}$$

to obtain

$$\sum_{k=0}^{n-1} w_k < \left(\sum_{k=0}^{n-1} u_k\right)\left(\sum_{k=0}^{n-1} v_k\right) \text{ and}$$
$$\sum_{k=0}^{n-1} w_k > \left(\sum_{k=0}^{m} u_k\right)\left(\sum_{k=0}^{m} v_k\right).$$

He had thus obtained

$$s_{m+1}s'_{m+1} < s''_n < s_n s'_n,$$

and by letting m grow beyond all bounds, the theorem was established.

When he came to generalize this theorem to the case of general real terms, CAUCHY wanted to apply the simpler case of series with positive terms. With the same notation as above, he obtained the formula

$$s'_n s_n - s''_n = \sum_{t=n}^{2n-2} \sum_{m+k=t} u_m v_k, \tag{11.6}$$

20 (ibid., 141–142,147–149,283–285).

and if the terms u_m and v_k were positive, the difference (11.6) would converge to zero as a consequence of the theorem for series of positive terms. If the terms were not all positive, CAUCHY could still apply the theorem to the corresponding series of numerical values $\rho_n = |u_n|$ and $\rho'_n = |v_n|$. Therefore, in this case, the difference (11.6) would still converge to zero and yet majorize the series of the original terms. Thus, CAUCHY had established the convergence and the product equation for absolutely convergent series of real terms.

CAUCHY'S concept of continuous functions. CAUCHY'S concept of continuous functions (see below) was among the main innovations of his new calculus. There, in the definition and in the proofs, his new foundation on an algebraic concept of limits played its most important role. In the eighteenth century, EULER had used the term *continuous* to indicate that the function was defined by the same analytic expression throughout its domain.[21] However, in the *Cours d'analyse*, CAUCHY took it upon himself to completely redefine the concept of continuous functions in order to capture a different property of the functions: their continuous, unbroken variation.

> "Let $f(x)$ be a function of the variable x and suppose that for every value of x between two given boundaries this function always takes a unique and finite value. If, starting from a value of x contained between these boundaries, one attributes to x an infinitely small increment α, the function will receive the increment
>
> $$f(x+\alpha) - f(x)$$
>
> which depends simultaneously on the new variable α and the value of x. Between the two boundaries assigned to the variable x, the function $f(x)$ will be a *continuous* function of this variable if for every value of x between these boundaries, the numerical value of the difference
>
> $$f(x+\alpha) - f(x)$$
>
> decreases indefinitely with that [numerical value] of α. In other words, *the function $f(x)$ remains continuous with respect to x between the given boundaries if, between these boundaries, an infinitely small increment of the variable produces an infinitely small increment of the function.*"[22]

---

21 (L. Euler, 1748). See (Lützen, 1978) and (Youschkevitch, 1976).

22 "*Soit $f(x)$ une fonction de la variable x, et supposons que, pour chaque valeur de x intermédiaire entre deux limites données, cette fonction admette constamment une valeur unique et finie. Si, en partant d'une valeur de x comprise entre ces limites, on attribue à la variable x un accroissement infiniment petit α, la fonction elle-même recevra pour accroissement la différence*

$$f(x+\alpha) - f(x),$$

qui dépendra en même temps de la nouvelle variable α et da la valeur de x. Cela posé, la fonction $f(x)$ sera, entre les deux limites assignées à la variable x, fonction continue de cette variable, si, pour chaque valeur de x intermédiaire entre ces limites, la valeur numérique de la différence

$$f(x+\alpha) - f(x)$$

décroit indéfiniment avec celle de α. En d'autres termes, la fonction $f(x)$ restera continue par rapport à x entre les limites données, si, entre ces limites, un accroissement infiniment petit de la va-

CAUCHY'S novel definition defines continuity not at a point (as is customary today, see below) but on an entire interval enclosed by two boundary points.[23] Thus, CAUCHY'S implicit choice of infinitesimal ω such that

$$|f(x+\alpha) - f(x)| = \omega$$

could seem to be independent of x on the interval and the definition would actually be of what is now known as a *uniformly continuous function*. The doubt over the proper interpretation is introduced by the fact CAUCHY'S use of the symbol ω to designate the infinitesimal: it "hides" the order in which the limit processes are to be carried out.

CAUCHY and series of functions. In a famous theorem which provided an important step in CAUCHY'S proof of the binomial theorem, CAUCHY sought to link the concepts of convergence and continuity. Since we will discuss the theorem in details in chapter 12, its entire wording and CAUCHY'S proof of it are reproduced here.

> "1st theorem. *Whenever the different terms of the series $u_0 + u_1 + u_2 + \cdots + u_n + \ldots$ are functions of one and the same variable x and continuous with respect to this variable in the neighborhood of a particular value for which the series is convergent, the sum s of the series is also a continuous function of x in the neighborhood of that particular value.*"[24]

As was customary, CAUCHY actually presented the proof before he made the theorem explicit. The proof which he gave proceeded along the following lines. If the sum is split after n terms

$$s = s_n + r_n = \sum_{k=0}^{n-1} u_n + \sum_{k=n}^{\infty} u_n, \tag{11.7}$$

the partial sum s_n is a polynomial and therefore continuous and the remainder r_n can be made less than any given quantity by the convergence of the series. In consequence, the difference $s(x+\alpha) - s(x)$ could be made less than any assignable quantity and the sum was therefore continuous. As we are well aware today, with our common interpretations of the basic concepts of continuity, limits, and convergence, the theorem is false as stated. In section 14.1.2, its future history through the works of ABEL, P. L. VON SEIDEL (1821–1896) (and G. G. STOKES (1819–1903)) and CAUCHY again is outlined to understand how ABEL'S contribution to rigorization was accepted and interpreted.

riable produit toujours un accroissement infiniment petit de la fonction elle-même." (A.-L. Cauchy, 1821a, 34–35).

23 See also (Bottazzini, 1990, lxxxi–lxxxiii) and (Giusti, 1984).

24 "*1.er Théorème.* Lorsque les différens termes de la série (1) sont des fonctions d'une même variable x, continues par rapport à cette variable dans le voisinage d'une valeur particulière pour laquelle la série est convergente, la somme s de la série est aussi, dans le voisinage de cette valeur particulière, fonction continue de x." (A.-L. Cauchy, 1821a, 131–132).

For now, we have to stress the importance this theorem played in CAUCHY'S proof of the binomial theorem. The argument was, that since both the binomial $(1+x)^m$ and the infinite power series (11.4) satisfied the same functional equation for all rational m, and since they were both continuous functions of m, they would coincide for all real values of m. Thus, CAUCHY proved the binomial theorem for all real exponents (here designated m).

In CAUCHY'S theory of rigorously restructuring calculus, the binomial theorem played an extremely important role.[25] It provided one of the basic, theoretical bricks which could be used to rebuild the existing theory of real analysis. Historians have argued that CAUCHY'S rigorization and his proof of the binomial theorem constitute a veiled attack on Fourier series.[26] However, although CAUCHY was critical toward J. B. J. FOURIER'S (1768–1830) reasoning, I find such a hypothesis largely unnecessary as CAUCHY'S rigorization program makes good sense from its own premises.[27]

11.6 Early reception of CAUCHY's new rigor

CAUCHY'S *Cours d'analyse* dealt exclusively with the theory functions from the perspective of infinite series. Later in the 1820s, he also published lectures pertaining to rigorously founding the theory of differentiation and integration.[28]

As a textbook for the *École Polytechnique*, the *Cours d'analyse* was not successful. Because of internal animosities among the teachers, CAUCHY'S textbook was never used as a textbook but it may have served as inspiration for students preparing for the entrance exams of the school. Among his fellow mathematicians, CAUCHY'S program also received mixed reactions. In Germany, A. L. CRELLE (1780–1855) mentioned CAUCHY'S textbooks in very positive terms,[29] and a German translation of the *Cours d'analyse* appeared in 1828.[30] However, a distinct German reaction also existed which sought to continue the formal, algebraic approach to foundations of analysis in the so-called *combinatorial school* initiated by C. F. HINDENBURG (1741–1808), and M. OHM (1792–1872) pursued his own rigorization program.[31]

Thus, in the 1820s, the mathematical community could be divided into three camps reflecting their attitudes toward rigor:

1. Some had picked up CAUCHY'S vision of a rigorization of analysis; both its theme and its tools. They joined in the restriction to arithmetical equality and adopted CAUCHY'S redefinition of central concepts in terms of limits.

25 (Grabiner, 1981b, 111).
26 See e.g. (Bottazzini, 1986, 110).
27 (Grabiner, 1981b, 111).
28 (A.-L. Cauchy, 1823; A.-L. Cauchy, 1829).
29 (Crelle, 1827; Crelle, 1828).
30 (A. L. Cauchy, 1828).
31 See (Jahnke, 1992).

2. Others, primarily Germans, felt a similar need for a new rigorous foundation of the calculus. Often inspired by needs stimulated by the German educational reforms, they sought to distill a combinatorial theory from the approaches of LAGRANGE and others.

3. The rest, and the vast majority, were mostly concerned with contributing *new* mathematical knowledge. Although they probably also sometimes worried about the foundations of their discipline, they left it to the teachers and experts to straighten it out.

In the 1820s, the need for rigorization had entered the agenda and at least two programs had been suggested for resolving the need. In the course of the century, the need became even more urgent and CAUCHY'S conception became the preferred solution — after it had undergone the interpretations of ABEL, G. P. L. DIRICHLET (1805–1859), G. F. B. RIEMANN (1826–1866), K. T. W. WEIERSTRASS (1815–1897) and others.

Chapter 12

ABEL's reading of CAUCHY's new rigor and the binomial theorem

N. H. ABEL (1802–1829) was one of the first converts to A.-L. CAUCHY'S (1789–1857) new program of rigorizing analysis. Alone and together with B. M. HOLMBOE (1795–1850), ABEL had studied L. EULER'S (1707–1783) works on analysis and other textbooks on the calculus from the late eighteenth century.[1] But during his time in Berlin, ABEL learnt of CAUCHY'S textbooks *Cours d'analyse* and *Resumé des leçons*.[2] His acquaintance with these works left clear traces in his publications and letters. Although CAUCHY'S rigorization program comprised all of analysis (at least in principle), ABEL was particularly interested in the theory of series. ABEL'S interest in the theory of series manifested itself in two publications, numerous remarks in letters, and an interesting draft in one of his notebooks.

ABEL'S most prestigious contribution to the rigorization of analysis was a paper published in 1826 which contained a new proof of the binomial theorem.[3] ABEL had previously employed a complete induction argument to deduce the binomial *formula*.[4] The method of complete induction had previously been used by B. BOLZANO (1781–1848) in his *Der binomische Lehrsatz*,[5] and together with ABEL'S curious and flattering remark concerning BOLZANO (see page 42), this might suggest that ABEL was familiar with this work. However, there is no direct evidence to support this speculation.

When it came to proving the binomial *theorem*, ABEL followed the path set out by CAUCHY'S *Cours d'analyse*, which he praised highly, recommending it to anybody who loved the rigor of mathematics.[6] However, in certain details, ABEL'S deduction differed slightly from the guideline of the *Cours d'analyse*. In particular, ABEL presented his own way of deducing the important ratio tests and when he found that *Cauchy's*

1 See the sections 3.2 and 2.2, above.
2 (A.-L. Cauchy, 1821a; A.-L. Cauchy, 1823).
3 (N. H. Abel, 1826f).
4 (N. H. Abel, 1826b).
5 (Bolzano, 1816, §7–10).
6 (N. H. Abel, 1826f, 313).

Theorem "suffered exceptions", he replaced it with a tailored — but also problematic — theorem which he found sufficient to carry through his argument. The description and analysis of all these aspects and the differences between ABEL'S and CAUCHY'S concepts and proofs are the purposes of the present chapter.

The *Cours d'analyse* was certainly ABEL'S main inspiration for his research on the theory of series. From another one of his letters,[7] we learn that ABEL had bought and read the first nine issues of CAUCHY'S *Exercises des mathématiques.* Although they proved highly interesting in another context,[8] these installments contained nothing with an explicit bearing on the rigorization of the theory of series. Another one of CAUCHY'S publications — the *Resumé des leçons* — did address infinite series and, as noticed, we know from one of his letters that ABEL was familiar with it. Continuing the quotation on page 31 taken from a letter to HOLMBOE, ABEL expressed his concerns over the state of analysis (see below) and wrote:

> "The Taylorian Theorem, the foundation for all higher mathematics is equally ill founded. Only one rigorous proof have I found and that is by Cauchy in his Resumé des leçons sur le calcul infinitesimal. There, he proves that
>
> $$\phi\left(x+\alpha\right)=\phi x+\alpha\phi' x+\frac{\alpha^2}{2}\phi'' x+\dots$$
>
> whenever the series is convergent (but it is frequently used in all cases)."[9]

Thus, ABEL claimed that CAUCHY had proved in the *Resumés* that any convergent Taylor series expansion represents the function. Certainly, CAUCHY considered the theorem of B. TAYLOR (1685–1731) of major importance in his lectures and repeated his programmatic criticism of working with divergent series. However, the statement which ABEL attributed to CAUCHY is exactly what CAUCHY criticized by ways of a counter example in the *Resumé*: In the *Résume*, CAUCHY considered the function defined by $f\left(x\right)=e^{-\frac{1}{x^2}}$ whose derivatives all vanished at the origin and whose Maclaurin series therefore was the zero function. This counter example and the use which CAUCHY made of it will be discussed further in section 21.3.

Thus, it appears that ABEL misread or misunderstood CAUCHY. How could a smart mathematician who was — in the words of I. GRATTAN-GUINNESS — was "more Cauchyian than Cauchy" be led to such a statement?[10] A hint may be taken from

7 See the quotation p. 306.

8 See section 16.2.3.

9 "*Det Taylorske Theorem, Grundlaget for hele den høiere Mathematik er ligesaa slet begrundet. Kun eet eneste strængt Beviis har jeg fundet og det er af Cauchy i hans Resumé des leçons sur le calcul infinitesimal. Han viser der at man har:*

$$\phi\left(x+\alpha\right)=\phi x+\alpha\phi' x+\frac{\alpha^2}{2}\phi'' x+\dots$$

saa ofte Rækken er convergente, (men man bruger den rask væk i alle Tilfælde)." (Abel→Holmboe, 1826/01/16. N. H. Abel, 1902a, 16–17).

10 (I. Grattan-Guinness, 1970b, 80).

from a manuscript entitled *Sur les séries* which ABEL worked on and hoped to present in A. L. CRELLE'S (1780–1855) *Journal*. However, the publication never materialized and ABEL'S manuscript was left in the form of an interesting draft. It was eventually published in the *Œuvres*.[11] In the draft, ABEL expanded an otherwise unspecified function

$$f(x+\omega) = a_0 + a_1 (x+\omega) + a_2 (x+\omega)^2 + \dots \tag{12.1}$$

and rearranged its terms to find

$$f(x+\omega) = a_0 + a_1 x + a_2 x^2 + \cdots + (a_1 + 2a_2 x + \dots)\,\omega + \dots .$$

From this, ABEL concluded

$$f(x+\omega) = f(x) + \frac{f'(x)}{1}\omega + \frac{f''(x)}{2}\omega^2 + \dots \tag{12.2}$$

"if this series is convergent".[12] The argument was followed by remarks to the effect that the series of (12.2) was indeed always convergent! This serves to illustrate that despite the extensive criticism which ABEL raised against the unrigorous reasoning with series, his own reasoning was constantly at risk of making the same mistakes. Furthermore, the example shows how the reordering of terms was an unrealized problem in the 1820s. This becomes interesting when we consider the emergence of a concept of *absolute convergence* (see below).

12.1 ABEL's critical attitude

ABEL'S name is frequently mentioned in the same sentence as CAUCHY and K. T. W. WEIERSTRASS (1815–1897) when historians of mathematics attempt to pin-point the movement within mathematics known as *rigorization* or — more specifically — *arithmetization*.[13] And certainly, after an almost religious conversion, ABEL became an ardent follower of a version of CAUCHY'S new rigor; a version which ABEL to a large extent helped form, himself. On the other hand, rigorizing the calculus meant refounding the entire domain of analysis on a completely new system, and ABEL'S mathematical contribution to the rigorization was limited to a single sub-discipline, *the theory of infinite series*. But of equal importance, ABEL'S written testimony of his conversion to *Cauchyism* and his hearted, public interpretation of some of its doctrines helped shape the *movement* in the nineteenth century. In this and the following chapter, ABEL'S critical attitude as well as his contributions to the theory of series will be investigated and analyzed.

11 (N. H. Abel, [1827] 1881).
12 (ibid., 204).
13 See e.g. (Kline, 1990, 948).

ABEL'S critical attitude was expressed both in his letters and in the opening paragraphs as well as in the overall structure of his important paper on the binomial theorem. ABEL attacked the usual way of reasoning about infinite series which he saw as an induction from the permissible ways of reasoning about *finite* series, i.e. polynomials. This distinction between finite and infinite arguments was one of the core components of ABEL'S critical attitude.

A direct, outspoken ban on divergent series. When ABEL announced the contents of his binomial paper, he explicitly pointed to the fact that convergence of a series was a requirement to be established before anything like an equality between the series and other (possibly infinite) expressions were to be asserted. He wrote, combining his distinction between finite and infinite expressions and his criticism of divergent series,

> "This equation [the CAUCHY product of infinite series] is completely correct when both of the series
>
> $$u_0 + u_1 + \dots \text{ and } v_0 + v_1 + \dots$$
>
> are finite. If they are infinite, they must firstly necessarily *converge* — because a divergent series has no sum — and then the series in the second term [the CAUCHY product] must also converge. Only with these restrictions is the statement above correct. If I am not mistaken, this restriction has hitherto not been taken into account."[14]

Thus, even in the case of the CAUCHY multiplication of infinite series, ABEL found reason to criticize current practice on the same grounds as stated above; different reasoning applied to finite and infinite series, and divergent series have no sum. When ABEL'S proof of the multiplication theorem has been described, a further analysis of its relations to CAUCHY'S original version can be described (see section 12.8, below).

In some of his letters, ABEL was even more outspoken about the status of divergent series and the implications they had had on the development of rigorous mathematics.

ABEL'S notion of *rigorous proof* and critical revision. In another frequently quoted letter, written shortly after leaving Berlin in 1826 and directed to C. HANSTEEN (1784–1873), ABEL spoke of turning more of his attention toward the study of analysis, and again commented on the status of the field.

> "I will commit all my strength to shedding some light on the immense darkness, which incontestably covers analysis. It [analysis] completely lacks all plan

14 "*Diese Gleichung ist vollkommen richtig, wenn die beiden Reihen*

$$u_0 + u_1 + \dots \textit{ und } v_0 + v_1 + \dots$$

endlich sind. Sind sie aber unendlich, so müssen sie erstlich nothwendig convergiren*, weil eine divergirende Reihe keine Summe hat, und dann muß auch die Reihe im zweiten Gliede ebenfalls* convergiren. *Nur mit dieser Einschränkung ist der obige Ausdruck richtig. Irre ich nicht, so ist diese Einschränkung bis jetzt nicht berücksichtigt worden.*" (N. H. Abel, 1826f, 311).

> and coherence, so it is highly remarkable that it can be studied by so many — and now worst of all that it is not treated rigorously. [...] Whenever one proceeds in the ordinary fashion, it is probably all right; but I have had to be very cautious because the theorems which have been accepted 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 probing."[15]

Here, we learn of another distinction which ABEL saw between *ordinary* proofs and *rigorous* proofs. He was acutely aware that a fundamental change in the techniques of proving mathematical theorems was required. At the same time, ABEL also expressed the opinion that it would be interesting to investigate how unrigorous reasoning had led to correct results in almost all cases. This idea of critically *revising* the existing structure of a mathematical theory is as old as the rigorization movement and had first been stated in the Berlin Academy prize problem for 1784.[16] ABEL also suggested an answer to the question when he emphasized that analysis until most recently had only worked with power series and for these, the established methods of reasoning did not lead to false results. A completely different situation could arise if other series were included in the study,[17] ABEL remarked thereby alluding to both *Fourier series* and *Poisson's example* described above.

ABEL and the paradoxes of analysis. On two occasions, in letters to HOLMBOE and HANSTEEN written in 1826, ABEL described some of the paradoxes to which unrigorous reasoning had led.[18] Of the two, the letter to HOLMBOE is the most detailed. There, ABEL ridiculed anybody willing to claim equalities such as

$$0 = 1 - 2^n + 3^n - 4^n + \dots . \tag{12.3}$$

ABEL gave no references as to where he had picked up this absurd equality but we get a hint from another much more famous example which he described. In the letter and in an intriguing footnote in the binomial paper — which is discussed below, see 12.6 — ABEL called attention to the series

$$\frac{x}{2} = \sum_{n=1}^{\infty} \frac{(-1)^{n-1} \sin nx}{n} \tag{12.4}$$

15 "*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. [...] Naar man blot gaaer almindelig tilværks saa gaaer det nok; men jeg har maattet være særdeles forsigtig, thi de engang uden strængt Bevis (ↄ: uden Bevis) antagne Sætninger har slaaet saa dybe Rødder hos mig at jeg hvert Øjeblik staaer Fare at bruge dem uden nøiere Prøvelse.*" (Abel→Hansteen, Dresden, 1826/03/29. N. H. Abel, 1902a, 22–23).

16 (Grabiner, 1981b, 40–43), the passage on revision in the prize proposal is translated (ibid., 41).

17 (Abel→Hansteen, Dresden, 1826/03/29. N. H. Abel, 1902a, 22–23).

18 (Abel→Holmboe, 1826/01/16. In ibid., 13–19) and (Abel→Hansteen, Dresden, 1826/03/29. In ibid., 22–26).

which is the *Fourier series* corresponding to the function $f(x) = \frac{x}{2}$ on the interval $]-\pi, \pi[$. However, as ABEL noted in the letter, by inserting e.g. $x = \pi$ he would be led to the absurd equality

$$\frac{\pi}{2} = \sum_{n=1}^{\infty} \frac{(-1)^{n-1} \sin n\pi}{n} = 0.$$

In his letter, ABEL applied this example to illustrate that although an equality held for $x < \pi$ it could fail in the limit $x = \pi$. This criticism is thus an elaboration of CAUCHY'S dismissal of the *generality of algebra*. ABEL continued,

> "Operations are applied to infinite series as if they were finite but is that permissible? I doubt it. — Where has it been proved that one obtains the differential of a series by differentiating each term? It is easy to present examples for which this it not true."[19]

Here, ABEL indirectly criticized J. B. J. FOURIER'S (1768–1830) interchange of the limit processes involved in term-wise integration. Differentiating the series (12.4), ABEL obtained

$$\frac{1}{2} = \sum_{n=1}^{\infty} (-1)^{n-1} \cos nx$$

in which the series was divergent. Now, the example (12.3) can be seen to result if this procedure of differentiation is repeated and either $x = \pi$ or $x = \frac{\pi}{2}$ is inserted.

ABEL'S reaction to *Poisson's example*. A strong connection between ABEL'S research on the theory of series and *Poisson's example* is clearly discernible from his letter to HOLMBOE.[20] There, ABEL explained how he had undertaken to find the sum of the series

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

which was an important open problem at the time. ABEL mentioned that a large number of mathematicians (including S.-D. POISSON (1781–1840) and CRELLE) had failed to solve the problem but that he, himself, had found a complete answer in the form

$$\cos mx + m \cos(m-2)x + \frac{m(m-1)}{2} \cos(m-4)x + \cdots = (2 + 2\cos 2x)^{\frac{m}{2}} \cos mk\pi$$

in which $m > -1$, k an integer and $\left(k - \frac{1}{2}\right)\pi < x < \left(k + \frac{1}{2}\right)\pi$; for $m < -1$, the series was divergent and this led to ABEL'S outburst against the use of divergent series:

> "Divergent series are the creations of the Devil and it is a shame that anybody dare construct a demonstration upon them."[21]

19 "*Man anvender alle Operationer paa uendelige Rækker som om de vare endelige, men er dette tilladt? Vel neppe. — Hvor staar det beviist at man faaer Differentialet af en uendelig Række ved at differentiere hvert Led? Det er let at anføre Exempler hvor dette ikke er rigtigt.*" (Abel→Holmboe, 1826/01/16. N. H. Abel, 1902a, 18).

20 (Abel→Holmboe, 1826/01/16. In ibid., 13–19).

Thus, ABEL'S criticism of divergent series was intimately tied to his interest in *Poisson's example* and the other paradoxes in the theory of infinite series.

The core components of ABEL'S criticism. ABEL'S critical position toward the contemporary conceptions of rigor in analysis can thus be divided into three parts. The first part was primarily rhetorical emphasizing his belief that the accepted standards were utterly insufficient and ABEL supported his argument by enlisting a number of "paradoxes", in particular (12.3).

The second — and more substantial part — consisted of an attempt at locating the points where the customary reasoning was led astray. Among the critical points, ABEL repeated and radicalized CAUCHY'S dogma that divergent series have no sum and should not be treated in analysis. Beside his ban on divergent series, ABEL also repeated CAUCHY'S concern for numerical equality and stressed that even if two expressions were numerically equal in the interior of an interval they needed not coincide in the endpoints. This led him to explicitly question specific practices such as the passing to the limit in power series and the term-wise differentiation (and integration).

The third component of ABEL'S critical position was more constructive. In his letters and publications, he suggested that most of the unfortunate paradoxes and malpractices arose out of considering series which were not power series. This led him to focus attention on power series which he saw as some sort of safe haven where the commonly used methods would still apply.

The structure of ABEL'S proof of the binomial theorem. The path which ABEL took in his publication on the binomial theorem makes a lot of sense when seen from a perspective integrating *Poisson's example* and the components of ABEL'S criticism listed above. In the binomial paper, ABEL separated two distinct problems concerning the binomial theorem. First, he wanted to find the precise set of assumptions on m and x for which the binomial series

$$\sum_{n=0}^{\infty} \binom{m}{n} x^n$$

converged. In order to do so, he developed and revised some important theorems in the theory of series. And second, he wanted to investigate whether — in the cases where the series converged — the sum of the series agreed with (one of the values of) the binomial

$$(1+x)^m .$$

This approach was adapted to overcome the problems of multi-valued functions which lie at the core of the *Poisson's example*.

21 "*Divergente Rækker ere i det Hele noget Fandenskab, og det er en Skam at man vover at grunde nogen Demonstration derpaa.*" (Abel→Holmboe, 1826/01/16. ibid., 16).

12.2 Infinitesimals

ABEL'S reading and rendering of CAUCHY'S fundamental concepts came to influence the development of analysis in the nineteenth century. At certain points, his interpretations were clearer and more specific and some of them would eventually coincide with the *standardized* interpretations laid down by men such as G. P. L. DIRICHLET (1805–1859) and WEIERSTRASS.

One of the basic notions which ABEL introduced in his binomial paper was that of *infinitesimals*. In a footnote, ABEL explained,

> "For brevity, in this paper ω denotes a quantity which can be less than any given arbitrarily small quantity."[22]

Despite the awe which ABEL felt for CAUCHY'S work, this definition is not truly in accord with CAUCHY'S notion of infinitesimals. As discussed above, CAUCHY had interpreted infinitesimals as *variables* with limit zero but in ABEL'S definition, the infinitesimals seem to reenter as completed quantities less than any finite quantity but different from zero. The limit process has seemingly faded into the background. To illustrate this way of designating infinitesimals by symbols, we may reconsider CAUCHY'S proof of the *Cauchy Theorem* (see page 217) interpreted in ABEL'S notation. ABEL did not undertake this proof, but the arguments are directly to those which he employed in proving the *Lehrsatz IV* (see below). By continuity of the finite polynomial s_n,

$$s_n(x+\alpha) - s_n(x) = \omega,$$

and by the convergence of s,

$$\begin{aligned} r_n(x+\alpha) &= \omega, \\ r_n(x) &= \omega. \end{aligned} \tag{12.5}$$

Therefore

$$s(x+\alpha) - s(x) = \omega$$

and the continuity has been "proved". This way of designating infinitesimals by the same symbols regardless of the way in which they depend on other variables and infinitesimals hid and obscured the basic problems of the above argument. In the argument, the n which appears in (12.5) must depend on α and ω and can be unbounded as α and ω vanish. However, it took quite some time and a detailed analysis of these dependencies to clear out the proof (see section 14.1.2, below).

22 "*Die Kürze wegen soll in dieser Abhandlung unter ω eine Größe verstanden werden, die kleiner sein kann, als jede gegebene, noch so kleine Größe.*" (N. H. Abel, 1826f, 313, footnote).

12.3 Convergence

While ABEL'S definition and use of infinitesimals were not completely in the line of CAUCHY'S new rigor, his concept of convergence — and the importance which he attributed to it — closely resembled CAUCHY'S.

> "*Definition*. An arbitrary
>
> $$v_0 + v_1 + v_2 + \cdots + v_m \text{ etc.}$$
>
> will be called convergent if the sum $v_0 + v_1 + \cdots + v_m$ steadily approaches a certain limit for ever increasing values of m. This limit will be called the *sum of the series*. In the contrary case, the series is called divergent and therefore has no sum. From this definition follows that for a series to converge, it will be necessary and sufficient that the sum $v_m + v_{m+1} + \cdots + v_{m+n}$ steadily approach zero for ever increasing values of m whatever value n may have."[23]

Just as CAUCHY had done, ABEL quickly related the convergence of a series to the *Cauchy criterion* and claimed that it constituted a necessary and sufficient condition for convergence. As described above, the assertion that convergence followed from the *Cauchy criterion* was later realized to be non-trivial, but in the 1820s it was considered obvious. Although both CAUCHY and ABEL drew the connection between convergence and the *Cauchy criterion*, ABEL gave the criterion a much more central position in his theory of series as will be described below (see page 231).

Immediately following his definition of convergence, ABEL made the rather curious remark that "in every arbitrary series, the general term v_m will approach zero."[24] Judging from the context, an omission of the word "convergent" must have crept in at this point.[25]

An extended ratio test: *Lehrsätze I&II*. The first theorem of ABEL'S binomial paper is most remarkable because of its conceptual contents. Without proof (see a modernized proof in box 2), ABEL observed that for any series of positive terms

$$\sum_{m=0}^{\infty} \rho_m$$

---

23 "Erklärung. *Eine beliebige Reihe*

$$v_0 + v_1 + v_2 + \cdots + v_m \textit{ u.s.w.}$$

soll convergent heißen, wenn, für stets wachsende Werthe von m, die Summe $v_0 + v_1 + \cdots + v_m$ sich immerfort eine gewisse Gränze nähert. Diese Grenze soll Summe der Reihe *heißen. Im entgegengesetzten Falle soll die Reihe divergent heißen, und hat alsdann keine Summe. Aus dieser Erklärung folgt, daß, wenn eine Reihe convergiren soll, es nothwendig und hinreichend sein wird, daß, für stets wachsende Werthe von m, die Summe $v_m + v_{m+1} + \cdots + v_{m+n}$ sich Null immerfort nähert, welchen Werth auch n haben mag.*" (ibid., 313).

24 "*In irgend einer beliebigen Reihe wird also das allgemeine Glied v_m sich Null stets nähern.*" (ibid., 313).

25 This omission has therefore also been silently corrected in the French translation found in (N. H. Abel, 1839; N. H. Abel, 1881).

Proof of *Lehrsatz I* In order to present a proof of *Lehrsatz I*, we write $\varepsilon = \frac{\alpha-1}{2} > 0$ and chose $n \in \mathbb{N}$ such that

$$\rho_{m+1} \geq (\alpha - \varepsilon)\, \rho_m \text{ for all } m \geq n.$$

Then, by iteration,

$$\rho_{m+1} \geq (\alpha - \varepsilon)^{m-n}\, \rho_n.$$

The choice of ε ensures that ρ_{m+1} increases beyond all bounds. Now assume that the series in (12.6), $\sum \varepsilon_m \rho_m$, is to be convergent. Then, in particular, a $k \in \mathbb{N}$ exists such that

$$|\varepsilon_{m+1}\rho_{m+1}| < \varepsilon \text{ for all } m \geq k.$$

If $m \geq \max(k, n)$,

$$\varepsilon > |\varepsilon_{m+1}| \cdot \rho_{m+1} \geq |\varepsilon_{m+1}|\, (\alpha - \varepsilon)^{m-n}\, \rho_n$$

meaning

$$|\varepsilon_{m+1}| \leq \frac{\varepsilon}{\rho_n} \frac{1}{(\alpha - \varepsilon)^{m-n}} \to 0 \text{ for } m \to \infty$$

because $\alpha - \varepsilon > 1$. In conclusion, if $\sum \varepsilon_m \rho_m$ is be convergent, the sequence $\{\varepsilon_m\}$ has to converge toward zero. And since this is not the case, the sum (12.6) cannot be convergent. □

Box 2: Proof of *Lehrsatz I*

for which the ratio of consecutive terms converges toward $\alpha > 1$,

$$\frac{\rho_{m+1}}{\rho_m} \to \alpha > 1,$$

any linear combination

$$\sum_{m=0}^{\infty} \varepsilon_m \rho_m \tag{12.6}$$

will be divergent, provided the sequence $\{\varepsilon_m\}$ does not converge to zero.

The contents of this *Lehrsatz I* is thus a generalization of one part of CAUCHY'S ratio test of convergence. It is remarkable from a conceptual viewpoint that ABEL'S first theorem would be one of *divergence* when his entire theory was so focused on convergent series. The *Lehrsatz I* is thus — as it stands — a negative demarcation criterion.

This apparent imbalance was leveled by the second theorem. ABEL'S *Lehrsatz II* is a counterpart to the *Lehrsatz I* describing analogous — but this time sufficient — conditions for convergence. ABEL found that if

$$\frac{\rho_{m+1}}{\rho_m} \to \alpha < 1$$

and $\{\varepsilon_m\}$ was a sequence of terms which do not exceed 1 (ABEL was not explicit about requiring ε_m to be positive; if this assumption is not made, the requirement would be $|\varepsilon_m| < 1$), the series

$$\sum_{m=0}^{\infty} \varepsilon_m \rho_m$$

was necessarily convergent. Throughout, ABEL'S use of numerical values was sporadic. At times, he noticed that numerical values had to be taken, at other times such a remark might be inferred from his language, and at yet other times — as will be shown with respect to *Lehrsatz VI* — it seems to have evaded his attention. For instance, in the example given above, ABEL actually required that ε_m did not surpass unity which could also mean $-1 < \varepsilon_m < 1$.

ABEL'S proof of *Lehrsatz II* had similarities with the modernized proof of *Lehrsatz I* given in box 2 although he chose to use the characterization of convergence by the *Cauchy criterion* given above. He argued that for m sufficiently large,

$$\rho_{m+k} < \alpha^k \rho_m \tag{12.7}$$

and consequently

$$\sum_{k=m}^{m+n} \rho_k < \rho_m \sum_{k=0}^{n} \alpha^k = \rho_m \frac{1-\alpha^{n+1}}{1-\alpha} < \frac{\rho_m}{1-\alpha}.$$

Since $\varepsilon_m < 1$, a similar conclusion would hold for the series $\varepsilon_m \rho_m$,

$$\sum_{k=m}^{m+n} \varepsilon_k \rho_k < \frac{\rho_m}{1-\alpha}.$$

ABEL concluded the argument by observing that $\rho_m \to 0$ followed from (12.7), and thus the convergence of the series was secured by the characterization.

Although ABEL'S way to the two theorems might seem obvious to a modern reader, it is interesting to compare ABEL'S theorems including their proofs with CAUCHY'S original deduction of the ratio test as given in the *Cours d'analyse* (see page 212, above). Such a comparison reveals that CAUCHY and ABEL devised different structural systems for their theories of infinite series. Although the results were the same, the theorems and proofs played slightly different roles in the two systems. In CAUCHY'S theory, the characterization of convergent series by means of the *Cauchy criterion* was noticed but was of little use in obtaining the other — more important — tests of convergence. Instead, these tests were derived from the convergence of geometric progressions which was proved directly. In ABEL'S theory, however, the *Cauchy criterion* was made into a central tool which he used to deduce his slightly generalized version of the ratio test (see figure 12.1). This made the proof of the ratio test much simpler in ABEL'S framework than it had been in CAUCHY'S *Cours d'analyse*. In the manuscript *Sur les séries*, ABEL placed the *Cauchy criterion* in an equally central position.

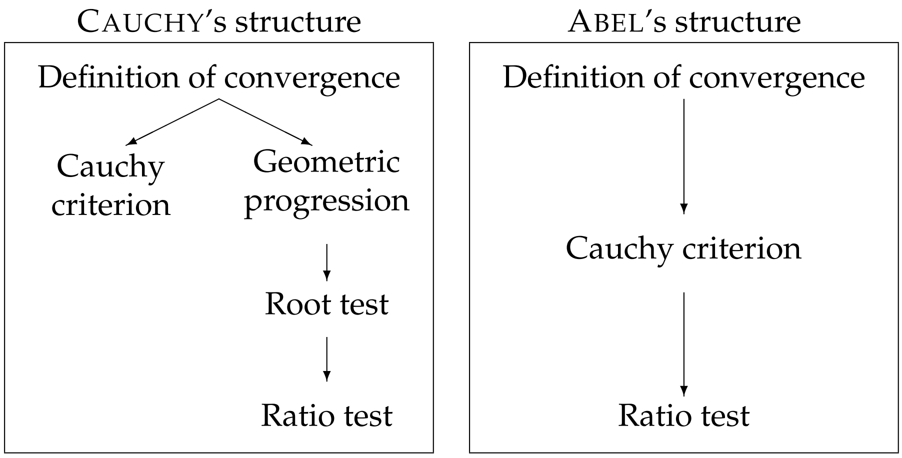

Figure 12.1: Comparison of CAUCHY's and ABEL's structures of the basic theory of infinite series.

An auxiliary theorem: *Lehrsatz III.* As his third theorem,[26] ABEL presented an auxiliary result which — although not difficult — was put to great use in the proofs to follow. He demonstrated that if $\{t_n\}$ denoted a sequence whose partial sums were bounded,

$$\sum_{k=0}^{m} t_k < \delta \text{ for all } m \in \mathbb{N},$$

and $\{\varepsilon_n\}$ denoted a *decreasing* sequence of positve terms, then

$$r_m = \sum_{k=0}^{m} \varepsilon_k t_k < \delta\varepsilon_0 \text{ for all } m \in \mathbb{N}.$$

ABEL'S proof consisted of a rather simple manipulation, in which he observed that with

$$p_m = \sum_{k=0}^{m} t_k,$$

each term could be written as

$$t_k = p_k - p_{k-1},$$

and, thus,

$$\begin{aligned} r_m &= \sum_{k=0}^{m} \varepsilon_k t_k = \sum_{k=0}^{m} \varepsilon_k \left(p_k - p_{k-1}\right) = \sum_{k=0}^{m} \varepsilon_k p_k - \sum_{k=0}^{m-1} \varepsilon_{k+1} p_k \\ &= \sum_{k=0}^{m-1} p_k \left(\varepsilon_k - \varepsilon_{k+1}\right) + \varepsilon_m p_m. \end{aligned}$$

Since $\{\varepsilon_n\}$ was decreasing,

$$0 < \varepsilon_k - \varepsilon_{k+1} < \varepsilon_k < \varepsilon_0,$$

26 (N. H. Abel, 1826f, 314)

it followed that

$$r_m < \sum_{k=0}^{m-1} \delta\left(\varepsilon_k - \varepsilon_{k+1}\right) + \varepsilon_m\delta = \delta\varepsilon_0,$$

and the inequality had been obtained.

12.4 Continuity

Just as had been the case with CAUCHY'S proof of the binomial theorem, the concept of *continuity* played an important role in ABEL'S proof. In his paper on the binomial theorem, ABEL gave rudiments of a different rendering of the theory of interaction between the concepts of continuity and convergence. ABEL'S definition of continuity seems to closely resemble CAUCHY'S (see page 216), although it may be noticed that ABEL'S definition is *only* formulated in the terminology of limits.

> "*Definition.* A function $f(x)$ shall be called a *continuous function* of x between the boundaries $x = 0$ and $x = b$ when for any arbitrary value of x between these limits, the quantity $f(x-\beta)$ for ever decreasing values of β approach the limit $f(x)$."[27]

Although their definitions of continuity are almost identical, ABEL and CAUCHY attributed slightly different meaning to their concepts when they were employed. The apparent ambiguity concerning the order in which quantification is to be made in CAUCHY'S definition was resolved in ABEL'S persistent insistence on *point-wise* definitions. ABEL'S definition as stated seems just as susceptible to the ambiguity as CAUCHY'S, but ABEL throughout interpreted it to mean that a function is continuous *at a point* $x \in [0, b]$ if $f(x-\beta) \to f(x)$ as $\beta \to 0$.

Combining continuity, convergence, and power series. The fourth and fifth theorems of ABEL'S binomial paper provided two important combinations of the concepts of continuity and convergence. The fourth theorem, *Lehrsatz IV*, stated and proved the continuity of a power series in the interior of its interval of convergence, while *Lehrsatz V* attempted to provide a rigorous replacement for what *Cauchy's Theorem* (see page 217) had promised but not rigorously delivered. At this point, *Lehrsatz IV* together with its proof will be described first, and *Lehrsatz V* will be postponed to be discussed in its proper context of ABEL'S famous *Ausnahme* or counter example to CAUCHY'S theorem (see section 12.6). At that point, the strong internal relations between the two theorems will also be described and explained.

As his fourth theorem, ABEL stated and proved the following result which has become a classic of the theory of series and is often associated with ABEL'S name.

---

27 "Erklärung. *Eine Function* $f(x)$ *soll* stetige Function *von* x*, zwischen den Grenzen* $x = 0$, $x = b$ *heißen, wenn für einen beliebigen Werth von* x*, zwischen diesen Grenzen, die Größe* $f(x-\beta)$ *sich für stets abnehmende Werthe von* β*, der Grenze* $f(x)$ *nähert.*" (ibid., 314).

> "*Lehrsatz IV*. When the series
>
> $$f(\alpha) = v_0 + v_1\alpha + v_2\alpha^2 + \cdots + v_m\alpha^m + \ldots$$
>
> converges for a certain value δ of α, it will also converge for every *smaller* value of α. Furthermore it will be of the sort that $f(\alpha - \beta)$ approaches the limit $f(\alpha)$ for ever decreasing values of β provided α is less than or equal to δ."[28]

In most modern presentations, the variable α is interpreted as a complex variable, and the theorem states the continuity of a power series in the interior of its disc of convergence. However, in this part of the paper, ABEL was exclusively interested in power series with real terms.

In order to facilitate comparison with ABEL'S proof of the fifth theorem of the paper and in order to exemplify ABEL'S use of infinitesimals in his arguments, a presentation of ABEL'S proof is worth giving. It should be remarked even before embarking on a tour of ABEL'S proof, that at a number of points its argument diverges from the modernized version of the proof; these occasions will be noticed below and elaborated in the following section.

ABEL began his proof by splitting the power series after m terms,

$$\phi(\alpha) = \sum_{n=0}^{m-1} v_n\alpha^n \text{ and } \psi(\alpha) = \sum_{n=m}^{\infty} v_n\alpha^n.$$

Then, he rewrote the tail of the series as

$$\psi(\alpha) = \sum_{n=m}^{\infty} \left(\frac{\alpha}{\delta}\right)^n v_n\delta^n$$

and obtained from *Lehrsatz III* the inequality

$$\psi(\alpha) < \left(\frac{\alpha}{\delta}\right)^m \cdot p \tag{12.8}$$

"where p denotes the largest number among the quantities $v_m\delta^m$, $v_m\delta^m + v_{m+1}\delta^{m+1}$, $v_m\delta^m + v_{m+1}\delta^{m+1} + v_{m+2}\delta^{m+2}$ etc."[29] This definition of p might seem strange or dangerous to the modern reader. When translated into modern notation, ABEL'S p corresponds to the following supremum

$$p = \sup_k \sum_{n=m}^{m+k} v_n\delta^n. \tag{12.9}$$

---

28 "Lehrsatz IV. *Wenn die Reihe*

$$f(\alpha) = v_0 + v_1\alpha + v_2\alpha^2 + \cdots + v_m\alpha^m + \ldots$$

für einen gewissen Werth δ von α convergirt, so wird sie auch für jeden kleineren *Werth von α convergiren, und von der Art seyn, daß $f(\alpha - \beta)$, für stets abnehmende Werthe von β, sich der Grenze $f(\alpha)$ nähert, vorausgesetzt, daß α gleich oder kleiner ist als δ.*" (N. H. Abel, 1826f, 314).

29 "*wenn p die größte der Größen $v_m\delta^m$, $v_m\delta^m + v_{m+1}\delta^{m+1}$, $v_m\delta^m + v_{m+1}\delta^{m+1} + v_{m+2}\delta^{m+2}$ u.s.w. bezeichnet.*" (ibid., 315).

Actually, if taken literally, ABEL'S p would be the *maximum* of the sums in (12.9) — not the supremum — but this distinction is beyond the point because for ABEL, the entire discussion on the definition of p was a non-issue. The quantity p was simply (and un-problematically) defined to be the greatest one among a sequence of numbers. The nature of the number p will be pursued in section 12.6, where *Lehrsatz V* will shed even more light on this *non-issue*. In the present case, the sequence of numbers is bounded since the series is assumed to converge at δ, but this was not explicitly remarked by ABEL.

In order to follow ideas of ABEL'S proof, it suffices to take either ABEL'S naïve definition of p or the modernized one expressed in (12.9). From the equality (12.8), ABEL then concluded, that

> "for any value of α which is less than or equal to δ, m can be taken sufficiently large that
>
> $$\psi(\alpha) = \omega."^{30}$$

Next, ABEL observed that $\phi(\alpha)$ was an entire function of α, i.e. a polynomial, and thus β could be taken small enough that

$$\phi(\alpha) - \phi(\alpha - \beta) = \omega.$$

By combining these two results, ABEL concluded

$$f(\alpha) - f(\alpha - \beta) = \omega.$$

Here we encounter ABEL'S way of operating with infinitesimals. The internal dependencies among ω, m, and α have been completely obscured by the notation and the argumentative style.

A simple observation, inspired by comparing ABEL'S proof with modern expositions of the calculus, concerns the use of infinitesimals. Today, infinitesimals have been completely abandoned from "rigorous" presentations of the calculus, and to a person trained within this program, ABEL'S usage of infinitesimals and even CAUCHY'S dual definitions involving both limits and infinitesimals can be repulsive. But to ABEL they were legitimate means of proving theorems.

ABEL'S *Lehrsatz IV* and the paradoxes of analysis. In the binomial paper, the fourth theorem is given without further comments, but in his letters, ABEL had related it to one of the strongest ongoing discussions among analysts. As already described in

30 "*Mithin kann man für jeden Werth von α, der gleich oder kleiner ist, als δ, m groß genug annehmen, daß*

$$\psi(\alpha) = \omega$$

ist." (ibid., 315).

section 12.1, ABEL observed that a common practice for evaluating infinite sums, say $\sum a_n$ had been to transform the series into a power series $\sum a_n x^n$, obtain an expression for the sum and then insert $x = 1$. Commenting on this practice, ABEL wrote,

> "This is probably right, but it appears to me that one cannot assume it without proof; just because
>
> $$\phi(x) = a_0 + a_1 x + a_2 x^2 + \dots$$
>
> for all values of x less than 1, it is not thereby said that the same conclusion holds for $x = 1$."[31]

In order to illustrate his point of criticism, ABEL remarked that his claim was certainly to the point if the power series failed to converge for $x = 1$ in which case it had no sum. However, when he gave explicit examples, he took them from the emerging theory of trigonometric series and not from within the realm of power series. And there is very good reason why he did not give a power series as a counter example; his fourth theorem states that for power series, the procedure of passing to the limit (inserting $x = 1$) can only fail in case the series is divergent for $x = 1$. Thus, *Lehrsatz IV* is *the* assurance needed to justify this procedure for the class of power series provided the resulting series is assumed to converge.

DIRICHLET'S modification of ABEL'S proof. In 1862, J. LIOUVILLE (1809–1882) reported having discussed ABEL'S fourth theorem with his friend DIRICHLET, who had died just a few years before. LIOUVILLE had expressed his concern about the original proof of ABEL'S very important theorem which he found difficult to present in courses and even to understand. On the spot, DIRICHLET gave an alternative proof of *Lehrsatz IV*, which LIOUVILLE felt would remove all such difficulties. It was this new proof by DIRICHLET which LIOUVILLE reproduced in verbatim in a short note in his *Journal de mathématiques pures et appliquées*.[32] Similarly, a page in G. F. B. RIEMANN'S (1826–1866) *Nachlass* contains his reworking of ABEL'S proof which also supports the impression that ABEL'S original proof was not universally accepted.[33]

Before the differences between the ABEL'S and DIRICHLET'S proofs are discussed and analyzed, a presentation of DIRICHLET'S new proof is required. A modern reconstruction of DIRICHLET'S proof is given in box 3.

For the infinite series

$$A = \sum_{m=0}^{\infty} a_m,$$

---

31 *"Dette er vel rigtigt; men mig synes at man ikke kan antage det uden Beviis, thi fordi man beviser at*

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

for alle Værdier af x som er mindre end 1, saa er det ikke derfor sagt at det samme finder Sted for $x = 1$." (Abel→Holmboe, 1826/01/16. N. H. Abel, 1902a, 17).

32 (G. L. Dirichlet, 1862). The proof is also described in (I. Grattan-Guinness, 1970b, 108).

33 (Laugwitz, 1999, 207).

DIRICHLET introduced the partial sums

$$s_n = \sum_{m=0}^{n} a_m.$$

He assumed that the numerical values of the partial sums were always bounded by a constant k and that they converge toward the limit A as n grows beyond all bounds. Then DIRICHLET introduced the associated power series

$$S = \sum_{m=0}^{\infty} a_m \rho^m,$$

where $0 < \rho < 1$ and thus wanted to prove that $S(\rho) \to A$ when $\rho \to 1$. He observed that since $a_m = s_{m+1} - s_m$, it could be rewritten as

$$S = s_0 + \sum_{m=1}^{\infty} (s_m - s_{m-1}) \rho^m. \tag{12.10}$$

DIRICHLET subsequently transformed this expression for the power series (12.10) into

$$S = (1-\rho) \sum_{m=0}^{\infty} s_m \rho^m \tag{12.11}$$

by the *finite* argument, i.e. by considering only the first $n+1$ terms of (12.10)

$$S_{n+1} = s_0 + \sum_{m=1}^{n} (s_m - s_{m-1}) \rho^m = (1-\rho) \sum_{m=0}^{n-1} s_m \rho^m + s_n \rho^n$$

and the observation that $s_n \rho^n$ "vanishes for $n = \infty$", i.e. $s_n \rho^n \to 0$ as $n \to \infty$. Since the two expressions correspond for any finite n, their limits also correspond,

$$S = \lim_{n\to\infty} S_{n+1} = (1-\rho) \lim_{n\to\infty} s_n = (1-\rho) \sum_{m=0}^{\infty} s_m \rho^m.$$

Now, DIRICHLET wanted to prove that the expression (12.11) converged to A as $\varepsilon = 1 - \rho$ converged to zero. To do so, he split the series (12.11) into two parts

$$S = (1-\rho) \sum_{m=0}^{n-1} s_m \rho^m + (1-\rho) \sum_{m=n}^{\infty} s_m \rho^m, \tag{12.12}$$

and observed that the first sum was bounded by $\varepsilon n k$, since $|s_m \rho^m| < |s_m| < k$. Thus, he claimed that it converged to zero with $\varepsilon \to 0$, which is true, provided n is kept fixed. As for the second sum, DIRICHLET claimed it could be written as

$$P(1-\rho) \sum_{m=n}^{\infty} \rho^m = P\rho^n = P(1-\varepsilon)^n, \tag{12.13}$$

provided P be chosen as a number between the smallest and the largest among the quantities $s_n, s_{n+1}, \ldots$. There is no explicit explanation for this claim, but it could be obtained in a number of ways, either from CAUCHY'S theory of means or as an easy consequence of the intermediate value theorem of the integral calculus. Because the partial sums $s_n, s_{n+1}, \ldots$ all converge to A, DIRICHLET had proved his claim that S converges to A when ρ converges to 1.

Comparison of ABEL'S and DIRICHLET'S proofs. It is interesting to note how the attitude toward infinitesimals and limit arguments evolved over the first 40 years after CAUCHY'S *Cours d'analyse* and we can get an indication of this by comparing ABEL'S and DIRICHLET'S proofs. Contrary to ABEL'S proof, DIRICHLET completely avoided the use of infinitesimals in his proof. Instead, he argued completely within the process based interpretation of limits when he reduced the series to finitely many terms, manipulated the polynomials and applied the limit process $n \to \infty$. However, DIRICHLET'S notation still hid the order in which limit processes are to be sequenced.

In the ultimate step of DIRICHLET'S proof, two limit processes were involved; both expressions (12.12) and (12.13) involved both ε and n which were intended to converge toward zero and infinity, respectively. A modern reconstruction of the limit processes of DIRICHLET'S argument could proceed along the lines suggested by the proof in box 3. In the box, it is illustrated how the limit processes can be straightened by *first* fixing a value of n such that $|s_m - A|$ is sufficiently small for all $m \geq n$ and *then* specifying the ε that will make the power series differ from A by as little as had been required. The order of DIRICHLET'S proof does not reflect the order in which the limit processes are to be carried out, and neither does his notation. Therefore, we are still faced with a line of argument in which limit processes are not as clearly identified and sequentially ordered as it is required today.

12.5 ABEL's "exception"

The "exception" in the binomial paper. In proofs of the binomial theorem which follow EULER'S method of extending the binomial formula through the use of the functional equation, some argument based on continuity has to be applied to get from rational to real exponents. To meet this demand in his proof, CAUCHY deduced and stated the so-called *Cauchy's Theorem* (see section 11.5). At the corresponding point of his binomial paper, ABEL discarded CAUCHY'S version of the theorem because he had discovered that it "suffered exceptions":[34]

> "Remark. In the above-mentioned work of Mr. *Cauchy* (on page 131) the following theorem can be found:
>
> »Whenever the different terms of the series
>
> $$u_0 + u_1 + u_2 + u_3 + \dots \text{ etc.}$$
>
> are functions of one and the same variable quantity and moreover continuous functions with regard to this variable in the vicinity of a particular value for which the series is convergent, then the sum s of the series will also be a continuous function of x in the vicinity of that particular value.«

34 For CAUCHY'S original formulation, which is authentically translated in ABEL'S paper, see page 217.

A modern reconstruction of DIRICHLET's proof. Let $\delta > 0$ be given and choose (by convergence) n such that

$$|s_m - A| < \frac{\delta}{6} \text{ for all } m \geq n.$$

Then chose $\varepsilon_1 > 0$ and $\varepsilon_2 > 0$ such that

$$\begin{aligned} n\varepsilon k &< \frac{\delta}{3} \text{ for } \varepsilon < \varepsilon_1, \text{ and} \\ |\rho^n - 1| &< \frac{\delta}{3k} \text{ for all } \varepsilon = 1 - \rho < \varepsilon_2. \end{aligned}$$

Then, if $\varepsilon < \min\{\varepsilon_1, \varepsilon_2\}$, the equation (12.12) becomes

$$|S - A| \leq (1-\rho) \sum_{m=0}^{n-1} |s_m| \rho^m + \left| (1-\rho) \sum_{m=n}^{\infty} s_m \rho^m - A \right|.$$

In the first sum, the interesting reconstructed inequality is

$$(1-\rho) \sum_{m=0}^{n-1} |s_m| \rho^m \leq \varepsilon \sum_{m=0}^{n-1} |s_m| \leq \varepsilon n k < \frac{\delta}{3}.$$

When the second sum is rewritten as

$$(1-\rho) \sum_{m=n}^{\infty} s_m \rho^m - A = P(1-\rho) \sum_{m=n}^{\infty} \rho^m - A = P\rho^n - A$$

where $P \in \left[\inf_{m \geq n} s_m, \sup_{m \geq n} s_m\right]$, the inequalities of interest obtained from the requirements are

$$\begin{aligned} |P\rho^n - A| &\leq |P\rho^n - P| + |P - A| \\ &\leq k \times \frac{\delta}{3k} + 2 \times \frac{\delta}{6} = \frac{2\delta}{3}. \end{aligned}$$

Combining the inequalities show that for $\varepsilon < \min\{\varepsilon_1, \varepsilon_2\}$,

$$|S - A| \leq \frac{\delta}{3} + \frac{2\delta}{3} = \delta.$$

Thus, as this modern reconstruction illustrates, it can be proved along the lines of DIRICHLET'S proof that for $\varepsilon \to 0$ (i.e. $\rho = 1 - \varepsilon \to 1$), the power series $S(\rho)$ converges to A. The interrelation among the limit processes is contained in the specification of ε_1 and ε_2. □

Box 3: A modern reconstruction of DIRICHLET's proof.

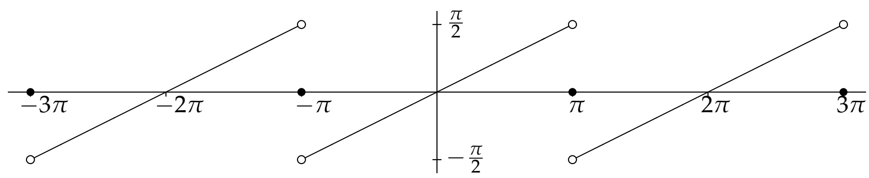

Figure 12.2: Graphical representation of ABEL's "Exception", $\sum_{n=1}^{\infty} \frac{(-1)^{n-1} \sin nx}{n}$.

> However, it appears to me that this theorem admits [or suffers] exceptions. For instance, the series
>
> $$\sin \phi - \frac{1}{2} \sin 2\phi + \frac{1}{3} \sin 3\phi - \ldots \text{ etc.}$$
>
> is discontinuous for every value $(2m+1)\pi$ of x where m is an integer. As is well known, a multitude of series with similar properties exist."[35]

The series

$$\sum_{n=1}^{\infty} \frac{(-1)^{n-1} \sin nx}{n} \tag{12.14}$$

is a particularly simple trigonometric series: it is the Fourier series expansion of the function $f(x) = \frac{x}{2}$ on the interval $]-\pi, \pi[$ (see figure 12.2). As such, it can be found in FOURIER'S works, for instance in the *Théorie analytique de la chaleur*, and even as a side result in one of EULER'S papers.[36] Possibly because EULER'S and FOURIER'S arguments for the convergence of the series (12.14) might have been wanting from the perspective of the new rigor, ABEL explicitly derived it as a result of some of the formulae proved in the binomial paper (see page 259, below).

Aspects of ABEL'S "exception". For subsequent reference, a few points concerning ABEL'S "exception" must be brought to attention. First, the exception was one of the

---

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

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

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

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

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." (N. H. Abel, 1826f, 316, footnote).

36 (Fourier, 1822, 182, 241) and (L. Euler, 1754, 584); see also (I. Grattan-Guinness, 1970b, 84–85).

"new series" which — according to ABEL'S opinion (see page 250, below) — had only recently entered analysis and brought so many paradoxes with it. Second, from a modern perspective, it is curious that ABEL called the series an "exception" and not a counter example or even a paradox. Although the binomial paper was translated from a French manuscript by CRELLE (see page 30), this choice of words appears not to have been merely accidental. Furthermore, it appears to have mattered to ABEL that the exception was not "singular" — if required, a multitude of similar exceptions could be devised. These points will enter into the argument in chapter 21. Finally, it should be observed that the "exception" was actually a recurring item in ABEL'S works on rigorization. Above (see page 225), it has been described how ABEL employed the same series to criticize the practice of differentiating a series by differentiating each term. Similarly, it appeared in one of ABEL'S drafts when he wanted to probe the limits of the theorem — *Lehrsatz V* — which was his tailored replacement for *Cauchy's Theorem*.[37]

12.6 A curious reaction: *Lehrsatz V*

ABEL'S fifth theorem: a revision of CAUCHY'S theorem. The fifth theorem of ABEL'S binomial paper plays a central role in a story to be told in a subsequent chapter. For the present, the theorem is mainly of interest because it, like *Lehrsatz IV*, provides an important combination of the three concepts currently under consideration: convergence, continuity, and power series. In his fifth theorem, ABEL found that the binomial series was a continuous function; a result which was inherently important in the approach to the binomial theorem chosen by CAUCHY and adapted by ABEL. Again, the statement of the theorem is worth quoting,[38]

> "*Lehrsatz V*. Let
>
> $$v_0 + v_1\delta + v_2\delta^2 + \ldots \text{ etc.},$$
>
> be a convergent series in which $v_0, v_1, v_2, \ldots$ are continuous functions of one and the same variable quantity x between the boundaries $x = a$ and $x = b$. Then the series
>
> $$f(x) = v_0 + v_1\alpha + v_2\alpha^2 + \ldots,$$
>
> where $\alpha < \delta$ is convergent and a continuous function of x between the same boundaries."[39]

37 (N. H. Abel, [1827] 1881, 202); see page 245, below.

38 The statement of the theorem as given in the paper is sloppy in a couple of respects. First, the β introduced at the end should obviously be a δ, and the convergence of the initial series

$$\sum_{n=0}^{\infty} v_n\delta^n$$

must obviously also be assumed. Both corrections have been made by the editors in ABEL'S collected works (N. H. Abel, 1881, I, 223–224).

ABEL'S proof of *Lehrsatz V*. ABEL'S proved the fifth theorem by an approach closely resembling his proof of the preceding theorem. He first did away with the first claim of convergence by referring to the fourth theorem. The fourth theorem also told him that the sum function was continuous with respect to α; and he then moved on to prove the continuity of the sum function considered to be a function of x. As was common practice, ABEL split the sum function into two

$$\phi(x) = \sum_{m=0}^{n-1} v_m(x)\,\alpha^m \text{ and } \psi(x) = \sum_{m=n}^{\infty} v_m(x)\,\alpha^m.$$

Just as he had done for the fourth theorem, he rewrote $\psi(x)$ as

$$\psi(x) = \sum_{m=n}^{\infty} \left(\frac{\alpha}{\delta}\right)^m v_m(x)\,\delta^m$$

and introduced a quantity $\theta(x)$ to denote "the greatest among the quantities $v_m\delta^m$, $v_m\delta^m + v_{m+1}\delta^{m+1}$, $v_m\delta^m + v_{m+1}\delta^{m+1} + v_{m+2}\delta^{m+2}$."[40] Thus, $\theta(x)$ in the fifth theorem took the place of the quantity p used in the proof of the fourth theorem (see also below). In his further argument, ABEL used *Lehrsatz III* to write

$$\psi(x) < \left(\frac{\alpha}{\delta}\right)^m \theta(x)$$

just as he had done previously and his proof of the continuity of $f(x)$ followed exactly the same arguments as had been used in the fourth theorem. Again, infinitesimals were used instead of explicit limit processes when ABEL claimed that m could be chosen such that $\psi(x) = \omega$, which allowed him to write

$$f(x) - f(x-\beta) = \phi(x) - \phi(x-\beta) + \omega.$$

Then it was a simple matter to observe that ϕ was a polynomial and therefore β could be chosen small enough that

$$\phi(x) - \phi(x-\beta) = \omega$$

and the theorem had been proved.

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39 "Lehrsatz V. *Es sei*

$$v_0 + v_1\delta + v_2\delta^2 + \dots \text{ u.s.w.,}$$

eine Reihe, in welcher v_0, v_1, v_2 continuierliche Functionen einer und derselben veränderlichen Größe x sind, zwischen den Grenzen $x = a$ und $x = b$, so ist die Reihe

$$f(x) = v_0 + v_1\alpha + v_2\alpha^2 + \dots,$$

wo $\alpha < \beta$ [$\alpha < \delta$], convergent und eine stetige Function von x, zwischen denselben Grenzen." (N. H. Abel, 1826f, 315).

40 "*wenn man durch $\theta(x)$ die größte unter den Größen $v_m\delta^m$, $v_m\delta^m + v_{m+1}\delta^{m+1}$, $v_m\delta^m + v_{m+1}\delta^{m+1} + v_{m+2}\delta^{m+2}$ u.s.w. bezeichnet.*" (ibid., 315).

An objection to *Lehrsatz V*: is θ uniformly bounded? The major problem with ABEL'S proof of the fifth theorem is closely tied to its connection with his proof of the preceding theorem. In his proof of *Lehrsatz IV*, ABEL had introduced the quantity p to denote the largest quantity among the partial tails of the series. In the proof of *Lehrsatz IV*, ABEL'S reasoning can be 'saved' by the observation that since the series $\sum v_m \delta^m$ is convergent, its tails are bounded. Therefore, an upper bound — if not an outright maximum — will exist which can be used for p. Such an argument is nowhere to be found in ABEL'S proof, and there are two points indicating that it was neither at his disposal nor of his concern. First, ABEL spoke of "the largest among" an infinite collection of quantities, i.e. of a maximum. If he had had anything but a naïve intuition about this step in his argument he might well have expressed himself differently using phrases analogous to "bounded by". Second, in the proof of the fifth theorem which is modelled precisely over the proof of the fourth theorem, this exact step in the argument falls apart.

When, in the proof of *Lehrsatz V*, ABEL introduced $\theta(x)$ analogous to the quantity p above

$$\theta(x) = \text{largest quantity among } \sum_{m=n}^{n+k} v_m(x)\,\delta^m \text{ for } k \geq 0, \tag{12.15}$$

it seems to be a point-wise definition of the *function* $\theta(x)$. ABEL clearly thought of $\theta(x)$ as a quantity which, given x represented the largest among an infinite collection of quantities each depending on x. When, in the proof, ABEL claimed that

$$\psi(x) = \psi(x-\beta) = \omega,$$

he implicitly used a supposed *property* of the function $\theta(x)$ — that the choice of n could be made uniformly throughout a small region surrounding x. However, in ABEL'S argument, there is no way of assuring that θ satisfies this requirement.

As P. L. M. SYLOW (1832–1918) has remarked,[41] ABEL'S argument is sound if one further restriction is imposed on the convergence. If a constant M exists which uniformly bounds the general term around x_0

$$|v_m(x)\,\delta^m| \leq M \text{ for all } m \text{ and for all } x \in \left[x_0 - x', x_0 + x''\right],$$

ABEL'S reasoning can be applied by observing that both

$$|\psi(x)| \text{ and } |\psi(x-\beta)| \text{ will be less than } M\frac{\left(\frac{\alpha}{\delta}\right)^m}{1-\frac{\alpha}{\delta}}.$$

However, as observed, this is a reconstruction and certainly not part of ABEL'S argument.

41 (N. H. Abel, 1881, II, 303).

Another proof of the *Lehrsatz V* from ABEL's notebook. In ABEL's notebooks, results similar to the *Lehrsatz V* can be found treated in the manuscript *Sur les séries* which was presumably written in 1827.[42] Thus, ABEL returned to the theorem after the binomial paper had been published and attacked it from a slightly different perspective. In his notes on the *Sur les séries*, M. S. LIE (1842–1899) interprets this fact as clear evidence that ABEL had become dissatisfied with the version printed in the *Journal*.[43] The manuscript was never completed for printing and its contents exhibit the characteristics of a draft. In particular, the precise assumptions and some of the notation are not made explicit and interpretation is slightly difficult. I interpret the relevant part of the manuscript as presenting *two* new deductions of the *Lehrsatz V*.

In his manuscript, ABEL dealt with a function $f(y)$ introduced as a power series in x with coefficients which vary continuously in y,

$$f(y) = \sum_{n=0}^{\infty} \phi_n(y)\, x^n \tag{12.16}$$

and assumed that the series was convergent for $x < \alpha$ and all values of y near β. ABEL's first "proof" of the continuity of f at $y = \beta$ consisted in interchanging the limit processes, and he wrote it as

$$\lim_{y=\beta-\omega} f(y) = \sum_{n=0}^{\infty} \left(\lim_{y=\beta-\omega} \phi_n(y) \right) x^n = \sum_{n=0}^{\infty} A_n x^n = R$$

with the convention $A_n = \lim_{y=\beta-\omega} \phi_n(y)$. The practice of interchanging limit processes had been among the points which attracted ABEL's criticism, in particular when it came to term-wise differentiation (see above). Accordingly, ABEL did not simply interchange the two processes but made the additional restriction that the series R had to be convergent.

However, as ABEL's own "exception" could have illustrated, even the convergence of the resulting series was sufficient to warrant the general interchange of limits. Thus, ABEL had to make some use of the particular form of the series (12.16). However, ABEL made no remarks on this argument and due to the style of the notebook, its role and status — was it an observation? a theorem? a hypothesis? — remains my suggestive interpretation.

ABEL's second "deduction" is much more interesting and is presented here based on ABEL's original argument and LIE's reconstruction of it.[44] In this deduction, ABEL studied the differences between the corresponding terms of the series for $f(\beta-\omega)$ and $f(\beta)$.

$$(\phi_n(\beta-\omega) - A_n)\, x^n.$$

42 (N. H. Abel, [1827] 1881, 201–202).
43 (N. H. Abel, 1881, II, 326).
44 (ibid., II, 326).

Assuming that x_1 was a value such that $x < x_1 < \alpha$, ABEL introduced a bound by assuming that the m'th term was the maximum of these differences,

$$\left(\phi_m\left(\beta-\omega\right)-A_m\right)x_1^m = \max_{n\geq 0}\left\{\left(\phi_n\left(\beta-\omega\right)-A_n\right)x_1^n\right\}. \tag{12.17}$$

This step resembles the introduction of the problematic quantity $\theta\left(x\right)$ in the binomial paper and the existence (i.e. finiteness) of such a maximum was apparently unproblematic to ABEL. Accordingly, LIE has suggested the same method of saving ABEL'S argument as SYLOW had done for the *Lehrsatz V*, i.e. by turning its existence into an explicit assumption (see above). ABEL concluded that

$$f\left(\beta-\omega\right)-R = \frac{\xi}{1-\frac{x}{x_1}}\left(\phi_m\left(\beta-\omega\right)-A_m\right)x_1^m$$

for some $\xi \in [-1,1]$. When he let ω vanish, ABEL observed that the term

$$\phi_m\left(\beta-\omega\right)-A_m$$

also vanished by the continuity of ϕ_m. Therefore, ABEL concluded, the function f was continuous.

As described, ABEL'S two notebook proofs of the *Lehrsatz V* are slightly different from the printed version. However, they share the same structure and many of the methods which they apply, in particular concerning the belief in the existence of uniform bounds (12.15 and 12.17). It is tempting to speculate with LIE that ABEL had realized that his original proof of *Lehrsatz V* was problematic — perhaps seizing on the same objection as SYLOW did and proposing the solution which amounts to uniform convergence (see above). However, despite the new proofs, ABEL'S treatment of *Lehrsatz V* continued to suffer from essentially the same problems and such an interpretation is not compelling. If ABEL had become uneasy about his proof, it was probably for another reason or perhaps he just wanted another proof of a well established result?

Probing the extent of *Lehrsatz V*. Following his new proof of *Lehrsatz V* in the notebook, ABEL observed that the theorem demonstrated the continuity of the function

$$f\left(y\right) = \sum_{n=1}^{\infty}\frac{x^n \sin ny}{n}$$

for all $x < 1$, although for $x = 1$, the function — which was the "exception" of his binomial paper — had certain discontinuities. Under similar assumptions, the series corresponding to $x = 1$ could also fail to be divergent, altogether, ABEL observed and exemplified. These remarks again illustrate ABEL'S repeated criticism of the unwarranted passage to the limit in series.

12.7 From power series to absolute convergence

As indicated in his letter to HANSTEEN and in the general approach of the binomial paper, ABEL put a lot of emphasis on power series in his attempt to rebuild the theory of series. In particular, ABEL'S replacement for the invalidated *Cauchy Theorem* was based on a particular kind of series which were power series in one variable with coefficients which were continuous functions of another variable. Well into the second half of the nineteenth century, this particular argument was found to be better recast within a concept of *absolute convergence* which had emerged over the century.

Emergence of a concept of absolute convergence. During the 19th century, numerical (absolute) values of real numbers and the moduli of complex numbers entered ever more explicitly in arguments of analysis. As described in the examples from CAUCHY'S *Cours d'analyse* and ABEL'S binomial paper, the only way mathematicians could describe numerical values in the first decades of the century was through verbal formulations. Generally, CAUCHY was quite careful about these in stating his theorems on series; but proper concern for numerical values was often lacking in ABEL'S formulations.[45] It appears that notation such as $|x|$ was only invented by WEIERSTRASS in unpublished papers of the 1840s and did not become customary until the 1870s.[46]

In the first decades of the 19th century, series of numerical values mostly entered the picture in connection with the multiplication theorem. In the *Cours d'analyse*, when CAUCHY generalized his multiplication theorem for series of positive terms to more arbitrary series, he based his argument on the assumption of convergence of the series of absolute terms. However, despite its use in proving theorems, CAUCHY'S implicit concept of absolute convergence still lacked most of its later structural position.

Immediately following the proof of the multiplication theorem, CAUCHY took an interesting step in investigating the consequences of relaxing the assumptions. He proved, based on squaring the series

$$\sum_{n=1}^{\infty} \frac{(-1)^{n-1}}{\sqrt{n}}, \tag{12.18}$$

that the assumption of *absolute* convergence was indeed necessary: Because the terms of the alternating series (12.18) are decreasing in absolute value, the series was convergent as CAUCHY had proved.[47] However, it was not absolutely convergent and when CAUCHY produced the square of the series, he obtained another divergent series.[48] Here, CAUCHY took a rather modern step of using a counter example to a fictitious

45 See e.g. the editors' remark in (Lakatos, 1976, 134).

46 (K. Weierstrass, 1876, 78) and e.g. (K. Weierstrass, [1841] 1894).

47 (A.-L. Cauchy, 1821a, 144).

48 (ibid., 149–150). The divergence of the series $\sum \frac{1}{\sqrt{n}}$ can be obtained by comparing with the harmonic series.

more general theorem in order to illustrate that the requirements of his own theorem were necessary. However, as F. MERTENS (1840–1927) was later to show,[49] if scrutinized more carefully, the example illustrated that the multiplication theorem could fail if *both* factors were non-absolutely convergent. This led P. L. WANTZEL (1814–1848) to prove a version of the multiplication theorem which only assumed *absolute* convergence of one of the factors (the other factor just being assumed convergent).

The real beginning of a concept of absolute convergence came with DIRICHLET'S paper on primes in arithmetic progression which was published in 1837.[50] In that paper, DIRICHLET introduced a separation of convergent series into two classes based on the convergence of the series which resulted when the terms were replaced by the absolute values: Either the series of absolute values remained bounded or it was unbounded. For series of the first class (absolutely convergent series), DIRICHLET stated that their convergence and sum remained unaffected if the order of terms was altered. In particular, in double (and multiple) sums, the order of summation wold not effect the result. Dirichlet observed that these properties — which were certainly nice and expected — could fail to hold for series of the second class, and he gave two examples of what could happen: a convergent series could either become divergent or alter its sum if its terms were rearranged.[51]

For his *Habilitation* in 1854, RIEMANN presented a paper on the representability of functions by trigonometric series.[52] The paper is a milestone in the theory of trigonometric series and the theory of integrals and also contains interesting remarks on the concept of absolute and non-absolute convergence. In the historical preface, RIEMANN outlined the previous developments in the field and claimed that DIRICHLET'S important 1829 paper on the convergence of trigonometric series was directly inspired by DIRICHLET'S discovery of the distinction between absolute and non-absolute convergence.[53] RIEMANN expressed his belief that the prevalence of power series in analysis was the reason why those concepts had not previously been separated.[54] There are no obvious traces of the alleged inspiration visible in DIRICHLET'S paper of 1829 but — as mentioned — the distinction became very explicit in a paper with a different topic in 1837.

RIEMANN advanced a step beyond DIRICHLET'S observation of the differences between absolutely and non-absolutely (conditionally) convergent series when he described a very simple method by which the partial sums of a conditionally convergent series could be made to approach any given value by proper rearrangement of the terms of the series. Central to RIEMANN'S argument was the realization that if a series $\sum a_n$ was conditionally convergent, the series of its positive and negative terms

49 (Mertens, 1875).
50 (G. L. Dirichlet, 1837), see also e.g. (I. Grattan-Guinness, 1970b, 94–95).
51 See also (ibid., 94–95).
52 (B. Riemann, 1854).
53 (ibid., 235) DIRICHLET'S paper is (G. L. Dirichlet, 1829).
54 (B. Riemann, 1854, 235). See below.

would both have to converge to infinity. This could be used to prescribe a procedure by which examples such as those given by DIRICHLET could be constructed. As D. LAUGWITZ (1932–2000) remarks,[55] the *Riemann arrangement theorem* is not a particularly deep mathematical result in its own right but exemplifies the new approach to the concepts of convergence which was developing in the mid-nineteenth century.

P. D. G. DU BOIS-REYMOND (1831–1889) on a generalization of *Lehrsatz V*. A concept of absolutely convergent series was thus being established in the nineteenth century and it was gradually emerging as a very central and useful tool in doing analysis. In 1871, DU BOIS-REYMOND published a short note which treated CAUCHY'S theorem on the continuity of an infinite sum of continuous functions which had also been the subject of ABEL'S *Lehrsatz V*.[56] DU BOIS-REYMOND was an active participant in the restructuring of analysis which — flooding from WEIERSTRASS' lectures in Berlin — took place in the last part of the nineteenth century. In the note which falls into this Weierstrassian tradition of rigorizing analysis, DU BOIS-REYMOND proved a theorem to the following effect.

Theorem 12 (DU BOIS-REYMOND) *If a series*

$$\sum_{n=1}^{\infty} w_n\left(x\right)\mu_n \tag{12.19}$$

is considered, for which the series

$$\sum_{n=1}^{\infty} \mu_n$$

converges absolutely *and for which the functions w_n are continuous functions on the interval $[a,b]$ and all the w_n as well as $\lim_{n\to\infty} w_n$ remain finite on that interval, then the series (12.19) is a continuous function of x on the interval $[a,b]$.* □

As well as providing a proof of this theorem, DU BOIS-REYMOND also gave examples in which the theorem would warrant continuity and examples in which no such conclusion could be drawn from the theorem. The theorem which DU BOIS-REYMOND presented was a *generalization* of ABEL'S fifth theorem; the latter could be deduced from the former (see box 4). It is interesting to compare the two theorems and the motivating problems which inspired them.

The focus on power series: Comparing theorems. ABEL'S fifth theorem sought to avoid the over-generality of *Cauchy's Theorem* by focusing on some particular form of convergence similar to the convergence of power series. Compared to *Cauchy's*

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55 (Laugwitz, 1999, 211).

56 (Bois-Reymond, 1871); DU BOIS-REYMOND referred to both CAUCHY and ABEL.

ABEL's *Lehrsatz V* derived from DU BOIS-REYMOND's theorem Actually, ABEL'S fifth theorem is a consequence of DU BOIS-REYMOND'S theorem.

Corollary 1 *Assume that*

$$\sum_{n=1}^{\infty} v_n(x)\,\delta^n \tag{12.20}$$

is convergent for some $\delta > 0$ and that the functions v_n are continuous functions of x on some interval I. Then, for any $0 < \alpha < \delta$, the function

$$f(x) = \sum_{n=1}^{\infty} v_n(x)\,\alpha^n \tag{12.21}$$

is a continuous function of x on the interval I. □

PROOF We wish to use DU BOIS-REYMOND'S theorem to prove the theorem stated above. For this, we write

$$f(x) = \sum_{n=1}^{\infty} v_n(x)\,\delta^n \left(\frac{\alpha}{\delta}\right)^n$$

and denote

$$w_n(x) = v_n(x)\,\delta^n \text{ and}$$
$$\mu_n = \left(\frac{\alpha}{\delta}\right)^n.$$

Now, we are ready to test the requirements of DU BOIS-REYMOND'S theorem. The first requirement, that $\sum \mu_n$ converges absolutely is obviously satisfied since $\alpha < \delta$. Secondly, the functions w_n are obviously finite and continuous since this was required of v_n. Lastly, we have to show that $\lim_{n\to\infty} w_n(x)$ is also finite. However, this is an easy consequence to draw from the convergence of (12.20) which ensures us that $\lim_{n\to\infty} w_n(x) = 0$ for all $x \in I$. Thus, the continuity of (12.21) follows from DU BOIS-REYMOND'S theorem. ■

Box 4: ABEL's *Lehrsatz V* derived from DU BOIS-REYMOND's theorem

Theorem, one further assumption was introduced in ABEL'S fifth theorem to the effect that the series

$$f\left(x,\delta\right)=\sum_{n=1}^{\infty}v_{n}\left(x\right)\delta^{n} \tag{12.22}$$

was convergent for some $\delta > 0$; and the conclusion of continuity only applied to $f\left(x,\alpha\right)$ where $0<\alpha<\delta$, i.e. on the interior of the interval of convergence. Apparently, this further requirement did away with ABEL'S own exception to *Cauchy's Theorem*; the series

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

could not easily be transformed into a power series with radius of convergence sharply greater than one. However, it was hardly this barring of known exceptions which prompted ABEL to formulate his fifth theorem in the way he did. Instead, two other factors are most likely to have contributed to the formulation of the theorem. First, ABEL'S fifth theorem was closely modelled over *Lehrsatz IV*, and their proofs were almost identical. At the crucial step of the proof, a generalization was made from the constant p to the function $\theta\left(x\right)$ which were both designed to serve as uniform bounds. Second, the focus on power series was introduced by ABEL as a heuristic which "saved" ordinary intuition and many previously established results.

In the letter to HANSTEEN quoted above, ABEL expressed his concern over a darkness which he saw persisting in the field of mathematical analysis. He even described some ideas concerning the reason for the relatively few paradoxes which these obscure and ill-founded procedures had created.

> "In my opinion, it [the reason for the few paradoxes] lies in the fact that the functions which analysis has dealt with have mostly been expressible by powers. As soon as others [functions] enter, which certainly does not happen often, it often does not go well and from false conclusions a string of connected false theorems flow."[57]

According to ABEL, it was the introduction of new kinds of series which had produced problems for theorems which implicitly relied on properties of power series although they were often expressed so as to apply to *all* series. As RIEMANN'S similar remarks seems to indicate, this was a generally held — and valid — belief in the nineteenth century.

Half a century after ABEL'S solution to the problem raised by his counter example to *Cauchy's Theorem*, DU BOIS-REYMOND devised another answer to the same problem. Set in a different time and inspired by the system of analysis which WEIERSTRASS

57 "*Efter mine Tanker ligger den deri at de Functioner som Analysen hidentil har beskjæftiget sig med mestendels lade sig udtrykke ved* Potenser. — *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.*" (Abel→Hansteen, Dresden, 1826/03/29. N. H. Abel, 1902a, 22–23).

taught in Berlin, DU BOIS-REYMOND'S solution differed from ABEL'S at a conceptual level. When compared with ABEL'S theorem and its proof, DU BOIS-REYMOND'S theorem differed in three respects. First, when he focused on the purpose which ABEL'S use of power series had served in the original proof, DU BOIS-REYMOND could relax the assumptions and only assume that the series $\sum \mu_n$ converged (absolutely). Second, during the semi-century, a more rigid concept of *absolute convergence* had emerged which made the use of numerical values in series explicit and consistent. In the process, *absolute convergence* had become a concept about which theorems could be proved. Finally, in DU BOIS-REYMOND'S proof, the uniformity requirement discussed above was explicitly taken into account by the assumption that $\lim_{n\to\infty} w_n(x)$ remain finite.

The example of DU BOIS-REYMOND'S revision of *Cauchy's Theorem* serves to illustrate how the concept of absolute convergence became a very central and powerful concept in the theory of series. Formulating theorems using absolute convergence often led to more *functional* assumptions which were directly usable in the proofs. In this example, this was contrasted with the older focus on *formal* assumptions which stressed the particular formal appearance of the objects — here the particular form of the series (12.22) — under consideration.

12.8 Product theorems of infinite series

Besides the theorems discussed above, another basic, important theorem on infinite series received renewed interest in ABEL'S paper on the binomial theorem; and again, this theorem goes back to CAUCHY'S *Cours d'analyse*.

In the binomial paper, ABEL'S sixth and final preliminary theorem dealt with the product of two infinite series. In its presentation, it reveals an intriguing transition in the understanding of the concept of absolute convergence and reads as follows,

> "*Lehrsatz VI.* When by ρ_0, ρ_1, ρ_2 etc., $\rho_0', \rho_1', \rho_2'$ etc one designates the numerical values of the respective terms of two convergent series
>
> $$v_0 + v_1 + v_2 + \ldots = p \text{ and}$$
> $$v_0' + v_1' + v_2' + \ldots = p',$$
>
> then the series
>
> $$\rho_0 + \rho_1 + \rho_2 + \ldots \text{ and}$$
> $$\rho_0' + \rho_1' + \rho_2' + \ldots$$
>
> are likewise convergent. Similarly, the series
>
> $$r_0 + r_1 + r_2 + \cdots + r_m$$
>
> whose general term is
>
> $$r_m = v_0 v_m' + v_1 v_{m-1}' + v_2 v_{m-2}' + \cdots + v_m v_0',$$

and whose sum is

$$(v_0 + v_1 + v_2 + \dots) \times (v'_0 + v'_1 + v'_2 + \dots)$$

will also be convergent."[58]

As it appears, the theorem consists of two halves each contributing a distinct conclusion:

1. Convergence of terms implies convergence of numerical terms.

2. Convergence of the *Cauchy product* toward the correct sum.

The first of these two conclusions is wrong, and it is difficult to explain the mishap in ABEL'S presentation. In the collected works, it has been corrected without comments by replacing "*so sind die Reihen [...] ebenfalls noch convergent*" with "*si les séries [...] sont de même convergente*".[59] In the proof, ABEL does not give arguments for the first part of the supposed theorem; instead it *is* used as an assumption in proving the *Cauchy product theorem*.

Thus, based on ABEL'S proof, SYLOW and LIE attributed the mishap to a slip of the pen or perhaps a slight incompetence on the part of the translator. This is probably the best interpretation available, but a little more may perhaps be inferred from the fact that such a misprint found its way into a professional journal — as did a number of others. First, this fact suggests that conceptual handling of series and series of numerical values was not very well established among the mathematical class to which CRELLE belonged. And secondly, we may also be tempted to infer something on the

58 "Lehrsatz VI. *Bezeichnet man durch* ρ_0, ρ_1, ρ_2 *u.s.w.,* $\rho'_0, \rho'_1, \rho'_2$ *u.s.w. die Zahlenwerthe der resp. Glieder zweier convergenten Reihen*

$$v_0 + v_1 + v_2 + \dots = p \textit{ und}$$
$$v'_0 + v'_1 + v'_2 + \dots = p',$$

so sind die Reihen

$$\rho_0 + \rho_1 + \rho_2 + \dots \textit{ und}$$
$$\rho'_0 + \rho'_1 + \rho'_2 + \dots$$

ebenfalls noch convergent, und auch die Reihe

$$r_0 + r_1 + r_2 + \cdots + r_m$$

deren allgemeines Glied

$$r_m = v_0 v'_m + v_1 v'_{m-1} + v_2 v'_{m-2} + \cdots + v_m v'_0,$$

und deren Summe

$$(v_0 + v_1 + v_2 + \dots) \times (v'_0 + v'_1 + v'_2 + \dots)$$

ist, wird convergent seyn." (N. H. Abel, 1826f, 316–317).

59 (N. H. Abel, 1881, 225).

standards of the newly established journal, which was hampered by some similar and less grave misprints in the first years, although its standards of technical printing were quite high.

ABEL'S proof of *Cauchy product theorem* followed a path similar to those taken by CAUCHY in his proofs (see section 11.5). ABEL let p_m and p'_m denote the partial sums of the factors p and p' and wrote

$$\sum_{k=0}^{2m} r_k = p_m p'_m + \underbrace{\sum_{k=0}^{m-1} p_k v'_{2m-k}}_{=t} + \underbrace{\sum_{k=0}^{m-1} v_{2m-k} p'_k}_{=t'} . \tag{12.23}$$

After introducing the notation

$$u = \sum_{k=0}^{\infty} \rho_k \text{ and } u' = \sum_{k=0}^{\infty} \rho'_k,$$

he found

$$t < u \sum_{k=0}^{m-1} \rho'_{2m-k} \text{ and } t' < u' \sum_{k=0}^{m-1} \rho_{2m-k}$$

"without reference to the sign", i.e. for the numerical values of t and t'. ABEL then employed the *Cauchy sequence characterization* of convergence (for its prominent position in the Abelian framework, see above) to ensure that since the series $\sum \rho_k$ and $\sum \rho'_k$ were convergent, the sums

$$\sum_{k=0}^{m-1} \rho'_{2m-k} \text{ and } \sum_{k=0}^{m-1} \rho_{2m-k}$$

would both tend to zero as m grew to infinity. Thus, ABEL claimed, by setting m equal to infinity, the equation (12.23) became

$$\sum_{k=0}^{\infty} r_k = \left(\sum_{k=0}^{\infty} v_k \right) \times \left(\sum_{k=0}^{\infty} v'_k \right) .$$

At this point, the theorem was proved, but ABEL continued his argument by generalizing the theorem through the use of power series. ABEL now abandoned the assumptions that both factors had to be absolutely convergent in favor of the assumption that both the factors and the Cauchy product were (simply) convergent. In his notes on ABEL'S binomial paper, SYLOW wrote of this generalization: "The theorem VI is due to *Cauchy* but the new form which it is given [...] originates with ABEL."[60] The generalized version of the *Cauchy product theorem* can thus be stated as follows.

Theorem 13 (Generalized Cauchy product theorem) *If the three series*

$$\sum_{k=0}^{\infty} t_k, \ \sum_{k=0}^{\infty} t'_k, \text{ and } \sum_{k=0}^{\infty} \sum_{n+m=k} t_n t'_m$$

60 "*Le théorème VI est dû à* Cauchy, *mais la forme nouvelle qu'il a reçue page 226 appartient à* Abel." (ibid., II, 303).

are all convergent, then

$$\left(\sum_{k=0}^{\infty} t_k\right) \times \left(\sum_{k=0}^{\infty} t'_k\right) = \sum_{k=0}^{\infty} \sum_{n+m=k} t_n t'_m. \qquad \square$$

ABEL proved this theorem through elegant application of the previously established theorems. First he simply assumed that $\{t_k\}$ and $\{t'_k\}$ were two sequences converging to zero. Then, by his *Lehrsatz II*, the two power series

$$\sum_{k=0}^{\infty} t_k \alpha^k \text{ and } \sum_{k=0}^{\infty} t'_k \alpha^k$$

would be convergent for $0 < \alpha < 1$, even if numerical values were taken. Thus, be the version of the *Cauchy product theorem* expressed in *Lehrsatz VI*,

$$\left(\sum_{k=0}^{\infty} t_k \alpha^k\right) \times \left(\sum_{k=0}^{\infty} t'_k \alpha^k\right) = \sum_{k=0}^{\infty} \left(\sum_{n+m=k} t_n t'_m\right) \alpha^k,$$

and by letting $\alpha \to 1$, *Lehrsatz IV* supplied the desired conclusion.

12.9 ABEL's proof of the binomial theorem

After having established his six preliminary theorems, ABEL proceeded to the binomial theorem. Throughout the binomial paper, ABEL never equated the expressions

$$(1+x)^m \text{ and } \sum_{\mu=0}^{\infty} \frac{\prod_{k=0}^{\mu-1}(m-k)}{\mu!} x^\mu$$

directly because the former could be multi-valued whereas the latter had only a single value as a function of x (see page 227). Instead, ABEL began his argument by asking for which values of m and x the binomial series

$$\phi(x) = \sum_{\mu=0}^{\infty} m_\mu x^\mu$$

converged, where he let m_μ represented the binomial coefficient

$$m_\mu = \frac{m(m-1)(m-2)\dots(m-\mu+1)}{\mu!} = \frac{\prod_{s=0}^{\mu-1}(m-s).}{\mu!}$$

Transformation into real series. ABEL wanted to include complex values of m and x, which he introduced by letting

$$x = a + ib \text{ and } m = k + ik',$$

where the notation i has been adopted for ABEL'S $\sqrt{-1}$. ABEL wrote the factors of the binomial coefficients in polar form,

$$\frac{m-\mu+1}{\mu} = \delta_\mu \left(\cos\gamma_\mu + i\sin\gamma_\mu\right),$$

which meant

$$\delta_\mu \left(\cos\gamma_\mu + i\sin\gamma_\mu\right) = \frac{k+ik'-\mu+1}{\mu},$$

and for each given μ, the values of δ_μ and γ_μ could be found. When these factors were multiplied to produce the binomial coefficients, ABEL found

$$m_\mu = \left(\prod_{n=1}^{\mu}\delta_n\right)\times\left(\cos\left(\sum_{n=1}^{\mu}\gamma_n\right)+i\sin\left(\sum_{n=1}^{\mu}\gamma_n\right)\right).$$

With the conventions

$$x = \alpha\left(\cos\phi + i\sin\phi\right),\ \lambda_\mu = \prod_{n=1}^{\mu}\delta_n, \text{ and } \theta_\mu = \mu\phi + \sum_{n=1}^{\mu}\gamma_n,$$

ABEL had thus decomposed the general term of the binomial series into the form

$$m_\mu x^\mu = \lambda_\mu\left(\cos\theta_\mu + i\sin\theta_\mu\right)\alpha^\mu,$$

thereby reducing the binomial series to its real and imaginary parts,

$$\phi(x) = \underbrace{1+\sum_{\mu=1}^{\infty}\lambda_\mu\alpha^\mu\cos\theta_\mu}_{=p} + i\underbrace{\sum_{\mu=1}^{\infty}\lambda_\mu\alpha^\mu\sin\theta_\mu}_{=q}. \tag{12.24}$$

Convergence of the binomial series. Having obtained the decomposition of the binomial series into real and imaginary parts (12.24), ABEL claimed that it converged if $\alpha < 1$ and diverged if $\alpha > 1$. In order to prove this claim, he applied his version of the ratio test, observing that because

$$\delta_{\mu+1} = \sqrt{\left(\frac{k-\mu}{\mu+1}\right)^2 + \left(\frac{k'}{\mu+1}\right)^2} \to 1 \text{ as } \mu\to\infty,$$

the ratio of consecutive terms converged to α,

$$\frac{\lambda_{\mu+1}\alpha^{\mu+1}}{\lambda_\mu\alpha^\mu} = \delta_{\mu+1}\alpha \to \alpha \text{ for } \mu\to\infty.$$

ABEL took care of the trigonometric factors of the general terms, $\cos\theta_\mu$ and $\sin\theta_\mu$ by applying his own version of the ratio test as expressed in his *Lehrsätze I&II*. However, he did not provide any details of the argument. In the simplest case, $\alpha < 1$, the absolute convergence of both the series p and q can be obtained directly from *Lehrsatz*

Proof that the trigonometric coefficients cannot approach zero First, we consider the series

$$p-1=\sum_{\mu=1}^{\infty}\lambda_\mu\alpha^\mu\cos\theta_\mu.$$

The calculation

$$\begin{aligned}\cos\theta_{\mu+1}&=\cos\left(\theta_\mu+\phi+\gamma_{\mu+1}\right)\\&=\cos\phi\cos\left(\theta_\mu+\gamma_{\mu+1}\right)-\sin\phi\sin\left(\theta_\mu+\gamma_{\mu+1}\right)\\&=\cos\phi\left(\cos\theta_\mu\cos\gamma_{\mu+1}-\sin\theta_\mu\sin\gamma_{\mu+1}\right)\\&-\sin\phi\left(\sin\theta_\mu\cos\gamma_{\mu+1}+\cos\theta_\mu\sin\gamma_{\mu+1}\right)\end{aligned}$$

shows that if $\left\{\cos\theta_\mu\right\}$ is convergent, $T=\lim\cos\theta_\mu$, then

$$T=-T\cos\phi+\sin\phi\sqrt{1-T^2}$$

because $\lim\cos\gamma_\mu=-1$ and $\lim\sin\gamma_\mu=0$.

Consequently, if $\left\{\cos\theta_\mu\right\}$ is convergent, its limit has to be given by the equation

$$T=\pm\sqrt{\frac{1-\cos\phi}{2}},$$

which is only zero if $\cos\phi=1$, i.e. if x is on the real axis.

Similarly, for the series

$$q=\sum_{\mu=1}^{\infty}\lambda_\mu\alpha^\mu\sin\theta_\mu,$$

the situation is completely analogous and the conclusion remains the same.

Box 5: Proof that the trigonometric coefficients cannot approach zero

II, because the trigonometric coefficients never surpass 1, numerically. On the other hand, if $\alpha>1$, the divergence of the series p and q rested on the observation that neither of the trigonometric coefficients approached zero. The details of this observation which unless x is real are provided in box 5.

In the case $\alpha<1$, ABEL proceeded to utilize his previously established theorems. First, he showed by *Lehrsatz VI*, that provided $\phi(m)$, $\phi(n)$, and the *Cauchy product* $\phi(m)\,\phi(n)$ were all convergent series, the product was equal to $\phi(m+n)$. And since $\phi(m+n)$ was assumed to be convergent, ABEL had showed that $\phi(m)$ was a solution to the functional equation.

In order to express everything in real variables, ABEL next introduced

$$\phi(m)=p+qi=r(\cos s+i\sin s)$$

which he wrote as

$$\phi\left(k+k'i\right)=f\left(k,k'\right)\left(\cos\psi\left(k,k'\right)+i\sin\psi\left(k,k'\right)\right).$$

With $n=l+l'i$, ABEL found the analogous of the above and proceeded to express $\phi\left(m+n\right)$ in the same way to find internal relations of f and ψ. He expressed the functional relation $\phi\left(m+n\right)=\phi\left(m\right)\phi\left(n\right)$ in terms of the real arguments

$$\begin{aligned}
&f\left(k+l,k'+l'\right)=f\left(k,k'\right)f\left(l,l'\right), \text{ and}\\
&\psi\left(k+l,k'+l'\right)=2M\pi+\psi\left(k,k'\right)+\psi\left(l,l'\right),
\end{aligned}$$

where M denoted an integer. Now, ABEL wanted to find the functions f and ψ which satisfied these equations. He first proved that f was a continuous function, basically because it was composed of continuous functions. Similarly, he claimed ψ could be assumed to be continuous by choosing a constant value for M.

ABEL then obtained the equation

$$\psi\left(k,k'+l'\right)+\psi\left(l,k'+l'\right)=2M\pi+\psi\left(0,k'\right)+\psi\left(0,l'\right)+\psi\left(k+l,k'+l'\right).$$

This equation helped him determine the way ψ depended upon its first argument. ABEL let $\theta\left(k\right)=\psi\left(k,k'+l'\right)$ which rendered the equation as

$$\theta\left(k\right)+\theta\left(l\right)=a+\theta\left(k+l\right) \tag{12.25}$$

and he proceeded to solve this equation. He did so by first proving directly that for integer ρ and any k,

$$\rho\theta\left(k\right)=\left(\rho-1\right)a+\theta\left(\rho k\right). \tag{12.26}$$

In particular, for $k=1$, ABEL found the solution

$$\theta\left(\rho\right)=\rho\left(\theta\left(1\right)-a\right)+a$$

for integer values of ρ. He then proved that this result extended first to rational values of ρ and then to any positive or negative real value of ρ by the continuity of θ. His extension to rational values was classical: By (12.26),

$$\begin{aligned}
\rho\theta\left(\frac{\mu}{\rho}\right)&=\left(\rho-1\right)a+\theta\left(\mu\right)=\left(\rho-1\right)a+\mu\left(\theta\left(1\right)-a\right)+a, \text{ i.e.}\\
\theta\left(\frac{\mu}{\rho}\right)&=a+\frac{\mu}{\rho}\left(\theta\left(1\right)-a\right).
\end{aligned}$$

ABEL next investigated the second argument of ψ by similar methods, and he found

$$\psi\left(k,k'\right)=\beta k+\beta'k'-2M\pi.$$

Having solved the functional equation of the angular function ψ, ABEL next reduced the functional equation of the modular function f to the same equation. He observed that if

$$f\left(k,k'\right) = e^{F\left(k,k'\right)},$$

the equation

$$f\left(k+l,k'+l'\right) = f\left(k,k'\right) f\left(l,l'\right)$$

was reduced to

$$F\left(k+l,k'+l'\right) = F\left(k,k'\right) + F\left(l,l'\right)$$

which he had just solved to find

$$F\left(k,k'\right) = \delta k + \delta' k'.$$

Therefore, he found the solution

$$\phi\left(k+k'i\right) = e^{\delta k+\delta' k'}\left(\cos\left(\beta k+\beta' k'\right) + i\sin\left(\beta k+\beta' k'\right)\right)$$

and reduced it to its real and imaginary terms. Still, the constants $\beta, \beta', \delta, \delta'$ were not more precisely determined. ABEL returned to this issue and provided formulae for determining these constants,

$$\beta = \arctan\frac{\alpha\sin\phi}{1+\alpha\cos\phi} \text{ and}$$
$$\delta = \frac{1}{2}\log\left(1+2\alpha\cos\phi+\alpha^2\right).$$

Finally, ABEL employed his *Lehrsatz IV* to treat the case $\alpha = 1$ as the limit case of the previously considered cases. He summarized the results of the entire investigation in the following way:

Theorem 14 *I. Whenever the series*

$$1 + \frac{m+ni}{1}\left(a+bi\right) + \frac{\left(m+ni\right)\left(m-1+ni\right)}{1\cdot 2}\left(a+bi\right)^2 + \ldots$$

is convergent, *it has the sum*

$$\left[\left(1+a\right)^2+b^2\right]^{\frac{m}{2}} e^{-n\arctan\frac{b}{1+a}} \times \left[\cos\left(m\arctan\frac{b}{1+a}+\frac{n}{2}\log\left[\left(1+a\right)^2+b^2\right]\right) + i\sin\left(m\arctan\frac{b}{1+a}+\frac{n}{2}\log\left[\left(1+a\right)^2+b^2\right]\right)\right]$$

II. The series is convergent for every value of m and n whenever the quantity $\sqrt{a^2+b^2}$ is less than one. If $\sqrt{a^2+b^2}$ is equal to one, the series is convergent for every value of m comprised between -1 and $+\infty$ if one does not simultaneously have $\alpha = -1$. If $\alpha = -1$, m must be positive. In every other case, the series is divergent.[61] □

61 (N. H. Abel, 1826f, 333–334)

This characterization contained the complete two-part answer to the questions which ABEL had raised: the sum of the binomial series when it is convergent and the conditions of its convergence. The cumbersome form of the sum of the binomial series arises partly from the fact that ABEL expressed its complex variables separated into real and imaginary parts, and partly from the answer it gives to the problem of multivalued answers: ABEL'S expression for the sum of the series only has a single value because the bracket is a positive number and the extraction of roots of positive numbers results in a canonical, positive value.

An example relating to ABEL'S "exception". At the very end of the paper, ABEL used the results which he had found to carry out the summation of certain interesting series. In particular, the first example is of interest in connection with ABEL'S famous *exception*.

In the first example,[62] ABEL proposed to sum the series

$$\alpha \sin\phi - \frac{1}{2}\alpha^2 \sin 2\phi + \frac{1}{3}\alpha^3 \sin 3\phi + \dots$$

which he found was convergent for $|\alpha| < 1$ where it converged toward the value β above,

$$\beta = \arctan\frac{\alpha\sin\phi}{1+\alpha\cos\phi} = \sum_{n=1}^{\infty}\frac{(-1)^{n-1}\sin n\phi}{n}\alpha^n.$$

To determine the value for $\alpha = 1$, it sufficed to let α approach the limit 1 provided the resulting series remained convergent (*Lehrsatz IV*). Thus, for ϕ between $-\pi$ and π,

$$\frac{1}{2}\phi = \arctan\frac{\sin\phi}{1+\cos\phi} = \sum\frac{(-1)^{n-1}\sin n\phi}{n}.$$

For $\phi = \pm\pi$, the situation was different because the series vanished and the expression for β degenerated. ABEL observed:

> "It follows, that the function
>
> $$\sin\phi - \frac{1}{2}\sin 2\phi + \frac{1}{3}\sin 3\phi - \dots$$
>
> has the remarkable property of being discontinuous for the values $\phi = \pi$ and $\phi = -\pi$."[63]

Thus, in this case, ABEL used the same object as in the exception for another purpose. This time, he wanted to illustrate the same point as in the notebook (see section 12.6): that although the series of the form $\sum v_m(x)\,\alpha^m$ was continuous for $\alpha < 1$, it needed not be continuous for $\alpha = 1$.

62 (ibid., 336–337).

63 "*Hieraus folgt, daß die Function:*

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

die merkwürdige Eigenschaft hat, für die Werthe $\phi = \pi$ und $\phi = -\pi$ unstetig zu seyn." (ibid., 336–337).

The solution to *Poisson's example.* The ultimate result in ABEL'S binomial paper concerned the series which had probably inspired him to work on the binomial theorem in the first place. As a result of applying his characterization of the convergence of the binomial series, ABEL found precise conditions for the convergence of the binomial series corresponding to $(2\cos x)^m$. The result — an analogy of which ABEL also communicated to HOLMBOE in a letter (see page 226) — was that the identity

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

was valid when m was positive and x belonged to the interval $\left]-\frac{\pi}{2}, \frac{\pi}{2}\right[$. Thus, ABEL ruled out validity of this formula in the situation of *Poisson's example* which had involved setting $x = \pi$. For general values of x, ABEL obtained an identity which involved a correction term,

$$(2\cos x)^m \cos 2\rho m\pi = \sum_{k=0}^{\infty} \binom{m}{k} \cos(m-2k)\,x$$

for $x \in \left]\frac{(2\rho-1)\pi}{2}, \frac{(2\rho+1)\pi}{2}\right[$. Thus, ABEL'S resolution to *Poisson's example* consisted of two steps deriving from his general proof of the binomial theorem. First, he divided the values of x into smaller intervals in which the value of $\cos x$ had a constant sign. And second, he considered all expressions as single valued and introduced an additional term to provide the correction.

12.10 Aspects of ABEL's binomial paper

Having presented and investigated the contents of ABEL'S binomial paper, I believe that three slightly broader aspects of it also merit attention: ABEL'S use of complex numbers, his use of functional equations, and the style of the binomial paper.

12.10.1 ABEL's understanding of complex numbers

Compared with CAUCHY'S *Cours d'analyse*, ABEL'S proof of the binomial theorem excelled by including complex values of the exponent. For complex values of the argument x, CAUCHY had reduced the study of the binomial series corresponding to $(1+x)^m$ to the study of two real series by writing out the real and imaginary parts. By diligent use of polar representations of complex numbers, ABEL succeeded in reducing the functional equations for complex exponents m to the simple, additive one.

Thus, in the binomial paper, ABEL worked with complex numbers which he *always* reduced to pairs of reals either as real and imaginary parts or in polar representation. In his inversion of elliptic integrals into elliptic functions (see chapter 16, below), ABEL also worked with complex numbers as arguments of functions. Again, complex numbers were reduced to real and imaginary parts. From the rather scarce evidence, it seems justified to say that ABEL held a strictly algebraic view of complex numbers and that he considered these numbers rather unproblematic.

Because some of ABEL'S initial theorems — in particular *Lehrsatz IV* — dealt with power series, they have subsequently been interpreted as pertaining to complex variables. However, there is no absolute indication that this was the interpretation which ABEL held. ABEL'S original formulations were not very explicit about these issues and often neglected taking numerical values into consideration. However, I find very little reason to suspect that ABEL would develop his theorems for complex variables and afterwards go through rather cumbersome arguments to reduce complex series to series of real terms.

Going through ABEL'S loans from the Christiania University library, V. BRUN (1885–1978) discovered that ABEL had — in 1822 — borrowed the volume of the transactions of the Danish Academy in which his fellow Norwegian C. WESSEL (1745–1818) had published his geometric interpretation of complex numbers as directed line segments in the plane.[64] However, as Ø. ORE (1899–1968) also pointed out, ABEL was probably much more interested in a paper on equations which C. F. DEGEN (1766–1825) published in the same volume.[65] Even if ABEL read WESSEL'S paper — which seems a reasonable assumption given the limited amount of Danish mathematical literature — he certainly never did anything to adopt its idea or promote it in any other way. This just supports K. ANDERSEN'S (⋆1941) hypothesis that the geometrical interpretation of complex numbers was not a *hot topic* in the first decades of the nineteenth century.[66]

12.10.2 ABEL on functional equations

As had been the case in both EULER'S and CAUCHY'S proof of the binomial theorem, functional equations played a central part in ABEL'S proof. In his *Cours d'analyse*, CAUCHY had developed the topic into a theory of its own and he studied multiple types of functional equations.[67]

In a paper published in 1827 in CRELLE'S *Journal*,[68] ABEL presented some results on functional equations, which when applied to the functional equation

$$\phi(x) + \phi(y) = \phi(x+y) \tag{12.27}$$

gave the solution $\phi(x) = Ax$ as the unique (continuous) solution to this equation. It was precisely this functional equation which was central to ABEL'S proof of the binomial theorem. Whereas EULER and CAUCHY had used the equation

$$f(x) f(y) = f(x+y)$$

64 (Brun, 1962, 110–111). For an analysis of WESSEL'S work, see (K. Andersen, 1999).
65 (C. F. Degen, 1799).
66 (K. Andersen, 1999, 94).
67 (J. Dhombres, 1992).
68 (N. H. Abel, 1827c).

as the foundation for their proofs, ABEL chose to focus on the other equation (12.27) because it was better suited for his investigations of complex exponents. When he had to address the multiplicative functional equation expressing the modulus of the series, he transformed it into the form (12.27) by way of exponentiation.

The additive functional equation (12.27) had also been studied by CAUCHY in his *Cours d'analyse*, and it can be interpreted as testimony to ABEL'S familiarity with that book that he was able to replace the basic tool of the previous proofs with one more suited for his slightly more general situation.

Two general problems concerning functional equations. As mentioned, ABEL published two papers in 1826 and 1827 in which he addressed questions concerning functional equations. The problems which he attacked were within the immediate scope of CAUCHY'S approach to the theory although they may appear a little odd. Thus, ABEL'S results can be seen as contributing to the early growth of the theory of functional equations.

In paper published in 1826,[69] ABEL dealt with functions f such that $f(z, f(x,y))$ was symmetric in x, y, z. For such functions, ABEL obtained the characterization:

> "Whenever a function $f(x,y)$ of two independent variable quantities x and y has the property that $f(z, f(x,y))$ is a symmetric function of x, y, z, there will always be a function ψ for which
>
> $$\psi(f(x,y)) = \psi x + \psi y."^{70}$$

Furthermore, ABEL found that the stipulated function ψ could be determined by the differential equation

$$\psi(x) = \psi'(y) \int \frac{\frac{\partial f}{\partial x}(x,y)}{\frac{\partial f}{\partial y}(x,y)}\, dx.$$

In the process, ABEL also integrated the equation

$$\frac{\partial r(x,y)}{\partial x}\phi(y) = \frac{\partial r(x,y)}{\partial y}\phi(x)$$

to find

$$r = \psi\left(\int \phi(x)\, dx + \int \phi(y)\, dy\right),$$

a result which will resurface in section 16.2.2 where ABEL'S deduction of the addition theorems of elliptic functions are described.

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69 (N. H. Abel, 1826e).

70 "*Sobald eine Function $f(x,y)$ zweier unabhängig veränderlichen Größen x und y die Eigenschaft hat, daß $f(z, f(x,y))$ eine symmetrische Function von x, y und z ist, so muß es allemal eine Function ψ geben, für welche*

$$\psi f(x,y) = \psi(x) + \psi(y)$$

ist." (ibid., 13).

The following year, ABEL treated another problem concerning the form of functions satisfying a functional relation.[71] In that paper, he studied the form of functions ϕ which satisfy

$$\phi(x) + \phi(y) = \psi(xf(y) + yf(x)). \tag{12.28}$$

ABEL found his main result which stated that the most general way of satisfying the equation (12.28) was with

$$\phi(x) = \phi'(0) f(0) \int \frac{dx}{f(x) f'(0) x} \text{ and } \psi(x) = \phi(0) + \phi\left(\frac{x}{f(0)}\right).$$

One very simple application of this result is particularly interesting and a reconstruction of it is given below. If we let $f(x) = 1$, we obtain $\alpha = f(0) = 1$ and $\alpha' = f'(0) = 0$. Therefore, the function ϕ which satisfies the equation ($\psi = \phi$)

$$\phi(x) + \phi(y) = \phi(x+y) \tag{12.29}$$

must satisfy the requirement

$$\phi(x) = \phi(0) \int \frac{dx}{1} = x\phi(0) + C,$$

where $C = 0$ by (12.29). Consequently, ABEL'S result leads to the result that the only (continuous) solution to the functional equation

$$\phi(x) + \phi(y) = \phi(x+y)$$

is the linear function $\phi(x) = Ax$ for some constant A. As observed, this paper—which was published in 1827—therefore contains a generalization of the additive functional equation (12.27) which had been so important to his proof of the binomial theorem. However, in the binomial paper, ABEL probably relied directly on CAUCHY'S *Cours d'analyse*.

12.10.3 Concepts and calculations in the binomial paper

ABEL'S paper on the binomial series shows a remarkable blend of concepts and explicit calculations. A superficial, textual analysis of ABEL'S paper reveals a division of the paper: First, six preliminary theorems were presented which were applicable to classes of series. These were cast in a strictly Euclidean presentational style with definitions of convergence and continuity, statements of the six theorems and proofs following each theorem. Second, detailed and explicit considerations of the convergence of the binomial series as well as formulae for its sum were given. These investigations relied extensively on explicit manipulations of the formulae in forms as described above. When analyzed from this perspective, ABEL'S binomial paper shows traits of both concept based and formula based mathematics as discussed in chapter 21, below. Thus, the binomial paper is an example of the transitional status of ABEL'S mathematics which exhibited similarities with both paradigms.

71 (N. H. Abel, 1827c).

Chapter 13

ABEL and OLIVIER on convergence tests

Besides his proof of the binomial theorem, N. H. ABEL'S (1802–1829) only other publication on analysis — in which his main interest was the theory of infinite series — is a curious little argument against another mathematician named L. OLIVIER.[1] In the first issue of the second volume of the *Journal für die reine und angewandte Mathematik*, A. L. CRELLE (1780–1855) had accepted a paper by OLIVIER in which the latter claimed to have obtained a general — yet very simple — test of convergence. Despite OLIVIER'S claims, the test was not generally applicable and in the next (i.e. third) volume of the *Journal*, ABEL published his refutation which consisted of a counter example to the original claim made by OLIVIER as well as a proof that *no such criterion* could ever be found: it was an utopian dream.[2]

13.1 OLIVIER's theorem

In the article entitled *Remarques sur les séries infinies et leur convergence*,[3] OLIVIER worked with a distinction between *convergent*, *indeterminate*, and *divergent* series (see below) and stated a criterion to distinguish convergent and non-convergent series. This criterion, which OLIVIER called a "criterion of convergence of infinite series"[4] was the following:

> "Thus, if one finds that in an infinite series the product of the n^{th} term – or the n^{th} group of terms which keep the same sign — by n is zero for $n = \infty$, one can regard this single circumstance as a sign that the series is convergent. Reciprocally, the series cannot be convergent unless the product $n \cdot a_n$ is zero for $n = \infty$."[5]

1 Very little is known about LOUIS OLIVIER. However, based on his pattern of publication, I believe that he was a non-professional mathematician with ties to Berlin. I hope to be able to present my analyses of OLIVIER'S mathematical production in the near future.

2 For an analysis of OLIVIER'S criterion and ABEL'S response, see also (I. Grattan-Guinness, 1970b, 139–143) and (Goar, 1999).

3 (Olivier, 1827).

4 "*criterium de la convergence des séries infinies*" (ibid., 34).

OLIVIER'S curious inserted remark — that the n^{th} group of terms with the same sign could be considered instead of a_n — probably derived from the fact that within such a group, reordering of the terms could not effect the convergence or sum of the series. However, both in OLIVIER'S paper and in the present analysis, the important case arises by considering individual terms.

For the historian, OLIVIER'S theorem requires an interpretation which is by no means easy or unambiguous; for instance, what does it mean that "this circumstance is a sign"? The main question in interpreting the theorem lies in this phrase, since it could be read to mean that *if* $na_n \to 0$ *then* the series will always be convergent. This was certainly the way it was interpreted by some of OLIVIER'S readers; however, the phrasing is sufficiently weak to call for further investigation. In the following, OLIVIER'S argument is outlined in order to illustrate how mathematicians in the early 19^{th} century still argued about infinite series. Then, to supplement the theorem and its proof, a consideration of the examples to which it was applied is necessary before a weighed interpretation of the theorem can be given.

13.1.1 OLIVIER's first proof

OLIVIER gave two arguments which illustrate how he came to believe in his theorem. The first argument was given immediately before the theorem was stated, whereas the second one was prompted by ABEL'S objection to the theorem and printed as a response to ABEL'S note.[6]

In 1827, OLIVIER divided infinite series into three categories: *convergent*, *indeterminate*, and *divergent*. His definitions and the ensuing proofs are difficult to represent fairly, because his concepts are different and vague and his style of reasoning is rather verbal and leaves few hints on the unclear points. OLIVIER'S definition of convergent series consisted of two requirements:

> "One calls a series *convergent* which has the following two properties, namely: that one finds its numerical value ever more exactly when one calculates successively more terms and that by continuing the calculation indefinitely, one can approach the true value of the entire series to any degree one wishes.""[7]

In OLIVIER'S definition, we see a curious and obscure mixture of the old and the new concepts of convergence. At the same time, OLIVIER speaks of numerical approx-

5 *"Donc si l'on trouve, que dans une série infinie, le produit du n^{me} terme, ou du n^{me} des groupes de termes qui conservent le même signe, par n, est zéro, pour $n = \infty$, on peut regarder cette seule circonstance comme une marque, que la série est convergente; et réciproquement, la série ne peut pas être convergente, si le produit $n.a_n$ n'est pas nul pour $n = \infty$."* (Olivier, 1827, 34).

6 (Olivier, 1828)

7 *"On appelle* convergente *une série, qui a les deux propriétés suivantes, savoir: qu'on trouve sa valeur numérique d'autant plus exactement, qu'on calcule successivement plusieurs termes, et qu'en continuant indéfiniment ce calcul, on peut se rapprocher de la vraie valeur de la série totale à tel degré qu'on voudra."* (Olivier, 1827, 31).

imation in language similar to A.-L. CAUCHY'S (1789–1857) and of the "true value" of the series which resembles the Eulerian formal equality between functions.

OLIVIER separated non-convergent series into *indeterminate* and *divergent* ones:

> "On the contrary, one calls a series *indeterminate* if continuing the calculation of terms does not make it approach anything.
>
> And one calls a series *divergent* in which the successive terms, added together, produces results which differ more and more from the true value of the series."[8]

This distinction between two types on non-convergent series was probably inspired by the discussion of *Poisson's example* in which OLIVIER also participated without making any noticeable contributions.[9]

OLIVIER proceeded to express the two criteria of his definition of convergence in a slightly different form. First, he observed, the terms of the series (or groups of terms with the same sign) had to constantly decrease. Second, the sum of terms after the n^{th} term, i.e. the tail of the series, had to be zero for $n = \infty$. He gave similar translations of the concepts of indeterminate and divergent series.

To obtain his theorem, OLIVIER first investigated the first condition concerning the vanishing of the terms. He stated that this condition would always be satisfied if the ratio of consecutive terms was always less than one. Thus, he apparently missed out on cases in which $\frac{a_{n+1}}{a_n} < 1$ but $\lim \frac{a_{n+1}}{a_n} = 1$. For the second condition to also be fulfilled, OLIVIER noted that it would be necessary and sufficient that $na_n \to 0$ for $n \to \infty$.

Under hypothesis $na_n \to 0$ and the further assumption that the terms a_n vanish as n increases, OLIVIER claimed that

$$R \leq na_n$$

if the series was written as

$$a_1 + a_2 + \cdots + a_n + R.$$

And thus, the vanishing of the tail R followed. How OLIVIER came to this [false] belief will be clearer below.

On the other hand, the tail R could not vanish without na_n also vanishing, OLIVIER claimed. Using the constantly decreasing nature of the terms, OLIVIER found

$$na_{2n} \leq R \leq na_n$$

where R suddenly meant "the sum of n terms which follows after the n^{th} term."[10] Consequently, if na_n vanished, so did R and the convergence of the series was secured.

---

8 "*Au contraire, on appelle* indéterminée *une série, qui ne donne aucun rapprochement, en continuant le calcul des termes.*
Et on appelle divergente *une série, dont les termes suivants, ajoutés aux précédents, ne donnent que des résultats, qui s'éloignent plus en plus de vraie valeur de la série.*" (ibid., 31).

9 (Olivier, 1826b). For a contemporary evaluation, see ([Saigey], 1826, 112).

10 "*[...] R, ou la somme des* n *termes qui suivent le* n^{me} *terme.*" (Olivier, 1827, 34).

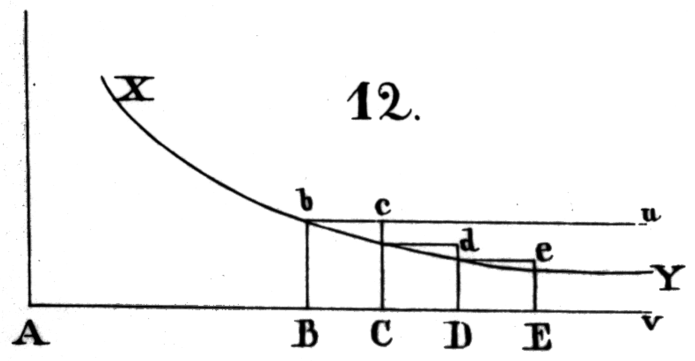

Figure 13.1: OLIVIER'S geometrical argument

Thus, OLIVIER'S proof rested on some uncertain principles. Firstly, OLIVIER'S division of series into three classes and his definition of convergent series was less sharp and less useful than other existing concepts. Secondly, he assumed that in any (numerically) convergent series, the general terms were monotonically decreasing. As indicated, this assumption was central to his proof. Finally, OLIVIER switched from considering R as the tail of the series having infinitely many terms to cutting it off after n terms. This transition between infinite and finite objects represented limit processes which were never spelled out.

13.1.2 OLIVIER's second proof

Reacting to ABEL'S criticism (see below), OLIVIER gave a short indication of the intuition behind his original proof.[11] There, he showed by way of a geometrical figure (see figure 13.1) how he had reasoned. OLIVIER led Bb denote the term a_n and observed that the area of the parallelogram $Bbuv$ which represents the value na_n would be greater than the smaller parallelograms, e.g. Bc, Cd, etc. The inequality was obtained by the constant decreasing of the terms a_n. The equality between the parallelogram $Bbvu$ and na_n represented OLIVIER'S different uses of infinite values for n. Thus, the above interpretation of OLIVIER'S first proof seems to be confirmed.

OLIVIER reacted explicitly to ABEL'S criticism by observing that although his theorem seemed to be well founded and the deduced examples were correct, ABEL had nevertheless observed that it was not "generally applicable". OLIVIER saw ABEL'S criticism as an indication of the care which should be observed when dealing with infinite quantities and locate the mistake to his working indifferently with finite and infinite quantities.

Finally, OLIVIER revised his theorem in order to make it correct by substituting the

11 (Olivier, 1828).

assumption

$$n\left(\sum_{m=1}^{\infty} a_{mn}\right) \to 0$$

for the original $na_n \to 0$. Here, we see how OLIVIER approached the convergence of *Cauchy sequences*.

13.1.3 The application to examples

When OLIVIER came to apply his theorem to particular series, he did so as *a complete test of convergence*, i.e. as the bi-implication

$$na_n \to 0 \quad \Leftrightarrow \quad \sum a_n \text{ convergent.}$$

There are, however, no definite tests of this interpretation, because OLIVIER did not apply his criterion to any divergent series for which $na_n \to 0$.

One of the most interesting examples, which OLIVIER did treat, was the binomial theorem. After long manipulations of the binomial coefficients, OLIVIER used his theorem to state that the binomial series

$$(1+c)^m = 1 + mc + \frac{m(m-1)}{1\cdot 2}c^2 + \dots$$

was convergent for any exponent if $c < 1$ and for $m \geq 0$ if $c = 1$. The binomial series was divergent for $c > 1$ or for $c = 1$ if $m < 0$. In this way, OLIVIER obtained the convergence of the binomial series for real arguments and exponents.

13.2 ABEL's counter example

Soon after the publication of OLIVIER'S paper, ABEL responded with a short note in the *Journal*.[12] There, ABEL commented on OLIVIER'S theorem in the following words,

> "The latter part of this theorem is very true but the first [part] does not seem to be so. For example, the series
>
> $$\frac{1}{2\log 2} + \frac{1}{3\log 3} + \frac{1}{4\log 4} + \dots + \frac{1}{n\log n} + \dots$$
>
> is divergent although $na_n = \frac{1}{\log n}$ is zero for $n = \infty$."[13]

Here, ABEL politely suggested that the first part of OLIVIER'S theorem "did not seem to be true". In one of his notebooks, ABEL'S draft for the paper can be found; there he was more dramatic, remarking "Thus, Mr. Olivier is seriously mistaken."[14] In

12 (N. H. Abel, 1828a).

13 "*La dernière partie de ce théorème est très juste, mais la première ne semble pas l'être. Par exemple la série*

$$\frac{1}{2\log 2} + \frac{1}{3\log 3} + \frac{1}{4\log 4} + \dots + \frac{1}{n\log n} + \dots$$

est divergente, quoique $na_n = \frac{1}{\log n}$ *soit zéro pour* $n = \infty$." (ibid., 79).

14 "*Donc M. Olivier s'est trompé sérieusement.*" (N. H. Abel, [1827] 1881, II, 199).

section 21.3, ABEL'S comments on OLIVIER'S theorem will serve as one among a class of cases where counter examples were employed for different ends and with differing confidence in the early nineteenth century.

Central to ABEL'S proof was the inequality

$$\log(1+x) < x \tag{13.1}$$

which he claimed was valid for all positive x. For $x \geq 1$, it was obvious to ABEL and he gave no argument. It can be easily obtained by observing that

$$x - \log(1+x)$$

is increasing for $x \geq 1$ and positive for $x = 1$. For $x < 1$, ABEL gave an argument employing the expansion of the logarithm into power series as

$$\log(1+x) = \sum_{n=1}^{\infty} \frac{(-1)^{n-1}}{n} x^n = x - \sum_{n=1}^{\infty} \left(\frac{1}{2n} - \frac{x}{2n+1} \right) x^{2n}.$$

From this, he observed that the parentheses were always positive which produced the desired inequality. Here, ABEL thus rearranged the terms of the logarithmic series without further ado.[15]

ABEL employed the inequality (13.1) for $x = \frac{1}{n}$ to produce

$$\frac{1}{n} > \log\left(1 + \frac{1}{n}\right) = \log \frac{n+1}{n} = \log(n+1) - \log n,$$

or written differently

$$\frac{\log(1+n)}{\log n} < \left(1 + \frac{1}{n \log n}\right).$$

Taking logarithms and using the inequality (13.1) again, ABEL obtained

$$\log\log(1+n) - \log\log n = \log\left(\frac{\log(1+n)}{\log n}\right) < \log\left(1 + \frac{1}{n\log n}\right) < \frac{1}{n \log n}.$$

ABEL had thus produced the inequality

$$\log\log(1+n) < \log\log n + \frac{1}{n\log n},$$

which when summed from 2 to n gave

$$\log\log(1+n) < \log\log 2 + \sum_{k=2}^{n} \frac{1}{k \log k}.$$

Since the left hand side obviously became infinite for $n = \infty$, the series on the right hand side was divergent contradicting OLIVIER'S theorem since $na_n = \frac{1}{\log n} \to 0$. ABEL'S conclusion was again remarkably reserved and apparently underplayed,

> "The theorem announced in the above citation is thus at fault in this case."[16]

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15 For more on the history of *absolute convergence*, see section 12.7.

16 *"Le théorème énoncé dans l'endroit cité est donc en défaut dans ce cas."* (N. H. Abel, 1828a, (400)).

13.3 ABEL's general refutation

After he had given his counter example to OLIVIER'S theorem, ABEL might have been expected to leave the matter. However, he had more to say on the issue. Creating some procedures to obtain from one divergent series another one which diverged much slower, ABEL could prove that the quest which OLIVIER had undertaken was bound to result in frustration.

ABEL observed that if the series

$$\sum_{n=0}^{\infty} a_n$$

was divergent, then so was the series where each term had been divided by the respective partial sums,

$$\sum_{n=1}^{\infty} \frac{a_n}{s_n} = \sum_{n=1}^{\infty} \frac{a_n}{\sum_{k=0}^{n-1} a_n}.$$

The proof was easily obtained from arguments resembling the proof above. ABEL observed for $n \geq 1$, inequality (13.1) produced

$$\log s_n - \log s_{n-1} < \frac{a_{n-1}}{s_{n-1}},$$

and therefore, by summation,

$$\log s_n - \log a_0 < \sum_{k=1}^{n} \frac{a_k}{s_k}.$$

Since $s_n \to \infty$, the left hand side diverged, which implied the divergence of the right hand side. We may express this result in modern language as lemma 2.

Lemma 2 *If $\sum_{n=0}^{\infty} a_n$ is a divergent series of positive terms, then the series defined as*

$$\sum_{n=1}^{\infty} \frac{a_n}{s_n}$$

will be divergent as well. □

Now, in order to generally refute the theorem proposed by OLIVIER, ABEL assumed that a function ϕ taking integer arguments existed such that the series $\sum a_n$ was convergent if and only if $\phi(n) a_n \to 0$ as $n \to \infty$. The series obtained as

$$\sum_{n=1}^{\infty} \frac{1}{\phi(n)} \tag{13.2}$$

would then produce a general counter example to this generalized theorem. The series (13.2) was divergent by the criterion, because $\phi(n) a_n = 1$. On the other hand, when the procedure of obtaining a derived divergent series was applied to (13.2), a new series was obtained

$$\sum_{n=2}^{\infty} \frac{1}{\phi(n) \sum_{k=1}^{n-1} \frac{1}{\phi(k)}}. \tag{13.3}$$

Since the series (13.2) was divergent, the series (13.3) would have to be divergent as well (by the procedure above). On the other hand, the generalized criterion, when applied to (13.3) gave

$$\phi(n)\, a_n = \frac{1}{\sum_{k=1}^{n-1} \frac{1}{\phi(k)}},$$

which by the very divergence of (13.2) converged to zero for $n \to \infty$. Thus, the series (13.3) produced a general counter example to ABEL'S generalization of OLIVIER'S proposed convergence criterion.

By this very elegant proof, ABEL turned OLIVIER'S proposed criterion against itself and it imploded. Thus, ABEL proved that no simple test of convergence of series could be devised. Interpreted as a question of delineation of concepts, ABEL'S result thus meant that the extent of the concept of *convergent* series was not easily determined by external criteria.

13.4 More characterizations and tests of convergence

In its published form, ABEL'S answer to OLIVIER'S paper was a negative one, in the sense that it refused a proposed theorem. However, in his notebooks, ABEL elaborated some of the ideas found therein to such a degree as to produce new, positive knowledge in the form of new characterizations and tests of convergence.[17]

In his notebook draft, ABEL obtained his own version of a limit comparison theorem which provided a necessary criterion for convergence. He claimed that if $\sum \phi(n)$ was a divergent series, and $\sum a_n$ was a convergent one, it would be necessary that "the smallest among the limits of $\frac{a_n}{\phi(n)}$ be zero."[18] ABEL'S proof was indirect: Under the contrary assumption, he wrote $u_n = p_n \phi(n)$ where $p_n \geq \alpha$. Then

$$\sum u_n > \sum \alpha \phi(n) = \alpha \sum \phi(n) \to \infty.$$

From this, ABEL obtained the second part of OLIVIER'S theorem which he had not objected to: Because $\sum \frac{1}{n}$ was known to be divergent, if $\sum a_n$ was to be convergent, it would be necessary that $n a_n$ vanished as n became infinite.

Pairs of convergent and divergent series. Also in the notebook, we find a generalization of the lemma 2 to the effect that the divergence of $\sum a_n$ implied the divergence of the series $\sum \frac{a_n}{s_n^{\alpha}}$ where $0 \leq \alpha \leq 1$ (lemma 2 results from setting $\alpha = 1$). A converse to this result was also obtained when ABEL proved that if the series $\sum a_n$ was divergent, then the series

$$\sum_{n=1}^{\infty} \frac{a_n}{s_n^{1+\alpha}}$$

17 (N. H. Abel, [1827] 1881).

18 (N. H. Abel, 1881, II, 198).

would be *convergent* if $\alpha > 0$. Thus, from a divergent series, ABEL had prescribed means of obtaining two derived series, one of which was divergent, the other convergent.

In another section of the note, ABEL devised another way of obtaining a divergent series, which would lead him to a new test of convergence. ABEL found that for any continuous function $\phi(n)$ which increased without bounds for $n \to \infty$, the series of derived terms,

$$\sum_{n=1}^{\infty} \phi'(n), \tag{13.4}$$

would be divergent.

ABEL'S proof proceeded from the Taylor series expansion of ϕ (to the second term and with remainder),

$$\phi(n+1) = \phi(n) + \phi'(n) + \frac{\phi''(n+\theta)}{2}, \text{ for some } 0 < \theta < 1.$$

At this point, ABEL'S draft style made the precise assumptions of the ensuing deductions difficult to interpret. However, if ABEL'S requirements interpreted to mean $\phi''(n) < 0$, we obtain what was his next line,

$$\phi(n+1) - \phi(n) < \phi'(n).$$

Then, the divergence of (13.4) followed by summation,

$$\phi'(n) > \phi(n+1) - \phi(0) \to \infty \text{ as } n \to \infty.$$

Subsequently, ABEL applied this procedure to prove the divergence of the series

$$\sum_{n=2}^{\infty} \frac{1}{n \prod_{k=1}^{m} \log^k n} \text{ for } m \text{ integral,}$$

where $\log^k n = \log \log^{k-1} n$. ABEL did so by defining

$$\phi_m(n) = \log^m(n+a)$$

and differentiating it to obtain

$$\phi'_m(n) = \frac{1}{(n+a) \prod_{k=1}^{m-1} \log^k(n+a)}.$$

As a consequence of the theorem stated above, the series (corresponding to $a = 0$)

$$\sum_{n=2}^{\infty} \phi'_m(n) = \sum_{n=2}^{\infty} \frac{1}{n \prod_{k=1}^{m-1} \log^k n}$$

was divergent.

On the other hand, ABEL next turned to a function intimately related to the one studied above,[19]

$$\psi(n) = \frac{\phi_m(n)^{1-\alpha}}{1-\alpha}.$$

This time, ABEL'S calculations produced the inequality

$$\psi(n+1) - \psi(n) > \psi'(n+1)$$

corresponding to the fact that ψ' was a decreasing function. Consequently, through a number of calculations, ABEL was led to a series

$$\sum_{n=1}^{\infty} \frac{1}{n \log^m(n)^{\alpha+1} \prod_{k=1}^{m-1} \log^k n}$$

which was convergent if $\alpha > 0$ and another one (corresponding to $\alpha = -1$)

$$\sum_{n=1}^{\infty} \frac{1}{n \prod_{k=1}^{m-1} \log^k n}$$

which was divergent.

A logarithmic test of convergence. These methods of constructing convergent and divergent series led ABEL to a new test of convergence. The underlying idea of ABEL'S argument starts from the two series, one convergent and the other divergent, and compares a given series with these two typical ones. He found by simple arguments based on the results above, that if

$$\lim \frac{\log\left(\frac{1}{u_n n \prod_{k=1}^{m-1} \log^k n}\right)}{\log^{m+1} n} > 1, \tag{13.5}$$

the series $\sum u_n$ was convergent. ABEL'S criterion also indicated, that if the limit in (13.5) was < 1, the series $\sum u_n$ would be divergent. In its polished form, ABEL'S criterion thus became the following:

Theorem 15 *For a series of positive terms $\sum u_n$, the limit*

$$k = \lim_{n\to\infty} \frac{\log\left(\frac{1}{u_n}\frac{d}{dn}\log^m n\right)}{\log^{m+1} n}$$

is considered. If $k > 1$, the series will be convergent; if $k < 1$, it will be divergent; and if $k = 1$, nothing can be said of the convergence or divergence of the series by this test. □

This result was later rediscovered by J. L. F. BERTRAND (1822–1900).[20]

19 ABEL actually also denoted this function by ϕ, but to avoid confusion, I have chosen to label it ψ.
20 (Bertrand, 1842).

ABEL'S continued interest in the theory of series and — in particular — in obtaining new tests of convergence for series are indications of a continued interest in this topic. Due to CAUCHY'S re-founding of analysis, tests of convergence were becoming increasingly important, and a number of new tests were discovered in the nineteenth century.[21]

21 See e.g. (I. Grattan-Guinness, 1970b, 131–151).

Chapter 14

Reception of ABEL's contribution to rigorization

In the course of the nineteenth century, analysis underwent an elaborate program of rigorization which effected both the techniques, results, and questions of the discipline. Basic notions such as real numbers, continuous functions, integrals, and trigonometric functions were revised and deeply changed as reflections of a fundamental transition in the ways mathematicians thought about their subject. The changing concepts and attitudes have been studied intensively by historians.[1] In the twentieth century, N. H. ABEL'S (1802–1829) critical attitude and his part in the revision of analysis have received some interest but in the first decades after his death, these were not issues which attracted the most interest to his mathematics. The following section briefly discusses the reception of ABEL'S work on rigorization and certain aspects of the subsequent development.

14.1 Reception of ABEL's rigorization

Because of the overwhelming development of analysis in the nineteenth century and ABEL'S apparently limited direct impact, the reception of ABEL'S contribution to the rigorization movement is only briefly described from two different perspectives.[2]

14.1.1 Binomial theorem

The most immediate reaction to ABEL'S binomial paper was actually a non-reaction. In 1829 and 1830, A. L. CRELLE (1780–1855) published two papers on the binomial theorem demonstrating that the subject had not been closed by ABEL'S paper of 1826.[3] CRELLE'S proofs were based on his previous research on the so-called *analytical facul-*

1 See e.g. (Bottazzini, 1986; Hawkins, 1970).
2 I hope to subsequently substantiate the analysis of the reception of this part of ABEL'S research through detailed studies of the works of selected, later mathematicians.
3 (A. L. Crelle, 1829a; A. L. Crelle, 1830).

ties and were only partially within the *Cauchyian* approach. CRELLE only considered real arguments and exponents and divided his research into two parts corresponding to the two papers. First, he showed by formal arguments from the trivial identity $1 = 1$ that the binomial and its series had to be identical. The argument involved finite differences which had been such a key component of his research within the German combinatorial school. Second, CRELLE investigated the convergence of the binomial series dividing into separate cases corresponding to various assumptions on a and k (he wrote his binomial as $(1+a)^k$). In each case, CRELLE considered the remainder terms of the series and established conditions of convergence or divergence.

Concerning CRELLE'S publications on the binomial theorem, two remarks can be made. First, the fact that CRELLE published on a particular case of the binomial theorem (real arguments and exponents) after ABEL'S more general result testifies to the debate between the *Cauchyian* program and the German algebraic school. CRELLE wrote in his introduction that he considered his proof to fulfill all requirements including being truly rigorous and general and simultaneously clear and elementary. These positive attributes were obtained through the use of algebraic manipulations.[4] Second, CRELLE did not initially consider or even mention the necessity of convergence of the binomial series. ABEL'S critical attitude may have provoked CRELLE to take up the issue in the second paper. Thus, at least in Germany, A.-L. CAUCHY'S (1789–1857) new program of numerical equality was not immediately accepted — not even in CRELLE'S *Journal* after ABEL'S publication and the translation of the *Cours d'analyse*.[5]

Later in the nineteenth and the twentieth century, when the German combinatorial school eventually lost ground, ABEL'S proof of the binomial theorem was recognized as the first rigorous and general proof.[6] The local criticism and scrutiny did not severely impair the evaluation of ABEL'S proof — primarily because the fundamental notions and knowledge of power series developed immensely over the nineteenth century.

14.1.2 From ABEL's "exception" to uniform convergence

As already indicated and cited, one of the major historical interests in ABEL'S contribution to the rigorization movement was the "exception" which he presented against *Cauchy's Theorem* (see section 12.6).[7] The Fourier series representation of the function $f(x) = \frac{x}{2}$ on an interval such as $]-\pi, \pi[$ provided an example that not every convergent sum of continuous functions was itself a continuous function as the Fourier series was periodically discontinuous at the end-points of the interval.

4 (A. L. Crelle, 1829a, 305).
5 (A. L. Cauchy, 1828).
6 See e.g. (Stolz, 1904).
7 This history is particularly well described in (Bottazzini, 1986). LAKATOS' reconstruction (Lakatos, 1976) is also extremely interesting.

ABEL'S "exception" met with little response in the 1820s and 1830s. Actually, it does not seem to have been quoted as influential in the first half of the 19th century. In the 1840s, however, other and probably independent events put *Cauchy's Theorem* (see page 217) back on the agenda. Independently and simultaneously in 1847, the mathematicians G. G. STOKES (1819–1903) and P. L. VON SEIDEL (1821–1896) published investigations of the conditions under which a convergent sum of continuous functions would *not* result in a continuous function.[8] In both cases, their research led to the realization that a particular mode of convergence was involved and SEIDEL gave it the name of "arbitrarily slow convergence";[9] STOKES developed a refined hierarchy of modes of convergence on intervals.[10] Of the two publications, I find SEIDEL'S particularly interesting because it was set up in the form of a proof analysis and stressed the importance of keeping focus on the relations between limit processes. SEIDEL even proposed notational advances which would help clarify the interdependence of nested limit processes. Such thoughts were important in completely separating limit processes from infinitesimals (see below).

CAUCHY'S eventual reaction. Apparently, even these researches of British and German mathematicians did not directly prompt any reaction from the French mathematicians, in particular CAUCHY. Eventually, CAUCHY did address the *Cauchy Theorem* again in an address to the Paris Academy of 1853.[11] In a paper, prompted by remarks made by French colleagues earlier that year,[12] CAUCHY described how the theorem of the *Cours d'analyse* could be amended so that it no longer suffered any exceptions. The fix which he proposed was the uniform convergence of the series in a form similar to the modern requirement. CAUCHY refined the assumptions of the theorem by requiring that a number N existed such that the difference $|s_m(x) - s_n(x)|$ was less than ε for *all* values of x in the interval I under consideration when $m, n \geq N$. CAUCHY'S requirement can easily be read as the modern definition of uniform convergence on the interval I,

$$\forall \varepsilon > 0\ \exists N > 0\ \forall m, n \geq N\ \forall x \in I: \ |s_m(x) - s_n(x)| < \varepsilon.$$

With this stricter assumption, the original proof of the theorem carried through even without a more elaborate notation to handle the two limit processes. In the paper,[13] CAUCHY considered the series

$$\sum_{n=1}^{\infty} \frac{\sin nx}{n}$$

8 (Seidel, 1847; Stokes, 1847). GRATTAN-GUINNESS has pointed to the works of BJØRLING and considered him the "fourth man" in this development besides STOKES, SEIDEL, and CAUCHY (who was also involved, see below). See (I. Grattan-Guinness, 1986).

9 (Seidel, 1847, 37).

10 See (Stokes, 1847).

11 (A.-L. Cauchy, 1853).

12 (Briot and Bouquet, 1853a; Briot and Bouquet, 1853b).

13 (A.-L. Cauchy, 1853, 31).

which represents the function $f(x) = \frac{\pi - x}{2}$ on the interval $]0, 2\pi[$. Thus, CAUCHY made use of a function similar to *Abel's exception* and spoke of his job as removing the possibility of such exceptions. However, judging from his subsequent proof revision, CAUCHY seems to have adapted a post-1820 use of counter examples (see chapter 21).

CAUCHY'S attitude toward the status of the *Cauchy Theorem* have been debated among historians of mathematics and different conclusions have been reached, in particular depending on which parts of CAUCHY'S 1853-paper have been emphasized.[14] With the advent of non-standard analysis in the twentieth century,[15] interpretations of various radicalism have proposed which render CAUCHY'S use of infinitesimals correct within an enlarged system of real numbers. Although it is difficult to argue that CAUCHY was a modern non-standard analyst,[16] the debate over re-interpretations of his works inspires consideration of the developments which led the modern conception of the basic notions to be fixed they way they were in the century before non-standard analysis.

The notions of convergence and continuity. ABEL'S reading of CAUCHY'S original definitions of continuity and convergence became standardized in the course of the nineteenth century century, noticeably through the works and teachings of G. P. L. DIRICHLET (1805–1859) and K. T. W. WEIERSTRASS (1815–1897). Whereas CAUCHY had possibly been unclear or ambiguous about his definitions, ABEL certainly read *point-wise* convergence and *point-wise* continuity into them.[17] This reading was enforced by the path subsequently taken in analytical research, in particular concerning trigonometric series. For example, a multiplicity of different modes of convergence were introduced as the century unfolded. Some of the new modes of convergence such as absolute convergence and the ones introduced by SEIDEL and STOKES have been touched upon above. Similar to the creation of the concept of uniform convergence, a deliberate distinction between *point-wise* and *uniform* continuity was eventually introduced by H. E. HEINE (1821–1881) in 1872.[18] Toward the end of the nineteenth century, the concepts of convergence were even complemented (or stretched) by concepts of *summability* which could also treat non-convergent series. In the form of summability introduced by G. F. FROBENIUS (1849–1917), a class of non-convergent series such as $\sum(-1)^n x^n$ was ascribed a *sum* in a new sense. FROBENIUS considered the class of series for which the average of the partial sums converged and called this limit the sum of the series. His central result generalized ABEL'S *Lehrsatz IV* and proved that his new definition of sum was a conservative extension of the *Cauchyian* definition. The variety of different concepts was the result of the explicit and precise formulation

14 See e.g. the different interpretations in (Giusti, 1984), (Laugwitz, 1987), and (Bottazzini, 1990, xci).
15 See e.g. (Cleave, 1971; Laugwitz, 1988–89).
16 SPALT has come close to making this claim in (Spalt, 1981), though. More recently, he has retracted and debated this claim, see (Spalt, 2002, 326).
17 See chapter 12.
18 See (Dugac, 1989, 91–94).

of basic notions in the *Cauchyian* sense to which ABEL had also contributed.

14.2 Conclusion

As described, ABEL'S contribution to the rigorization movement in analysis consisted of three aspects. Firstly, ABEL'S initial publication in the field on a general and rigorous proof of the binomial theorem was an impressive and early adoption of the *Cauchyian* program in the theory of series. In six theorems, ABEL presented results pertaining to series which were tailored for his proof of the binomial theorem. Compared with CAUCHY'S original proof, ABEL generalized the binomial theorem to include complex exponents by solving a slightly different functional equation. However, the six introductory theorems and the definitions with which they operated also revealed a development from CAUCHY'S *Cours d'analyse*. Most importantly, ABEL consistently read CAUCHY'S definitions of convergence and continuity as *point-wise* definitions. These interpretations led him to an *exception* to *Cauchy's theorem* and he subsequently replaced the effected theorem with his own version. Later, the exception would lead to the concept of *uniform convergence*. Secondly, ABEL partook in the debate over criteria of convergence which had become very important in CAUCHY'S reformulation of the theory of series. By means of a counter example and a very general argument, ABEL showed that no criteria of a particular form could complete delineate the concept of convergent series. The methods which he employed to this end also led him to a new test of convergence which, however, he did not publish. Eventually, ABEL'S private criticism and scrutiny of the existing methods in analysis was expressed in his letters but only indirectly in his publications. This critical attitude may have influenced some of his contemporaries but — I believe — it was generally not considered among his most important contributions until more historical enquiries made it a central indication of the historical development of rigor in analysis.

ABEL'S contribution to the rigorization movement has simultaneously been interpreted in terms of changing epistemic standards. It has been described how ABEL was aware that a new set of standards were being deployed and that arguments should be modified to conform to these new norms. Moreover, ABEL advocated a critical revision which aimed at investigating how true results and only few paradoxes could arise from standards of argument which were no longer deemed to be rigorous. These aspects as well as ABEL'S famous *exception* and the growth of new concepts will become important issues when the transition of paradigms is discussed in chapter 21. There, the notion of critical revision is invoked as an explanation of the apparent cumulative nature of mathematics and the a rather literal interpretation of the exception is suggested.

In conclusion, I find it fair to say that ABEL'S work on the rigorization of the theory of series was an interlude — albeit a quite passionate one. ABEL'S contributions were slightly marginal and not universally appreciated. Mathematicians who subsequently adhered to the rigorization program may have included ABEL among their heroes and have certainly adopted some of his notions and results but ABEL'S direct role was far from the role of the key initiator of the rigorization — CAUCHY.

Part IV

Elliptic functions and the *Paris mémoire*

Chapter 15

Elliptic integrals and functions: Chronology and topics

After the calculus was invented in the seventeenth century, it was quickly applied to classical problems concerning curves. One of the main achievements of the new tool was the ability to treat curves which had previously been outside the reach of geometry. For instance, the quadrature of the hyperbola provided an analytical way of describing and treating logarithmic functions. After the calculus had conquered such basic curves as the logarithmic and trigonometric ones, the determination of the length of an ellipse became a major obstacle on the path to generality.

In the eighteenth century, L. EULER'S (1707–1783) new vision of the calculus as founded upon *functions* also transformed the way in which curves were approached.[1]

The way in which the elliptic transcendentals enter into the realm of analysis touches upon a number of points which will be described below and — primarily — connected to N. H. ABEL'S (1802–1829) work:

1. During the eighteenth century, a need came to be felt to include higher transcendentals (i.e. functions different from the algebraic, logarithmic, and trigonometric ones) into analysis on a par with the well established elementary functions. In order to accept these new objects into analysis, they had to undergo a process of *becoming known*. This process manifested itself in various ways, e.g. in the search for acceptable analytical representations of the new objects.

2. Because the new functions were in some senses generalizations of the elementary functions, their study opened possibilities of generalization of existing results. In the process, insights into the new objects were obtained which also helped making them known.

3. Ultimately, the consensus on how to introduce particular new higher transcendentals — as primitive functions of algebraic differentials — led to a research pro-

[1] For EULER'S approach to analysis, see section 10.1.

gram aimed at describing in a general way properties of larger classes of transcendentals.

By the end of the 1820s, the theory of elliptic (and even higher) transcendentals was establishing itself as one of the major research fields in mathematics in the century. Thus, much effort was put into the field and many connections with and implications for other fields were discovered.

Evidently, the above themes represent aspects of the fundamental change toward *concept based mathematics*. Based on a description of the background and contents of ABEL'S work with these objects, the transition becomes quite evident and susceptible of further qualification.

The theory of transcendental functions constitutes a major part of ABEL'S works and provide material for further analysis of some of the characterizations of his work. For instance, it will become clear that he employed one standard of rigor — different from the sharp manifest of his foundational research on infinite series — studying these new objects, even when the theory of infinite series was involved. Infinite series (and products) were used to represent the *new* objects of analysis by better established ones. This process of coming to *know* new objects can be traced in ABEL'S works and merits attention.

Furthermore, interesting aspects of ABEL'S general inclination toward algebraic methods is evident in many of his researches on transcendental objects. In order to analyze this algebraic approach to the theory of transcendental objects, an understanding of the questions which ABEL wanted to answer is required. Only when questions and methods are viewed together can sense be made of the statement that "ABEL'S approach was algebraic".

15.1 Elliptic transcendentals before the nineteenth century

Ellipses were known to and treated by mathematicians since the times of the Greeks. They knew that the ellipse can be obtained by a central projection from a circle and deduced numerous results concerning these objects in their mathematical investigations of conic sections. However, with the advent of symbolic notation, the calculus, and I. NEWTON'S (1642–1727) theory of gravitation, the study of curves such as the ellipse changed.

15.1.1 Rectification of the ellipse

When the creators and early practitioners of the calculus sought to promote their new tool, they often attacked problems which belonged to the classical realm of analysis of curves. Traditionally, the study of curves included such problems as the construction of points on the curve, its rectification and quadrature and the determination of its tangents, centres of curvature, involutes, and evolutes. A number of these properties *together* constituted the knowledge required for a curve to be known and it was one of the greatest achievements of the calculus to devise a method for obtaining most of them from a single defining equation.[2]

A variety of different curves was treated in the first decades of the calculus, and many problems were reduced to "simpler" problems, primarily to the construction of points on algebraic curves, the rectification of the circle (inverse trigonometric functions) and the quadrature of the hyperbola (logarithmic functions). A classification of construction problems was established based on the simpler problems to which the solution of the given problem could be reduced.

Despite the efforts of the mathematicians, certain problems defied the accepted known means of solution; for instance, when asked to compute the arc lengths of ellipses and some other curves, mathematicians found that the known simple integrals did not suffice.

In the year 1675, G. W. LEIBNIZ (1646–1716) directed two enquiries concerning the rectification of the ellipse to the British mathematicians J. GREGORY (1638–1675) and NEWTON. LEIBNIZ received the answer that the British mathematicians could only compute the length of an ellipse by approximation, i.e. with the help of infinite series, and did not possess any closed expression for the length. LEIBNIZ, himself, at the time believed that he could reduce this problem (and the rectification of the hyperbola) to the quadratures of the circle and the hyperbola. Later, LEIBNIZ realized that he had been misled by a computational error.[3]

As can be seen from box 6, the rectification of the ellipse involved the computation of an integral of the form $\int \frac{R(x)\,dx}{\sqrt{P_4(x)}}$ in which R and P_4 were polynomials such that $\deg P_4 \leq 4$. Such integrals (with the relaxed assumption that R be only a rational function) were soon called *elliptic integrals* by G. C. FAGNANO DEI TOSCHI (1682–1766).[4] LEIBNIZ' question reflects the search for simpler, finite representations of elliptic integrals.

In a paper written in 1732 but not published until six years later,[5] EULER deduced a series representation of a quarter of the circumference of an ellipse. Based on a figure (see figure 15.1) in which M represented a point on the ellipse with center C and semi-axes $CA = a$ and $CB = b$, EULER expressed the differential of the arc-length $\widehat{AM}$ as

$$ds = \frac{b^2\sqrt{b^2 + t^2 + nt^2}\,dt}{(b^2 + t^2)^{\frac{3}{2}}}$$

2 For information on these aspects of curves, see e.g. (Loria, 1902). For a general discussion on the conceptions of curves before the advent of the calculus, see (H. J. M. Bos, 2001).
3 (Hofmann, 1949, 75,118).
4 (Natucci, 1971, 516). For more on FAGNANO DEI TOSCHI'S work on elliptic integrals, see below.
5 (L. Euler, 1732a).

Rectification of the ellipse Consider the ellipse with major axis $2a$ and minor axis $2b$ given by the Cartesian equation

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} = 1.$$

Obviously, from this equation

$$x = a\cos\theta \text{ and } y = b\sin\theta$$

which means

$$\frac{dx}{d\theta} = -a\sin\theta \text{ and } \frac{dy}{d\theta} = b\cos\theta.$$

To compute the arc length, we find

$$\begin{aligned} s(\theta) &= \int \sqrt{\left(\frac{dx}{d\theta}\right)^2 + \left(\frac{dy}{d\theta}\right)^2}\, d\theta = \int \sqrt{a^2\sin^2\theta + b^2\cos^2\theta}\, d\theta \\ &= b\int \sqrt{1-k^2\sin^2\theta}\, d\theta \text{ with } k^2 = \frac{b^2-a^2}{b^2}. \end{aligned}$$

This is precisely the form of A.-M. LEGENDRE'S (1752–1833) second kind of elliptic integrals with the modulus k (denoted $F(\theta, k)$ in table 15.2).

In order to reduce the integral to the form $\int \frac{R(x)\,dx}{\sqrt{P_4(x)}}$, we substitute $z = \sin\theta$ and get

$$dz = \cos\theta\, d\theta = \sqrt{1-\sin^2\theta}\, d\theta \Rightarrow d\theta = \frac{1}{\sqrt{1-z^2}} dz$$

and thus

$$\int \sqrt{1-k^2\sin^2\theta}\, d\theta = \int \frac{\sqrt{1-k^2z^2}}{\sqrt{1-z^2}}\, dz = \int \frac{1-k^2z^2}{\sqrt{(1-k^2z^2)(1-z^2)}}\, dz.$$

Box 6: Rectification of the ellipse

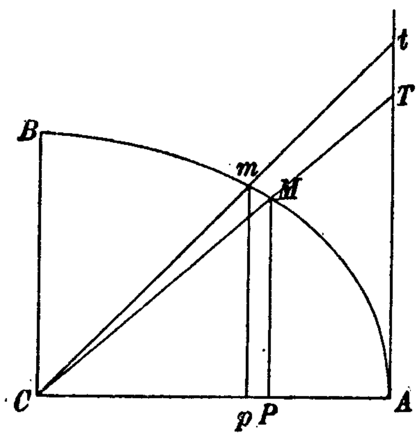

Figure 15.1: EULER'S rectification of an ellipse by infinite series (L. Euler, 1732a, 2)

in which the semi-axes were related by

$$a^2 = (n+1)\,b^2.$$

EULER then expanded the square root by use of the binomial theorem

$$\sqrt{(b^2+t^2)+nt^2} = \sqrt{b^2+t^2} + \sum_{\mu=1}^{\infty} \frac{(-1)^{\mu-1}\prod_{k=1}^{\mu-1}(2k-1)}{\prod_{k=1}^{\mu}(2k)} \frac{n^\mu t^{2\mu}}{(b^2+t^2)^{\frac{2\mu-1}{2}}}.$$

Thus, EULER obtained the differential in the form ($A_0, A_1, \ldots$ were specified constants)

$$ds = b^2 \sum_{\mu=0}^{\infty} \frac{A_\mu n^\mu t^{2\mu}\,dt}{(b^2+t^2)^{\mu+1}}.$$

which he next integrated term-wise from 0 to ∞ to obtain the rectification of a quarter of the ellipse in the form

$$\widehat{AMB} = \frac{\pi}{2} \sum_{\mu=0}^{\infty} \frac{\prod_{k=0}^{\mu-1}(2k+1)^2}{\prod_{k=1}^{\mu}(2k)^2}\,(2\mu-1)\,n^\mu.$$

15.2 The lemniscate

Another curve which received the attention of mathematicians starting with the brothers JAKOB I BERNOULLI (1654–1705) and JOHANN I BERNOULLI (1667–1748) was the so-called *lemniscate*.[6] The curve was defined by the Cartesian equation

$$\left(x^2+y^2\right)^2 = a^2\left(x^2-y^2\right),$$

and both brothers recognized that the arc length of the curve depended on an integral of the form

$$\int \frac{dz}{\sqrt{1-z^4}}$$

(see box 7).

In Italy, the autodidact nobleman FAGNANO DEI TOSCHI took up the study of the lemniscate.[7] By a set of theorems, FAGNANO DEI TOSCHI was able to prove that the division of the quadrant of the lemniscate into k parts could be constructed by ruler and compass if k was of one of the forms 2×2^m, 3×2^m, or 5×2^m. By elimination of the intermediate variable x in the substitutions

$$x = \frac{\sqrt{1-\sqrt{1-z^4}}}{z} \text{ and } x = \frac{\sqrt{2}u}{\sqrt{1-u^4}},$$

FAGNANO DEI TOSCHI obtained that

$$\frac{dz}{\sqrt{1-z^4}} = 2\frac{du}{\sqrt{1-u^4}}$$

and he had obtained the duplication of any segment of the lemniscate arc.

6 See (H. J. M. Bos, 1974). Mostly, discovery of the curve is attributed to BERNOULLI alone as he holds priority of publication and gave the curve its name.

7 Unfortunately, I not have had access to FAGNANO DEI TOSCHI'S original works. Instead, the short outline is based on (R. Ayoub, 1984; Siegel, 1959).

Rectification of the lemniscate To find the arc length of the lemniscate given by the polar equation

$$r^2 = a^2 \cos 2\theta,$$

we compute

$$2r\,dr = -2a^2 \sin 2\theta \; d\theta,$$

i.e.

$$\begin{aligned} ds^2 &= r^2\,d\theta^2 + dr^2 = a^2 \frac{\cos^2 2\theta + \sin^2 2\theta}{\cos 2\theta}\,d\theta^2, \\ ds &= \frac{a\,d\theta}{\sqrt{\cos 2\theta}} = \frac{a\,d\theta}{\sqrt{1-2\sin^2\theta}}, \\ s(\theta) &= a \int \frac{d\theta}{\sqrt{1-2\sin^2\theta}}. \end{aligned}$$

This is an example of an elliptic integral of the first kind. With the substitution $z = \sin\theta$, we find (see box 6)

$$s = a \int \frac{1}{\sqrt{1-2z^2}} \frac{dz}{\sqrt{1-z^2}} = a \int \frac{dz}{\sqrt{1-3z^2+2z^4}}.$$

Box 7: Rectification of the lemniscate

15.2.1 Addition of lemniscatic arcs

EULER took his inspiration directly from the works of FAGNANO DEI TOSCHI. In 1750, after FAGNANO DEI TOSCHI had published his collected works, the author sent a copy to the Berlin Academy of Sciences of which he was a member. The following year, on 23 December 1751, the work came into the hands of EULER who was given the assignment of commenting upon it.[8] C. G. J. JACOBI (1804–1851) has called this date the birthday of elliptic functions.

In the process of preparing an answer for FAGNANO DEI TOSCHI, EULER became very interested in the topic of lemniscate integrals. EULER commented on his new research in a letter to C. GOLDBACH (1690–1764):

> "Recently, I have come across a curious integration. Just as the integral of the equation $\frac{dx}{\sqrt{1-xx}} = \frac{dy}{\sqrt{1-yy}}$ is $yy + xx = cc + 2xy\sqrt{1-cc}$, the integral of the equation $\frac{dx}{\sqrt{1-x^4}} = \frac{dy}{\sqrt{1-x^4}}$ is
>
> $$yy + xx = cc + 2xy\sqrt{1-c^4} - ccxxyy."^{9}$$

---

8 (Siegel, 1959).

9 "*Neulich bin ich auch auf curieuse Integrationen verfallen. Dann gleich wie von dieser Äquation* $\frac{dx}{\sqrt{(1-xx)}} = \frac{dy}{\sqrt{(1-yy)}}$ *das integrale ist* $yy + xx = cc + 2xy\sqrt{(1-cc)}$, *also ist von dieser Äquation*

Figure 15.2: LEONHARD EULER (1707–1783)

In the letter, EULER applied the theorem to demonstrate how the difference between two segments of the arc of an ellipse could be rectified. There is no proof of the theorem in the letter but EULER published a proof in 1756/56.[10] The proof progressed by direct differentiation of the purported integral

$$y^2 + x^2 = c^2 + 2xy\sqrt{1-c^4} - c^2x^2y^2 \tag{15.1}$$

to obtain

$$\frac{dx}{\sqrt{1-x^4}} = \frac{dy}{\sqrt{1-y^4}}.$$

The theorem founded a particular branch of the theory of elliptic integrals as it contained the so-called *addition theorem* for lemniscate integrals. If x and y were related by (15.1), the equation

$$\int_0^x \frac{dt}{\sqrt{1-t^4}} = \int_0^y \frac{dt}{\sqrt{1-t^4}} + C$$

$\frac{dx}{\sqrt{(1-x^4)}} = \frac{dy}{\sqrt{(1-y^4)}}$ *das integrale:*

$$yy + xx = cc + 2xy\sqrt{(1-c^4)} - ccxxyy.''$$

(Euler→Goldbach, 1752. Euler and Goldbach, 1965, 347–348); also (Fuss, 1968, I, 567).

10 (L. Euler, 1756/57).

holds where C is constant (independent of x and y). However, if we let $y = 0$, we find $x^2 = c^2$, and thus

$$C = \int_0^c \frac{dt}{\sqrt{1-t^4}}.$$

In this form, the addition theorem for lemniscate integrals is apparent,

$$\int_0^x \frac{dt}{\sqrt{1-t^4}} + \int_0^y \frac{dt}{\sqrt{1-t^4}} = \int_0^z \frac{dt}{\sqrt{1-t^4}}, \text{ if}$$
$$x^2 + y^2 + z^2x^2y^2 = z^2 + 2xy\sqrt{1-z^4}.$$

15.2.2 EULER's rectification of the lemniscate

At least twice, EULER deduced expressions for the arc length of the lemniscate by infinite series. In the third part of his trilogy, the *Institutiones calculi integralis*,[11] EULER found the relation

$$\int_0^1 \frac{x^{m+1}\,dx}{\sqrt{1-x^2}} = \frac{m}{m+1}\int_0^1 \frac{x^{m-1}\,dx}{\sqrt{1-x^2}}. \tag{15.2}$$

Subsequently,[12] EULER expressed the length of the first quadrant of the lemniscate based on the relation

$$\int_0^1 \frac{dx}{\sqrt{1-x^4}} = \int_0^1 \left(1+x^2\right)^{-\frac{1}{2}} \frac{dx}{\sqrt{1-x^2}}.$$

Using the binomial theorem, he wrote

$$\left(1+x^2\right)^{-\frac{1}{2}} = 1 - \frac{1}{2}x^2 + \frac{1\cdot 3}{2\cdot 4}x^4 - \frac{1\cdot 3\cdot 5}{2\cdot 4\cdot 6}x^6 + \ldots$$

and when the term-wise integration was carried out, EULER found the expression

$$\int_0^1 \frac{dx}{\sqrt{1-x^4}} = \frac{\pi}{2}\left(1 - \frac{1^2}{2^2} + \frac{1^2 3^2}{2^2 4^2} - \frac{1^2 3^2 5^2}{2^2 4^2 6^2} + \ldots\right)$$

using the relation (15.2), above.

The deduction of the result in the *Institutiones* is less complicated than the similar result for the ellipse given by EULER in 1732 and described above. However, the basic tools of the two approaches are the same: expansion by use of the binomial theorem and term-wise integration of the power series which was thus obtained.

15.3 LEGENDRE's theory of elliptic integrals

Toward the end of the eighteenth century, LEGENDRE gave the theory of elliptic integrals a new twist with his contributions. In a number of lengthy papers and mono-

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11 (L. Euler, 1768, XI, 208).
12 (ibid., XI, 211).

1793	Mémoire sur les transcendantes elliptiques
1811,1817,1816	Exercises de calcul intégral sur divers ordres de transcendantes et sur les quadratures
1825,1826,1828	Traité des fonctions ellipitiques et des intégrales eulériennes

Table 15.1: LEGENDRE's publications on elliptic transcendentals

Figure 15.3: ADRIEN-MARIE LEGENDRE (1752–1833)

graphs, LEGENDRE developed his theory of elliptic and other higher integrals.[13] Eventually, in the 1820s toward the end of his life, LEGENDRE decided to publish his research on elliptic integrals in the form of a number of monographs. The first two volumes of the *Traité des fonctions elliptiques et des intégrales eulériennes*, laid the foundation and presented the state of the art in the field.

Generally, LEGENDRE'S theory of elliptic integrals concerned the transformation and numerical approximation of these integrals. An important position in LEGENDRE'S approach to the theory was taken by the classification of such integrals into a small number of canonical forms. LEGENDRE worked with three canonical forms which he termed the elliptic functions of the first, second, and third kinds [*espèce*] (see table 15.2 and box 8). Later, ABEL reserved the word *elliptic function* for the inverse

[13] (A. M. Legendre, 1811–1817; A.-M. Legendre, 1793).

Kind	Integral in x	Integral in ϕ	Symbol
First	$\int \frac{dx}{\sqrt{(1-x^2)(1-k^2x^2)}}$	$\int \frac{d\phi}{\sqrt{1-k^2\sin^2\phi}}$	$F(\phi,k)$
Second	$\int \sqrt{\frac{1-k^2x^2}{1-x^2}}\,dx$	$\int \sqrt{1-k^2\sin^2\phi}\,d\phi$	$E(\phi,k)$
Third	$\int \frac{dx}{(1+nx^2)\sqrt{(1-x^2)(1-k^2x^2)}}$	$\int \frac{d\phi}{\left(1+n\sin^2\phi\right)\sqrt{1-k^2\sin^2\phi}}$	$\Pi(\phi,n,k)$

The variables ϕ and x are related by $x = \sin\phi$.
In accordance with 18th century practice, the integrations are to be performed from 0 to the x and ϕ, respectively.

Table 15.2: LEGENDRE's classification of elliptic integrals

function of the integrals and to avoid confusion, I will often refer to LEGENDRE'S *elliptic functions* as *elliptic integrals*.

LEGENDRE introduced the notation

$$\Delta(\phi) = \Delta(\phi,k) = \sqrt{1-k^2\sin^2\phi}.$$

LEGENDRE'S approach based on reducing elliptic integrals to a number of basic forms was not entirely new. In the 1780s, J. L. LAGRANGE (1736–1813) had made similar attempts reducing all elliptic integrals to the basic form

$$\int \frac{M\left(x^2\right)\,dx}{\sqrt{(1\pm p^2x^2)\,(1\pm q^2x^2)}}$$

in which M was a rational function.[14] Later, ABEL took up the reduction and suggested another set of basic forms. Thus, from the last decades of the 18th century and into the 1820s, the theory of elliptic integrals was still in its formative process and the basic representations had not been decided upon yet. With the advent of LEGENDRE'S *Traité des fonctions elliptiques*, its author hoped to settle the foundations once and for all and his categorization into three kinds of integrals was successful. Soon thereafter, however, the theory changed dramatically with the novel ideas of ABEL and JACOBI, and instead it was JACOBI'S *Fundamenta nova* which became the foundation of the theory and established its notation.[15]

Complete integrals and the reduction program. LEGENDRE introduced the notation E^1, F^1, Π^1 for the *complete integrals* which corresponded to $\phi = \frac{\pi}{2}$. These particular numbers (functions of the modulus k) received some special attention and a number of remarkable relations were discovered among them. LEGENDRE developed the complete integrals E^1 and F^1 into series. LEGENDRE also devoted quite some effort to the problems of *comparison* of elliptic integrals of the three kinds.

14 (Lagrange, 1784–1785, 264).
15 (C. G. J. Jacobi, 1829). See also chapter 20.

LEGENDRE'S reduction of elliptic integrals In his reduction, LEGENDRE first considered the integral $\int \frac{P\,dx}{R}$ in which P was a polynomial and R was the square root of a fourth degree polynomial,

$$R = \sqrt{\sum_{m=0}^{4} \alpha_m x^m}.$$

Obviously, such integrals could be studied by studying the simpler ones of the form $\int \frac{x^k\,dx}{R}$ which LEGENDRE denoted Π^k (this is distinct from the Π which denotes integrals of the third kind). LEGENDRE found

$$x^{k-3}R = \int d\left(x^{k-3}R\right) = (k-3)\int x^{k-4}R\,dx + \int x^{k-3}R'\,dx \tag{15.3}$$

$$= \sum_{m=0}^{4}\left(k-3+\frac{m}{2}\right)\alpha_m \Pi^{m+k-4}. \tag{15.4}$$

This meant, that for any $k \geq 4$, the integral Π^k depended algebraically on the previous integrals $\Pi^0, \Pi^1, \ldots, \Pi^{k-1}$. By writing out (15.4), LEGENDRE observed that also the integral Π^3 only depended algebraically on Π^0, Π^1, and Π^2. Therefore, the integral $\int \frac{P\,dx}{R}$ in which P was a polynomial could be reduced algebraically to the integrals Π^0, Π^1, and Π^2. Furthermore, knowledge of Π^0 and Π^2 would also entail knowledge of Π^1 by a linear transformation.

Thus, elliptic integrals $\int \frac{P(x)\,dx}{R}$ in which P was a polynomial had been taken care of and had been reduced to the two integrals $\int \frac{dx}{R}$ and $\int \frac{x^2\,dx}{R}$. For more general, rational functions P, LEGENDRE expanded into partial fractions, considered $\int \frac{dx}{(1+nx)R}$, and applied a similar line of argument.

Initially, LEGENDRE chose and ordered his basic forms according to the conic sections whose rectification they described. Thus, the integral $E = \int \Delta\,d\phi$ was considered the most basic as it described the rectification of the ellipse. The second class was initially represented by $\Upsilon = \Delta \tan\phi - \int \Delta\,d\phi + b^2 \int \frac{d\phi}{\Delta}$ which represented the arc of the hyperbola. This class was, however, soon replaced by $F = \int \frac{d\phi}{\Delta}$. The third class was represented by the integral $\Pi = \int \frac{d\phi}{\left(1+n\sin^2\phi\right)\Delta}$ which, contrary to the two first kinds, involved a third *parameter*, n.

Box 8: LEGENDRE'S reduction of elliptic integrals

In order to facilitate computation of numerical values, LEGENDRE developed a theory by which a sequence of moduli could be constructed which allowed the numerical approximation of elliptic integrals to be reduced.

15.4 Left in the drawer: GAUSS on elliptic functions

When C. F. GAUSS (1777–1855) was informed of ABEL'S first publication on elliptic functions, the *Recherches*, his answer was extraordinary.[16] GAUSS was impressed with ABEL'S work and was happy to see that the young Norwegian had relieved him of the obligation to publish a third of his own knowledge concerning these elliptic functions. Furthermore, GAUSS was surprised to see that ABEL had followed almost exactly the same route as he, himself, had taken to the point where their symbols were the same. As GAUSS never published any of the monographs on elliptic functions which he had intended, historians have had to look in his diary and in some of his manuscripts for hints concerning his results and methods.

From 1797, GAUSS' diary documents an increasing interest in the lemniscate integral.[17] Despite a lasting interest and many connections to other parts of his research, GAUSS never published on the theory of lemniscate integrals. Thus, GAUSS' ideas only indirectly influenced the development of the theory of elliptic functions in the 1820s. In order to illustrate how GAUSS arrived at some of his insights and to understand his remarks on ABEL'S *Recherches*, a brief discussion of important points in his manuscripts and in his mathematical diary is given. Emphasis is here put on the inversion of the lemniscate integral into GAUSS' lemniscate function, the periods of the lemniscate function, and GAUSS' representation of the lemniscate function by various infinite expressions.

Whereas GAUSS' diary obviously provides a strict chronological frame, his manuscripts are less clearly ordered. The manuscripts contained in the *Werke* have been compiled and put into an order which fit the editor. Therefore, and because GAUSS' ideas had no direct impact on his immediate successors, I have taken the liberty to treat his production in its thematic contexts.

The role of GAUSS' knowledge. As mentioned, GAUSS deferred publication on the subject of lemniscate functions. According to SCHLESINGER, GAUSS had hoped to publish on his research on higher transcendentals in a form which would combine his three greatest interests in the field: the lemniscate function, the arithmetic-geometric means and the hypergeometric series.[18] GAUSS did publish on the hypergeometric series,[19] and there is a brief description of arithmetic-geometric means in his work *De-*

16 (Gauss→Bessel, 1828.03.30. In Gauss and Bessel, 1880).
17 (C. F. Gauss, 1981; J. J. Gray, 1984).
18 (Schlesinger, 1922–1933, 27).
19 (C. F. Gauss, 1813).

terminatio attractionis.[20] Thus, when GAUSS spoke of the third of his research which ABEL had anticipated, he probably referred to the theory of lemniscate functions. Concerning the lemniscate function, GAUSS had performed the inversion, extended to complex variables, found the resulting function to be doubly periodic, expressed its addition formulae, and obtained infinite representations of it. The division problem of the lemniscate had played an important role (together with the arithmetic-geometric means) in motivating his research. Concerning all these results and aspects, GAUSS was certainly correct in observing the similarity with ABEL'S approach. It is possible, but not a necessary assumption, that rumors of GAUSS' investigations and their methods had spread to ABEL through H. C. SCHUMACHER (1784–1873) and C. F. DEGEN (1766–1825); certain of GAUSS' letters to SCHUMACHER suggest that GAUSS for a short while considered it a possibility.[21]

15.5 Chronology of ABEL's work on elliptic transcendentals

Little is known of ABEL'S first encounters with elliptic functions. Presumably, ABEL took up DEGEN'S suggestion in the letter to C. HANSTEEN (1784–1873) and began studying the higher transcendentals possibly through the works of EULER and LEGENDRE. A letter from his stay in Copenhagen 1823 indicates that he had shown DEGEN a small paper in which "inverse functions of elliptic transcendentals" played a role.[22] In both editions of ABEL'S *Œuvres*, a number of manuscripts are included which were among the papers destroyed in a fire in B. M. HOLMBOE'S (1795–1850) house in 1849.[23] According to HOLMBOE, the manuscripts date from before ABEL'S European tour, i.e. they were written before 1825.[24] Apparently based on these manuscripts and the letter from Copenhagen (as there are no other primary sources),[25] some historians have credited ABEL with possessing the key results and methods around 1823.

It was, however, not until during and after the European tour that ABEL developed and published his research on elliptic functions and higher transcendentals which would merit so much attention. ABEL'S mature research on the topics can be seperated into three categories. His first publication on the subject was the *Recherches*, which introduced the crucial idea of inverting elliptic integrals of the first kind into elliptic functions and established the latter as doubly periodic functions of a complex variable.

Simultanously with the publication of ABEL'S *Recherches*, the German mathematician CARL GUSTAV JACOB JACOBI announced some results on the transformation of

20 (C. F. Gauss, 1818).
21 (Schlesinger, 1922–1933, 167). I hope to have more to say on this at a later stage in connection with future research on the mathematical milieu in Copenhagen in the early nineteenth century.
22 (Abel→Holmboe, Kjøbenhavn, 1823/08/04. N. H. Abel, 1902a, 5).
23 (N. H. Abel, 1881, II, 324) and (Stubhaug, 1996, 560).
24 (Holmboe in N. H. Abel, 1839, i)
25 (Abel→Holmboe, Kjøbenhavn, 1823/08/04. In N. H. Abel, 1902a, 4–8).

VII	*Propriétés remarquables de la fonction* $y = \phi x$ *déterminée par l'équation* $fy.dx - fx\sqrt{(a-y)(a_1-y)(a_2-y)\dots(a_m-y)} = 0$, f *étant une fonction quelconque de* y *qui ne devient pas nulle ou infinie lorsque* $y = a, a_1, a_2, \dots, a_m$
VIII+IX	*Sur une propriété remarquable d'une classe très étendue de fonctions transcendantes*
X	*Sur la comparaison des fonctions transcendantes*
XIII	*Théorie des transcendantes elliptiques*

Table 15.3: ABEL'S early unpublished works on elliptic integrals and related topics. The manuscripts are no longer extant but HOLMBOE dated them all to the period before ABEL'S European tour. The roman numerals indicate the position of the manuscript in (N. H. Abel, 1881, II).

elliptic integrals which were astounding to ABEL. JACOBI had obtained results which were special cases of ABEL'S own findings, and ABEL was surprised by the sudden element of competition. For a period of time, ABEL devoted himself to explaining and elaborating the results of JACOBI within his own framework and this constituted a second topic in his research on elliptic functions.

ABEL'S last approach to elliptic functions was the most general. Applying the theory which he developed in the *Paris memoir* concerning integration of algebraic differentials (see chapter 19) to the special case of elliptic functions, ABEL could sketch a very general approach to elliptic functions which — based on functions of the first kind — introduced all kinds of elliptic functions.

These aspects — which far from exhaust the discipline of elliptic and higher transcendentals in the early nineteenth century — will be addressed in the subsequent chapters. A complete description of the history of these transcendental objects is way outside the scope of the present work as their study was one of the most important and widely studied mathematical topics in the period. Instead, selections have been made to illustrate how new objects were being introduced and how new tools — and primarily algebraic tools — were being put to use in the investigation of these new objects.

Chapter 16

The idea of inverting elliptic integrals

As noted, N. H. ABEL (1802–1829) probably started developing his interest in elliptic integrals as a direct response to C. F. DEGEN'S (1766–1825) suggestion. Before his European tour, he was already well acquainted with the comprehensive treatment the theory had been given by A.-M. LEGENDRE (1752–1833) and he had begun developing his own ideas. Soon, the theory of elliptic functions would occupy most of his resources. As already stated, ABEL'S first publication on the theory of elliptic transcendentals was his *Recherches sur les fonctions elliptiques* which appeared in two parts in A. L. CRELLE'S (1780–1855) *Journal* in 1827 and 1828.[1]

16.1 The importance of the lemniscate

ABEL'S *Recherches* were designed to address particular questions pertaining to elliptic integrals of the first kind because these integrals had "the most remarkable and simple properties".[2] Later, ABEL'S penetrating knowledge of elliptic integrals of the first kind would also allow him to attack all elliptic functions from a general perspective (see chapter 20).

In the *Recherches*, ABEL studied integrals of the form

$$\int \frac{dx}{\sqrt{(1-c^2x^2)\,(1+e^2x^2)}} \tag{16.1}$$

which do not immediately belong to either of the kinds classified by LEGENDRE. ABEL argued that his choice of representation made the obtained formulae more simple and stressed that in (16.1), the integrand was more symmetric than in LEGENDRE'S standard form.

The simplicity of central formulae which ABEL emphasized is particularly clear in one specific application of the theory which ABEL developed in the second part of the *Recherches*.[3] There, ABEL solved the division problem for the lemniscate integral which

1 (N. H. Abel, 1827b; N. H. Abel, 1828b).
2 (N. H. Abel, 1827b, 102).
3 (N. H. Abel, 1828b).

Figure 16.1: Stamp depicting Gauss and the construction of the regular 17-gon.

is undoubtedly the simplest integral of the form (16.1) corresponding to $e = c = 1$.[4] Thus, there is a suggestion that ABEL'S choice of representation of the integrals is a direct reflection of one of the main purposes of the *Recherches*, the adaption of C. F. GAUSS' (1777–1855) methods from the *Disquisitiones arithmeticae* to the division of the lemniscate.[5]

16.2 Inversion in the *Recherches*

The issue of CRELLE'S *Journal* which contained ABEL'S inversion of elliptic integrals into elliptic functions was published on 20 September 1827;[6] the date is of importance in analyzing the internal relations between ABEL'S and C. G. J. JACOBI'S (1804–1851) approaches (see below and section 18.1, below).

In the introduction to the *Recherches*, ABEL described his idea:

> "In this memoir, I propose to study the inverse function, i.e. the function $\phi\alpha$ determined by the equations
>
> $$\alpha = \int \frac{d\theta}{\sqrt{1 - c^2 \sin^2\theta}} \text{ and}$$
> $$\sin\theta = \phi(\alpha) = x.\text{"}^{7}$$

[4] See (Glaisher, 1902).
[5] See also chapter 7.
[6] (N. H. Abel, 1881, II, 305).

Thus, from ABEL'S own description of it, the idea of considering the inverse functions appears quite natural. However, by this simple step, an entirely new class of functions was introduced, and they certainly looked different from anything known so far.

With ABEL'S choice of representation of the integral, the inversion became

$$\alpha = \int_0^x \frac{dx}{\sqrt{(1-c^2x^2)(1+e^2x^2)}} \rightsquigarrow \phi(\alpha) = x. \tag{16.2}$$

First, ABEL introduced a special name for the integral from 0 to $\frac{1}{c}$:

> "By thus letting
>
> $$\frac{\omega}{2} = \int_0^{\frac{1}{c}} \frac{\partial x}{\sqrt{[(1-c^2x^2)(1+e^2x^2)]}},$$
>
> it is evident that $\phi(\alpha)$ is positive and increasing from $\alpha = 0$ to $\alpha = \frac{\omega}{2}$."[8]

This remark seems to indicate that ABEL was well aware that for the inversion to be meaningful, the integral had to be a strictly monotonous function.

ABEL'S next step consisted in the observation that the integral was an odd function of x, and thus $\phi(-\alpha) = -\phi(\alpha)$. At this point, ABEL had thus obtained the function ϕ for a segment of the real axis $\left[-\frac{\omega}{2}, \frac{\omega}{2}\right]$.

16.2.1 Going complex

ABEL'S study of the inverse functions of elliptic integrals relied importantly on the extension of these inverse functions to allow for imaginary and complex arguments. As discussed below, this aspect is extremely interesting in connection with the creation of a (rigorous) theory of complex integration.

In analogy with the substitution of $-x$ for x used above, ABEL observed:

> "By inserting into (1) xi instead of x (where i for short represents the imaginary quantity $\sqrt{-1}$) and designating the value of α by βi, it gives
>
> $$xi = \phi(\beta i) \text{ and } \beta = \int_0^x \frac{\partial x}{\sqrt{[(1+c^2x^2)(1-e^2x^2)]}}."^{9}$$

7 *"Je me propose, dans ce mémoire, de considérer la fonction inverse, c'est-à-dire la fonction $\phi\alpha$, déterminée par les équations*

$$\alpha = \int \frac{\partial\theta}{\sqrt{(1-c^2\sin^2\theta)}} \textit{ et}$$
$$\sin\theta = \phi(\alpha) = x."$$

(N. H. Abel, 1827b, 102).

8 *"En faisant donc*

$$\frac{\omega}{2} = \int_0^{\frac{1}{c}} \frac{\partial x}{\sqrt{[(1-c^2x^2)(1+e^2x^2)]}},$$

il est évident, que $\phi\alpha$ e[s]t positif et va en augmentant depuis $\alpha = 0$ jusqu'à $\alpha = \frac{\omega}{2}$ [...]" (ibid., 104).

The formal substitution of an imaginary value thus seemed to preserve the form of the function with the sole exception that the roles of the quantities c^2 and e^2 were interchanged. To ABEL this was fully sufficient, as he simple wrote:

> "thus, one sees by supposing c instead of e and e instead of c,
>
> $$\frac{\phi(\alpha i)}{i} \text{ changes into } \phi(\alpha)."^{10}$$

Thus, when I let $\phi_{(c,e)}(\alpha)$ denote the function in (16.2), ABEL'S formal imaginary substitution gave

$$\phi_{(c,e)}(\alpha i) = i\phi_{(e,c)}(\alpha)$$

and he had found the function $\phi_{(c,e)}$ for a section of the imaginary axis $\left[-\frac{\bar{\omega}}{2}, \frac{\bar{\omega}}{2}\right] i$,[11] in which

$$\frac{\bar{\omega}}{2} = \int_0^{\frac{1}{e}} \frac{dx}{\sqrt{(1+c^2x^2)(1-e^2x^2)}}.$$

16.2.2 Addition theorems

Of central importance to ABEL'S approach to the inversion was the use which he made of addition formulae for elliptic functions.

Auxiliary functions f and F. ABEL introduced two auxiliary functions which he named f and F, derived from $\phi(\alpha)$, which played central parts in his deductions and were treated analogous to ϕ,

$$f(\alpha) = \sqrt{1-c^2\phi^2(\alpha)} \text{ and } F(\alpha) = \sqrt{1+e^2\phi^2(\alpha)}.$$

Obviously, the product of these functions equals $\phi'(\alpha)$ and the functions f and F were, themselves, doubly periodic functions corresponding to JACOBI'S cn and dn, respectively, which will be introduced and discussed later.

9 *"En mettant dans (1.) xi au lieu de x (ou i, pour abréger, représente la quantité imaginaire $\sqrt{-1}$) et désignant la valeur de α par βi, il viendra*

$$xi = \phi(\beta i) \text{ et } \beta = \int_0^x \frac{\partial x}{\sqrt{[(1+c^2x^2)(1-e^2x^2)]}}."$$

(N. H. Abel, 1827b, 104).

10 *"[...] donc on voit, qu'en supposant c au lieu de e et e au lieu de c,*

$$\frac{\phi(\alpha i)}{i} \text{ se changera en } \phi\alpha."$$

(ibid., 104).

11 I write $[a,b]\,i$ as a short-hand for the segment of the imaginary axis which can also be written as $\{xi : x \in [a,b]\}$.

ABEL'S derivation of the addition formulae. ABEL'S way of obtaining addition formulae for elliptic functions resembles L. EULER'S (1707–1783) argument (see section 15.2.1) because both proceeded from a suggested formula. In ABEL'S case, he sought to establish the identity

$$\phi\left(\alpha+\beta\right)=\frac{\phi\left(\alpha\right)f\left(\beta\right)F\left(\beta\right)+\phi\left(\beta\right)f\left(\alpha\right)F\left(\alpha\right)}{1+e^2c^2\phi^2\left(\alpha\right)\phi^2\left(\beta\right)} \tag{16.3}$$

and similar formulae for the auxiliary functions f and F,

$$f\left(\alpha+\beta\right)=\frac{f\left(\alpha\right)f\left(\beta\right)-c^2\phi\left(\alpha\right)\phi\left(\beta\right)F\left(\alpha\right)F\left(\beta\right)}{1+e^2c^2\phi^2\left(\alpha\right)\phi^2\left(\beta\right)} \text{ and} \tag{16.4}$$

$$F\left(\alpha+\beta\right)=\frac{F\left(\alpha\right)F\left(\beta\right)+e^2\phi\left(\alpha\right)\phi\left(\beta\right)f\left(\alpha\right)f\left(\beta\right)}{1+e^2c^2\phi^2\left(\alpha\right)\phi^2\left(\beta\right)}. \tag{16.5}$$

ABEL denoted the right hand side of (16.3) by $r=r\left(\alpha,\beta\right)$ and proceeded to differentiate r with respect to α. The expression which he obtained after inserting the values of f and F proved to be symmetric in α and β. Therefore, and because r itself was symmetric in α and β, ABEL concluded that

$$\frac{\partial r}{\partial\alpha}=\frac{\partial r}{\partial\beta}. \tag{16.6}$$

This differential equation, ABEL claimed,[12] showed that r was a function of $\alpha+\beta$,

$$r=\psi\left(\alpha+\beta\right). \tag{16.7}$$

Upon inserting $\beta=0$, ABEL immediately recognized $\psi=\phi$, and the addition formula for ϕ had been obtained.

To understand how ABEL concluded that the solution to the differential equation (16.6) must be of the form (16.7), we can get a hint from one of his earlier papers, published in the *Journal*.[13] In that paper, ABEL had established that the solution of the equation[14]

$$\left(\frac{\partial r}{\partial x}\right)\sigma\left(y\right)=\left(\frac{\partial r}{\partial y}\right)\sigma\left(x\right)$$

has the solution

$$r=\psi\left(\int\sigma\left(x\right)\,dx+\int\sigma\left(y\right)\,dy\right)$$

where ψ was arbitrary.[15] To apply to the situation of the addition formulae, take $\sigma=1$ to obtain (16.7).

12 The validity of this claim will be discussed below.
13 (N. H. Abel, 1826e).
14 ABEL'S use of d has been replaced by ∂; and ABEL wrote ϕ where I have substituted σ.
15 (ibid., 12–13).

Periods of ϕ. With the addition formulae in place, ABEL inserted $\beta = \pm\frac{\omega}{2}$ and $\beta = \frac{\bar{\omega}}{2}$ to find by direct computation

$$\phi\left(\alpha \pm \frac{\omega}{2}\right) = \pm\phi\left(\frac{\omega}{2}\right)\frac{f(\alpha)}{F(\alpha)} \text{ and } \phi\left(\alpha \pm \frac{\bar{\omega}}{2}i\right) = \pm\phi\left(\frac{\bar{\omega}}{2}i\right)\frac{F(\alpha)}{f(\alpha)}.$$

Similarly, for the auxiliary functions

$$f\left(\alpha \pm \frac{\omega}{2}\right) = \mp\frac{F\left(\frac{\omega}{2}\right)}{\phi\left(\frac{\omega}{2}\right)}\frac{\phi(\alpha)}{F(\alpha)} \text{ and } f\left(\alpha \pm \frac{\bar{\omega}}{2}i\right) = \frac{f\left(\frac{\bar{\omega}}{2}i\right)}{f(\alpha)};$$
$$F\left(\alpha \pm \frac{\omega}{2}\right) = \frac{F\left(\frac{\omega}{2}\right)}{F(\alpha)} \text{ and } F\left(\alpha \pm \frac{\bar{\omega}}{2}i\right) = \mp\frac{f\left(\frac{\bar{\omega}}{2}i\right)}{\phi\left(\frac{\bar{\omega}}{2}i\right)}\frac{\phi(\alpha)}{f(\alpha)}.$$

When he combined these and inserted e.g. $\alpha = \alpha + \frac{\omega}{2}$ and $\beta = \frac{\omega}{2}$, ABEL found

$$\begin{aligned}\phi(\alpha+\omega) &= \phi\left(\alpha + \frac{\omega}{2} + \frac{\omega}{2}\right) = \phi\left(\frac{\omega}{2}\right)\frac{f\left(\alpha + \frac{\omega}{2}\right)}{F\left(\alpha + \frac{\omega}{2}\right)} \\ &= \phi\left(\frac{\omega}{2}\right)\frac{-\frac{F\left(\frac{\omega}{2}\right)}{\phi\left(\frac{\omega}{2}\right)}\frac{\phi(\alpha)}{F(\alpha)}}{\frac{F\left(\frac{\omega}{2}\right)}{F(\alpha)}} = -\phi(\alpha).\end{aligned}$$

In other words, $\phi(\alpha + 2\omega) = \phi(\alpha)$, and ABEL had discovered that 2ω was a *period* of ϕ. Similarly, $2\bar{\omega}i$ was also found to be a period of ϕ.

The value of ϕ for any complex value $\alpha + \beta i$ of its argument could thus be found, ABEL emphasized, from the values $\phi(\alpha), f(\alpha), F(\alpha)$ and $\phi(i\beta), f(i\beta), F(i\beta)$. Furthermore, if

$$\alpha + \beta i = \left(m\omega \pm \alpha'\right) + \left(n\bar{\omega} \pm \beta'\right)i$$

such that $\alpha' \in \left[0, \frac{\omega}{2}\right]$ and $\beta' \in \left[0, \frac{\bar{\omega}}{2}\right]$, the values of these six functions could be obtained from the values of $\phi(\alpha'), f(\alpha'), F(\alpha')$ and $\phi(\beta' i), f(\beta' i), F(\beta' i)$ by formulae such as

$$\phi(\alpha) = \phi\left(m\omega \pm \alpha'\right) = \pm(-1)^m \phi\left(\alpha'\right).$$

Consequently, the value of ϕ (and of f and F) at any complex argument was determined by the values of $\phi(\alpha)$ (and f and F) in which $\alpha \in \left[0, \frac{\omega}{2}\right]$ or $\alpha \in \left[0, \frac{\bar{\omega}}{2}\right] i$.

ABEL'S extension of the elliptic function ϕ to the entire complex plane may thus be summarized in the following steps (see figure 16.2):

1. The elliptic function $\phi(\alpha)$ was obtained by inversion of the elliptic integral on a segment of the real axis $\left[0, \frac{\omega}{2}\right]$. Because the function was odd, it was simultaneously found for $\alpha \in \left[-\frac{\omega}{2}, 0\right]$.

2. By a formal, imaginary substitution the function $\phi(i\beta)$ was found for $\beta \in \left[0, \frac{\bar{\omega}}{2}\right] i$ and consequently for $\beta \in \left[-\frac{\bar{\omega}}{2}, 0\right] i$. The value of $\phi_{(c,e)}(i\beta)$ was obtained from the inversion of a related elliptic function $\phi_{(e,c)}(\beta)$ on a segment of the real axis.

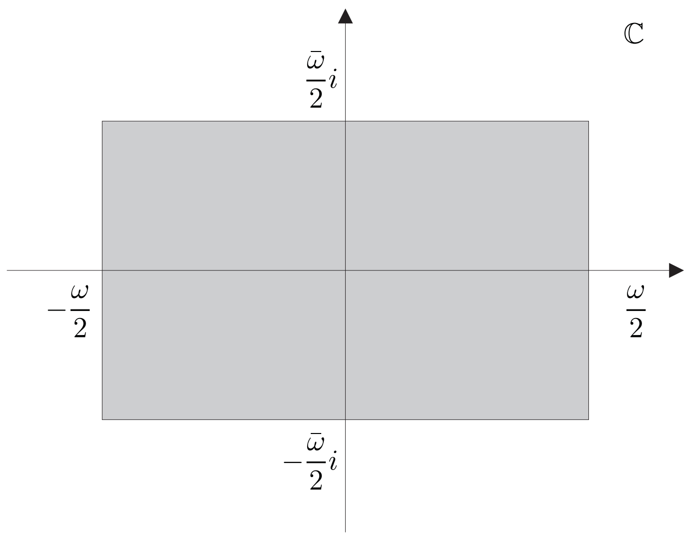

Figure 16.2: ABEL'S extension to the complex rectangle

3. The important addition formulae was demonstrated by differentiation.
4. The two periods of ϕ, 2ω and $2\bar{\omega}$, were direct results of the addition formulae which also provided a way of reducing $\phi(\alpha + i\beta)$ to the values $\phi(\alpha')$ and $\phi(\beta' i)$ in which $\alpha' \in \left[0, \frac{\omega}{2}\right]$ and $\beta' \in \left[0, \frac{\bar{\omega}}{2}\right] i$.

Zeros and poles of ϕ. Solution of $\phi(x) = \phi(a)$. After having established the addition formulae, ABEL proceeded to investigate the singular points of ϕ, i.e. its zeros and poles. He found that every zero of ϕ was of the form

$$m\omega + n\bar{\omega}i \text{ for } m, n \in \mathbb{Z}$$

and that every pole of ϕ was of the form

$$\left(m + \frac{1}{2}\right)\omega + \left(n + \frac{1}{2}\right)\bar{\omega}i \text{ for } m, n \in \mathbb{Z}.$$

ABEL applied a formula — which he had derived directly from the addition formulae — to the equation

$$\phi(x) - \phi(y) = 0$$

and concluded that the complete solution to this equation was of the form

$$x = (-1)^{m+n} y + m\omega + n\bar{\omega}i \text{ for } m, n \in \mathbb{Z}. \tag{16.8}$$

This determination of all the roots of the (transcendental) equation $\phi(x) - \phi(a) = 0$ would soon become very important for ABEL'S main objective, the solution of the division problem (see below).

16.2.3 The question of complex integration

Before going into the division problem, we pause to discuss and comment on ABEL'S inversion and extension of elliptic functions to include complex variables.

In a letter to B. M. HOLMBOE (1795–1850), ABEL praised A.-L. CAUCHY (1789–1857) highly and described how he had struggled to understand the nine issues of the *Exercises de mathématiques* which he had bought and studied:

> "Cauchy is mad and nothing is to be obtained from him although at present, he is the mathematician who knows how mathematics should be conducted. His things are excellent but he writes very obscurely. At first, I almost did not understand a thing of his works but now it is better. He is having a series of papers printed under the title *Exercises des mathématiques*. I buy them and read them carefully. Nine issues have appeared since the beginning of this year."[16]

A large part of CAUCHY'S *Exercises de mathématiques* from the year 1826 concerns the introduction of CAUCHY'S revolutionary new idea of *residues*. The year before, in 1825, CAUCHY had laid the foundation for his theory of complex integration with a brochure entitled *Mémoire sur les intégrales définies, prises entre des limites imaginaries*.[17] Although ABEL never referred directly to the memoir, P. L. M. SYLOW (1832–1918) has found evidence in certain calculations in one of ABEL'S notebooks from the time in Paris that ABEL knew of it.[18]

ABEL'S concept of *functions*. In the *Recherches*, ABEL spoke of *finding* the values of the function $\phi(\alpha)$ for given α. Thus, ABEL'S deductions do not seem to be *defining* this function but rather to be manipulations which make the value of the function available to the observer. This concept of functions resembles EULER'S (see section 10.1) in the sense that the function is tacitly supposed to exist for all values of the variable although it is only strictly meaningful for a subset of the arguments, in this case a segment of the real axis.[19]

ABEL on integration between imaginary limits. As noted, SYLOW found some suggestion in ABEL'S *Notebook A* — which dates from 1826 — that ABEL actually thought of his elliptic functions as defined by complex integration. For instance, one finds in that notebook the formula

$$f(x+yi) = \int_0^{(x+yi)} \frac{dp}{\sqrt{(1-p^2)(1-c^2p^2)}}$$

16 "*Cauchy er fou, og der er ingen Udkomme med ham, omendskjøndt han er den Mathematiker som for nærværende Tid veed hvorledes Mathematiken skal behandles. Hans Sager ere fortræffelige men han skriver meget utydelig. I Førstningen forstod jeg næsten ikke et Gran af hans Arbeider nu gaar det bedre. Han lader nu trykke en Række Afhandlinger under titel* Exercises des Mathematiques. *Jeg kjøber og læser dem flittig. 9 Hefter ere udkomne fra dette Aars Begyndelse.*" (Abel→Holmboe, Paris, 1826/10/24. N. H. Abel, 1902a, 43).

17 (A.-L. Cauchy, 1825).

18 (N. H. Abel, 1881, II, 284).

19 See also (Nørgaard, 1990).

which is nowhere found in the published version in the *Recherches*.[20] Later in the same notebook, ABEL wrote

$$\phi\left(x+y\sqrt{-1}\right)=p+q\sqrt{-1}$$

and deduced the differential equations

$$\left(\frac{dp}{dx}\right)=\left(\frac{dq}{dy}\right)\text{ and }\left(\frac{dp}{dy}\right)=-\left(\frac{dq}{dx}\right)$$

which are the important *Cauchy-Riemann equations*.[21] Thus, as SYLOW concludes,[22] there is good reason to believe that ABEL had studied CAUCHY'S works on integration between imaginary limits. However, there is still no direct indication that ABEL allowed any of these studies or considerations to have an impact on the way he presented his inversion of elliptic integrals.

Complex integration or formal substitution in the *Recherches*? As the evidence seems to be inconclusive, the interpretation of ABEL'S inversion must be left to the historian and depends on the temper of the interpretor. I believe that ABEL'S inversion was formal in the sense that he employed a formal, imaginary substitution to obtain the extension to imaginary arguments. Whether or not, he found any reassurance of his method in CAUCHY'S theory of integration remains an undecidable question.

16.2.4 GAUSS' unpublished results on lemniscate functions

The idea of inverting elliptic integrals into elliptic functions did not belong uniquely to ABEL. Actually, contrary to beliefs expressed throughout the secondary literature, the idea had occurred to LEGENDRE.[23] What LEGENDRE did not fully realize, though, was that the inverted functions should most naturally be considered as functions of a complex variable. This idea is most frequently attributed to GAUSS in whose drawer it remained, however. We may learn a bit more of the idea of inverting elliptic integrals by considering extracts from GAUSS' unpublished works and by comparing with the approach taken by JACOBI after ABEL'S inversion had been published.

In what appears to be GAUSS' first manuscript on the lemniscate function, we get an impression of his approach. GAUSS wrote:

> "We designate the value of the integral from $x=0$ to $x=1$ by $\frac{1}{2}\bar{\omega}$. We denote the variable x of the respective integral by the sign sin lemn and its complementary integral to $\frac{1}{2}\bar{\omega}$ by cos lemn. Thus,
>
> $$\sin\operatorname{lemn}\int\frac{dx}{\sqrt{1-x^4}}=x,\quad \cos\operatorname{lemn}\left(\frac{1}{2}\bar{\omega}-\int\frac{dx}{\sqrt{1-x^4}}\right)=x."^{24}$$

20 (Abel, MS:351:A, 64).
21 (ibid., 100).
22 (N. H. Abel, 1881, II, 284).
23 See (Krazer, 1909, 55) and (J. J. Gray, 1984, 103).

Thus, the function sin lemnα produces the upper limit of the lemniscate integral whose value is α. This is the inverse function of the lemniscate integral and the direct counterpart to (special case of) the elliptic function ϕ which ABEL later, independently, introduced in his *Recherches*.

Was GAUSS' sin lemn a complex function? Clearly, GAUSS had taken the step of considering the inverse function of the lemniscate integral. He invested extensive effort in developing representations by infinite series, infinite products, and ratios of infinite series. In the literature, GAUSS is universally credited with the discovery of the doubly periodic nature of the lemniscate function.[25] This claim is generally supported by GAUSS' consideration of the degree of the division problem for the lemniscate. GAUSS found, and noted in his diary, that the division problem for the lemniscate into n parts led to an equation of degree n^2.[26] This may well have been GAUSS' motivation for considering complex values of the argument. In a manuscript, in which GAUSS wrote the lemniscate function as the ratio of two infinite products

$$\sin\operatorname{lemn}\phi = \frac{P(\phi)}{Q(\phi)},$$

he stated formulae such as

$$\sqrt[4]{2}P\left(\phi + \frac{1}{2}\omega\right) = p\phi \text{ and } p\left(\phi + \frac{1}{2}\omega\right) = -\sqrt[4]{2}P(\phi)$$

which amounted to

$$P(\phi+\omega) = \frac{1}{\sqrt[4]{2}}p\left(\phi + \frac{1}{2}\omega\right) = -P(\phi).$$

This demonstrated the periodic nature of P and a similar result was obtained for Q. More interestingly, GAUSS also wrote

$$P(i\psi\omega) = ie^{\pi\psi^2}P(\psi\omega) \text{ and } Q(i\psi\omega) = e^{\pi\psi^2}Q(\psi\omega)$$

which would indicate that

$$\sin\operatorname{lemn}(i\psi) = i\sin\operatorname{lemn}(\psi)$$

and therefore produce the second period of sin lemnϕ. GAUSS' manuscripts also contain numerous formulae expressing the addition and multiplication of the lemniscate function.[27]

24 "*Valorem huius integralis ab* $x = 0$ *usque ad* $x = 1$ *semper per* $\frac{1}{2}\varpi$ *designamus. Variabilem* x *respectu integralis per signum* sin lemn *denotamus, respectu vero complementi integralis ad* $\frac{1}{2}\varpi$ *per* cos lemn. *Ita ut*

$$\sin\operatorname{lemn}\int\frac{dx}{\sqrt{1-x^4}} = x, \quad \cos\operatorname{lemn}\left(\frac{1}{2}\varpi - \int\frac{dx}{\sqrt{1-x^4}}\right) = x."$$

(C. F. Gauss, 1863–1933, III, 404).

25 (Schlesinger, 1922–1933) and see (J. J. Gray, 1984, 102–103).

26 (ibid., 102).

27 [Ref]

16.2.5 JACOBI's inversion in the *Fundamenta nova*

As will be described in section 18.1, a third inversion of elliptic integrals was performed by CARL GUSTAV JACOB JACOBI who in 1829 published the first book entirely devoted to the study of the new elliptic functions.[28] As will also be illustrated in section 18.1, JACOBI'S main objective with his research on elliptic integrals and functions was the development of transformation theory. After having devised the first set of theorems concerning the transformation of elliptic integrals, JACOBI presented his version of the inversion:

> "Letting $\int_0^\phi \frac{d\phi}{\sqrt{1-k^2\sin^2\phi}} = u$, geometers have accustomed themselves to call the angle ϕ the *amplitude* of the function u. In the following, this angle is denoted by ampl$u$ or shorter by
>
> $$\phi = \operatorname{am} u.$$
>
> Thus, if
>
> $$u = \int_0^x \frac{dx}{\sqrt{(1-x^2)(1-k^2x^2)}}$$
>
> then
>
> $$x = \sin\operatorname{am} u."^{29}$$

JACOBI then introduced the complete integrals already stressed by LEGENDRE,

$$K = \int_0^1 \frac{dx}{\sqrt{(1-x^2)(1-k^2x^2)}} = \int_0^{\frac{\pi}{2}} \frac{d\phi}{\sqrt{1-k^2\sin^2\phi}} \text{ and}$$

$$K' = \int_0^{\frac{\pi}{2}} \frac{d\phi}{\sqrt{1-k'k'\sin^2\phi}} \text{ where } k'k' + kk = 1.$$

---

28 (C. G. J. Jacobi, 1829).

29 "*Posito* $\int_0^\phi \frac{d\phi}{\sqrt{1-k^2\sin^2\phi}} = u$, *angulum* ϕ amplitudinem *functionis* u *vocare geometrae consueverunt. Hunc igitur angulum in sequentibus denotabimus per* ampl$u$ *seu brevius per:*

$$\phi = \operatorname{am} u.$$

Ita, ubi

$$u = \int_0^x \frac{dx}{\sqrt{(1-x^2)(1-k^2x^2)}},$$

erit:

$$x = \sin\operatorname{am} u."$$

(ibid., 81).

Next, JACOBI stated the addition formulae which were presented as well known results concerning elliptic integrals. Only then were complex values of the variable introduced through a substitution $\sin\phi = i\tan\psi$ in the integrand:

$$\frac{d\phi}{\sqrt{1-k^2\sin^2\phi}} = \frac{i\,d\psi}{\sqrt{\cos^2\psi + k^2\sin^2\psi}} = \frac{i\,d\psi}{\sqrt{1-k'k'\sin^2\psi}}.$$

Finally, JACOBI obtained the doubly periodic nature of the function $\sin \mathrm{am} u$ from the addition formulae.

As described, JACOBI'S inversion is quite similar to ABEL'S approach. Based on two different elliptic integrals (corresponding to the complementary moduli k and k'), JACOBI could obtain the value of $\sin \mathrm{am} iu$ on the imaginary axis. Then, by the addition formulae which were apparently assumed to be valid for these complex values of u and v, the two independent periods were deduced. JACOBI was aware that the doubly periodic nature was a new and important feature of these new functions:

> "elliptic functions have two periods, one real and one imaginary whenever the modulus k is real. Both [periods] will be imaginary when the modulus itself is imaginary. We call this the *principle of double periodicities*."[30]

JACOBI'S book became the corner stone of the research on elliptic functions in the following generation, and his notation and ways of introducing elliptic functions became standard for a while until he changed it by introducing elliptic functions by certain infinite series (see chapter 20). In that respect, JACOBI'S works surpassed LEGENDRE'S effort to update his monographs with the newest developments by ABEL and JACOBI which resulted in a supplement to his *Traité des fonctions elliptiques* published in 1828.[31]

16.2.6 Comparison: An earlier idea on inversion

As indicated, ABEL'S inversion in the *Recherches* was the first inversion of elliptic integrals into elliptic functions of a complex variable to appear in print. However, prior to his departure on the European tour, ABEL had written a manuscript which also dealt with the inversion of functions and which is interesting in the discussion of whether ABEL used complex integration or not.

The result. In a manuscript which bears the lengthy but very accurate title *Propriétés remarquables de la fonction* $y = \phi x$ *déterminée par l'équation* $fy.dx - fx\sqrt{(a-y)(a_1-y)(a_2-y)\ldots(a_m}$ 0, f *étant une fonction quelconque de* y *qui ne devient pas nulle ou infinie lorsque* $y =$

---

30 "*functiones ellipticas duplici gaudere periodo, altera reali, altera imaginaria, siquidem modulus k est realis. Utraque fit imaginaria, ubi modulus et ipse est imaginarius. Quod* principium duplicis periodi *nuncupabimus.*" (C. G. J. Jacobi, 1829, 87).

31 (A. M. Legendre, 1825–1828, III).

$a, a_1, a_2, \ldots, a_m$,[32] ABEL considered a problem also bearing on the inversion of elliptic integrals. There, he studied the function $y = \phi(x)$ given by a differential equation (which occurs in the title of the paper)

$$\frac{dy}{dx} = \frac{\sqrt{\psi(y)}}{f(y)}, \text{ in which } \psi(y) = \prod_{k=1}^{m} (a_k - y)$$

and $f(a_1), \ldots, f(a_m)$ were finite and non-zero. Of such functions $\phi(x)$, ABEL proved that they have $\frac{m(m-1)}{2}$ (possibly non-distinct) periods $2(\alpha_k - \alpha_m)$ determined by

$$\alpha_k = \int_0^{a_k} \frac{f(y)\, dy}{\sqrt{\psi(y)}}.$$

One of ABEL'S applications of the result. To recognize the connection with the inversion of elliptic integrals, consider first the case (given by ABEL) of trigonometric functions,

$$f(y) = 1 \text{ and } \psi(y) = (1-y)(1+y)$$

i.e.

$$\int \frac{f(y)\, dy}{\sqrt{\psi(y)}} = \int \frac{dy}{\sqrt{1-y^2}} = \arcsin y.$$

This gave

$$\alpha_1 = \int_0^1 \frac{dy}{\sqrt{1-y^2}} = \frac{\pi}{2} \text{ and } \alpha_2 = \frac{-\pi}{2},$$
$$\phi(x + 2n\pi) = \phi(x).$$

A speculative application of the same result. There is no explicit restrictions on the roots $a_1, \ldots, a_m$ mentioned by ABEL. If we, extending ABEL'S example, allow the roots to be imaginary and consider the lemniscate integral

$$f(y) = 1 \text{ and } \psi(y) = 1 - y^4 = (\pm 1 - y)(\pm i - y),$$

we find periods

$$\alpha_1 = -\alpha_2 = \int_0^1 \frac{dy}{\sqrt{1-y^4}}, \text{ and}$$
$$\alpha_3 = -\alpha_4 = \int_0^i \frac{dy}{\sqrt{1-y^4}} = i\alpha_1.$$

In the last integral, the imaginary integration could be performed via the formal substitution $iy = z$ which ABEL employed in the *Recherches*.[33] Thus, the two periods of the elliptic functions were immediate generalizations of the results obtained in the manuscript.

32 (N. H. Abel, [1825] 1839a).
33 (N. H. Abel, 1827b, 104). See above.

ABEL'S deduction. ABEL'S way of obtaining the described results was to expand the function ϕ in a *Taylor series*

$$\phi(x+v) = \underbrace{\phi(x)}_{=y} + \sum_{k=1}^{\infty} v^{2k} Q_{2k} + \sqrt{\psi(y)} \sum_{k=0}^{\infty} v^{2k+1} Q_{2k+1}.$$

With $y = a_k$ and α_k equal to the corresponding value of x,

$$\alpha_k = \int_0^{a_k} \frac{f(y)\, dy}{\sqrt{\psi(y)}},$$

ABEL found

$$\phi(\alpha_k + v) = a_k + \sum_{k=1}^{\infty} v^{2k} Q_{2k}$$

and thus in this case, $\phi(\alpha_k + v)$ was an even function of v,

$$\phi(\alpha_k + v) = \phi(\alpha_k - v).$$

By inserting $v' = \alpha_k - v$, ABEL obtained

$$\phi\left(2\alpha_k - v'\right) = \phi\left(v'\right).$$

Therefore,

$$\phi(2\alpha_k - 2\alpha_m + v) = \phi(v),$$

and the function was therefore periodic. In general

$$\phi\left(v + 2\sum_{k,k'=1}^{m} n_{k,k'}\left(\alpha_k - \alpha_{k'}\right)\right) = \phi(v).$$

In particular, by taking for v a zero of ϕ, the values

$$v + 2\sum_{k=1}^{m} n_k \alpha_k \text{ for } n_1, \ldots, n_m \in \mathbb{Z} \text{ with } \sum_{k=1}^{m} n_k = 0$$

were also zeros of ϕ.

16.2.7 Conclusion

Because ABEL'S general inversion — which admittedly did not explicitly concern elliptic integrals — was written before he embarked on the European tour in 1825, it cannot rely on any knowledge of the new theory of complex integration which was presented that same year. Also admittedly, the manuscript does not contain any complex integrals or complex periods but I find the suggested application of the result plausible. I do so, because I read ABEL'S inversion of elliptic integrals in the *Recherches* rather literally and see in it a formal substitution without any justification in complex integration. This theme will surface again when the need and means of representations for elliptic functions are discussed in chapter 17.

16.3 The division problem

As already indicated, one of the main objectives of ABEL'S *Recherches* was the so-called *division problems* which encompass deducing and solving the equations which determine the division of the function $\phi(m\alpha)$ into m parts, i.e. the equations which determine $\phi(\alpha)$ from $\phi(m\alpha)$.

ABEL'S starting point for these investigations was the addition formula (16.3). Based on these formula, he found expressions such as

$$\phi((n+1)\beta) = -\phi((n-1)\beta) + \frac{2\phi(n\beta) f(\beta) F(\beta)}{1 + c^2e^2\phi^2(n\beta)\phi^2(\beta)}.$$

Consequently, ABEL observed that the functions $\phi(n\beta)$, $f(n\beta)$, and $F(n\beta)$ depended rationally on $\phi(\beta)$, $f(\beta)$, and $F(\beta)$ and he wrote, e.g.

$$\phi(n\beta) = \frac{P_n}{Q_n}$$

in which P_n and Q_n were polynomial functions of $\phi(\beta)$, $f(\beta)$, and $F(\beta)$. ABEL then let $x = \phi(\alpha)$, $y = f(\beta)$, $z = F(\beta)$ and manipulated the equation to obtain the relations

$$\begin{aligned} Q_{n+1} &= Q_{n-1}R_n \text{ and} \\ P_{n+1} &= -P_{n-1}R_n + 2yzP_nQ_nQ_{n-1} \end{aligned}$$

in which

$$R_n = Q_n^2 + e^2c^2x^2P_n^2.$$

Obviously, R_n was a polynomial function in x^2, and from the basic formulae

$$\phi(\beta) = \frac{P_1}{Q_1} \text{ and } \phi(2\beta) = \frac{2\phi(\beta) f(\beta) F(\beta)}{1 + e^2c^2\phi^4(\beta)} = \frac{P_2}{Q_2},$$

i.e.

$$Q_0 = 1, Q_1 = 1, P_0 = 0, P_1 = x,$$

ABEL found that Q_n was always an entire function of x^2. Furthermore, $\frac{P_{2n}}{xyz}$ and $\frac{P_{2n+1}}{x}$ were also entire functions of x^2. ABEL merely provided the first few particular cases but the argument is easily completed by induction:

$$\begin{aligned} \frac{P_{2n+2}}{xyz} &= \frac{-P_{2n}}{xyz}Q_{2n+1}^2 - \frac{P_{2n+1}}{x}\left(\frac{c^2e^2x^2P_{2n}P_{2n+1}}{yz} + 2Q_{2n+1}Q_{2n}\right) \\ &= \frac{-P_{2n}}{xyz}Q_{2n+1}^2 - \frac{P_{2n+1}}{x}\left(c^2e^2x^2 \times x^2\frac{P_{2n}}{xyz}\frac{P_{2n+1}}{x} + 2Q_{2n+1}Q_{2n}\right) \end{aligned}$$

and all parts are seen to be entire functions of x^2 by the induction hypothesis. Similarly,

$$\begin{aligned} \frac{P_{2n+1}}{x} &= \frac{-P_{2n-1}}{x}R_{2n} + \frac{2yzP_{2n}Q_{2n}Q_{2n-1}}{x} \\ &= \frac{-P_{2n-1}}{x}R_{2n} + 2y^2z^2\frac{P_{2n}}{xyz}Q_{2n}Q_{2n-1} \end{aligned}$$

and the same conclusion holds because $y^2 = 1 - c^2x^2$ and $z^2 = 1 + e^2x^2$.

Bisection by algebraic means. The bisection of the function ϕ was the easiest example of the program which ABEL was developing. In order to express $x = \phi\left(\frac{\alpha}{2}\right)$ by $\phi(\alpha)$, ABEL employed the addition formula for f and F (16.4 and 16.5) to get

$$f(\alpha) = f\left(\frac{\alpha}{2} + \frac{\alpha}{2}\right) = \frac{y^2 - c^2x^2z^2}{1 + e^2c^2x^4} = \frac{\left(1 - c^2x^2\right) - c^2x^2\left(1 + e^2x^2\right)}{1 + e^2c^2x^4} \text{ and}$$
$$F(\alpha) = F\left(\frac{\alpha}{2} + \frac{\alpha}{2}\right) = \frac{z^2 + e^2x^2y^2}{1 + e^2c^2x^4} = \frac{\left(1 + e^2x^2\right) + e^2x^2\left(1 - c^2x^2\right)}{1 + e^2c^2x^4}.$$

When ABEL formed the ratio

$$\frac{F(\alpha) - 1}{1 + f(\alpha)} = \frac{\frac{2e^2x^2\left(1 - c^2x^2\right)}{1 + e^2c^2x^4}}{\frac{2\left(1 - c^2x^2\right)}{1 + e^2c^2x^4}} = e^2x^2,$$

he found that

$$\phi\left(\frac{\alpha}{2}\right) = \frac{1}{e}\sqrt{\frac{F(\alpha) - 1}{1 + f(\alpha)}}.$$

Thus, ABEL had proved that if the value of $\phi(\alpha)$ was known, $\phi\left(\frac{\alpha}{2}\right)$ could be expressed algebraically and actually using only the extraction of square roots.

Division into an odd number of parts. In order to complete the program, ABEL had to find a way of algebraically obtaining $\phi\left(\frac{\alpha}{2n+1}\right)$ from $\phi(\alpha)$ and this proved both more difficult and much more fruitful. In chapter 7, we have already met the *Mémoire sur une classe particulière* where ABEL later introduced *Abelian equations* as generalizations of these investigations.[34]

ABEL wanted to solve the equation which he wrote as

$$\phi(\alpha) = \frac{P_{2n+1}}{Q_{2n+1}}$$

and similar equations for $f(\alpha)$ and $F(\alpha)$. Although ABEL'S argument was greatly simplified by his slightly more general approach of the memoir on *Abelian equations*, the original version of the *Recherches* is worth a brief description to facilitate a comparison. I have suppressed most of the computational technicalities in the following typical step of the proof.

ABEL let θ denote an imaginary $(2n+1)$'th root of unity and introduced three auxiliary functions,

$$\phi_1(\beta) = \sum_{m=-n}^{n} \phi\left(\beta + \frac{2m\omega}{2n+1}\right),$$
$$\psi(\beta) = \sum_{\mu=-n}^{n} \theta^{\mu}\phi_1\left(\beta + \frac{2\mu\bar{\omega}i}{2n+1}\right), \text{ and } \psi_1(\beta) = \sum_{\mu=-n}^{n} \theta^{\mu}\phi_1\left(\beta - \frac{2\mu\bar{\omega}i}{2n+1}\right).$$

34 (N. H. Abel, 1829c).

Based on a direct application of the addition formulae, ABEL obtained an expression

$$\phi_1\left(\beta \pm \frac{2\mu\bar{\omega}i}{2n+1}\right) = R_\mu \pm R'_\mu\sqrt{\left(1-c^2x^2\right)\left(1+e^2x^2\right)}$$

in which R_μ and R'_μ were rational functions of $x = \phi(\beta)$. Consequently, ABEL found that both the functions

$$\psi(\beta)\,\psi_1(\beta) = \lambda(\beta) \text{ and } \psi(\beta)^{2n+1} + \psi_1(\beta)^{2n+1} = \lambda_1(\beta) \tag{16.9}$$

were rational in x. By direct calculation, he also found that both functions (16.9) were invariant if another root $\beta + \frac{k\omega+k'\bar{\omega}i}{2n+1}$ of the equation

$$\phi((2n+1)\beta) = \frac{P_{2n+1}}{Q_{2n+1}} \tag{16.10}$$

was substituted for β. Thus, ABEL knew that $\lambda(\beta)$ and $\lambda_1(\beta)$ were rational in the coefficients of (16.10), in particular in the quantity $\phi((2n+1)\beta)$. When he solved the system of equations (16.9), ABEL found

$$\psi(\beta) = \sqrt[2n+1]{\frac{\lambda_1(\beta)}{2} + \sqrt{\frac{\lambda_1(\beta)^2}{4} - \lambda(\beta)}}. \tag{16.11}$$

From these, ABEL obtained $\phi_1(\beta)$ and then $\phi(\beta)$ by similar arguments.

However, as ABEL observed, the formula for $\phi(\beta)$ which he had obtained also contained the quantities

$$\phi\left(\frac{\omega}{2n+1}\right) \text{ and } \phi\left(\frac{\bar{\omega}i}{2n+1}\right).$$

Thus, in order to completely solve the problem, these two quantities should also be determined and ABEL demonstrated how the equation $P_{2n+1} = 0$ which determined these could be reduced to equations of lower degrees, one of degree $2n+2$ and $2n+2$ equations of degree n. Furthermore, ABEL also proved that the equations of degree n were always solvable by radicals. In the process, ABEL employed tools similar to those described above as well as some knowledge of primitive roots of an integer. Importantly, ABEL knew qualitatively how the roots were interrelated (by 16.8) and used this knowledge to investigate the system of roots and prove its reduction to equations of lower degree, some of which were proved to be solvable by radicals.

16.3.1 Division of the lemniscate

The culmination of ABEL'S research into the division problem was his application of the theory to the case of the lemniscate. The symmetry of ABEL'S representation of the elliptic integrals became evident when he chose $e = c = 1$ to obtain the lemniscate integral

$$\phi(\alpha) = x, \alpha = \int_0^x \frac{dx}{\sqrt{1-x^4}}.$$

In light of the previous result, ABEL'S investigations mainly concerned the division of the complete integral into an odd number of equal segments. He did so by first carrying out the division of the complete integral into $4v+1$ parts. ABEL'S argument was explicitly designed to make the case accessible with the Gaussian approach to the solution of cyclotomic equations. A brief outline of ABEL'S reasoning will provide a few interesting comparisons with GAUSS' approach and the more general solution of the problem found in ABEL'S paper on *Abelian equations*.

ABEL first assumed that $4v+1$ was the sum of two squares, $4v+1=\alpha^2+\beta^2$ and found that $\alpha+\beta$ had to be an odd integer. In this case, he found an equation which he wrote as

$$\phi\left(\left(\alpha+\beta i\right)\delta\right)=x\frac{T}{S} \tag{16.12}$$

with $\alpha=m\delta$, $\beta=\mu\delta$, $x=\phi\left(\delta\right)$, and T and S two entire functions of x^4. ABEL'S real objective was the considerations pertaining to $\delta=\frac{\omega}{\alpha+\beta i}$ for which he obviously found that $x=\phi\left(\delta\right)=\phi\left(\frac{\omega}{\alpha+\beta i}\right)$ was a root of the equation $T=0$. It thus became his objective to solve this equation.

Expressing the roots of $T=0$. First, by his very powerful determination of the roots of $\phi\left(\xi\right)=0$, ABEL found that all the roots of $T=0$ were related by

$$\left(\alpha+\beta i\right)\delta=m\omega+\mu\bar{\omega}i=\left(m+\mu i\right)\omega$$

since $\omega=\bar{\omega}$ for the lemniscate integral. Thus, any root was contained in the formula

$$x=\phi\left(\frac{m+\mu i}{\alpha+\beta i}\omega\right)$$

if m and μ were allowed to assume all integral values. However, in order to count each root only once, ABEL found that the set of roots of $T=0$ could be listed as

$$\phi\left(\frac{\rho\omega}{\alpha+\beta i}\right)\text{ for }-\frac{\alpha^2+\beta^2-1}{2}\leq\rho\leq\frac{\alpha^2+\beta^2-1}{2} \tag{16.13}$$

and he argued by an application of the Euclidean algorithm for integers: ABEL let λ,λ' be determined by the equation

$$\alpha\lambda'-\beta\lambda=1,$$

and t denote an integer in order to obtain

$$\mu+\beta\underbrace{\left(\mu\lambda+t\alpha\right)}_{=k}-\alpha\underbrace{\left(\mu\lambda'+t\beta\right)}_{=k'}=0.$$

Then, with $\rho=m+\alpha k-\beta k'$, ABEL obtained

$$\frac{m+\mu i}{\alpha+\beta i}=\frac{\rho}{\alpha+\beta i}-k-k'i$$

and therefore,

$$\phi\left(\frac{m+\mu i}{\alpha+\beta i}\omega\right)=\phi\left(\frac{\rho}{\alpha+\beta i}\omega-k\omega-k'i\omega\right)=(-1)^{k+k'}\phi\left(\frac{\rho}{\alpha+\beta i}\right).$$

Because of the relation

$$\rho=m+\mu\left(\lambda\alpha+\lambda'\beta\right)+t\left(\alpha^2+\beta^2\right),$$

the bounds of (16.13) were obtained.

The expressed roots of $T=0$ are all distinct. To realize that all the roots of the equation $T=0$ were contained in the list (16.13), ABEL first observed that none of the roots corresponding to different values of ρ could be identical. He did so using the same techniques as above. Then, ABEL found that T had no multiple roots by observing that a multiple root of T would be a common root of $T=0$ and $T'=0$. However, any root of $T'=0$ would also be a root of $S=0$ which was forbidden by assuming that the rational function $\frac{T}{S}$ (16.12) was expressed in its reduced form. As a consequence, ABEL found that all the roots of the original equation reduced to roots of an equation $R=0$ of degree $2v$ in which the roots were

$$\phi^2\left(\frac{\omega}{\alpha+\beta i}\right),\phi^2\left(\frac{2\omega}{\alpha+\beta i}\right),\ldots,\phi^2\left(\frac{2v\omega}{\alpha+\beta i}\right).$$

This equation could, ABEL observed, "easily be solved by the method of GAUSS."[35] Actually, ABEL solved it using the same approach as in the general division problem, i.e. the approach which led to (16.11), above.

Geometrical division of the lemniscate. Having solved the equation $R=0$, ABEL thus had access to the values

$$\phi\left(\frac{k\omega}{\alpha+\beta i}\right)\text{ for }k=1,2,\ldots,2v,$$

and he now proceeded to obtain the value $\phi\left(\frac{\omega}{4v+1}\right)$ by the following brief argument. By the addition theorem, ABEL expressed

$$\phi\left(\frac{mv}{\alpha+\beta i}+\frac{mv}{\alpha-\beta i}\right)=\phi\left(\frac{2m\alpha\omega}{4v+1}\right)$$

in terms of $\phi\left(\frac{mv}{\alpha+\beta i}\right)$ and $\phi\left(\frac{mw}{\alpha-\beta i}\right)$ where the latter could be obtained from the former "by changing i into $-i$".[36] Since 2α and $4v+1$ were relatively prime, ABEL could write any integral n as

$$n=2m\alpha-(4v+1)\,t,$$

35 *"Cela posé, on peut aisément résoudre l'équation $R=0$, à l'aide de la méthode de M. Gauss."* (N. H. Abel, 1828b, 165).

36 (ibid., 166).

i.e.

$$\phi\left(\frac{2m\alpha\omega}{4v+1}\right) = \phi\left(\frac{n\omega}{4v+1} + t\omega\right) = (-1)^t \phi\left(\frac{n\omega}{4v+1}\right)$$

which for $n = 1$ made $\phi\left(\frac{\omega}{4n+1}\right)$ accessible (known).

Throughout, ABEL had explicitly only considered the division into a prime number of parts. When ABEL then made the further assumption that $4v+1 = 1+2^n$, he found by considering the expressions obtained that all the root extractions reduced to square roots. In particular, ABEL had to use that the solution of the equation $\theta^{2^{n-1}} = 1$ could be reduced to square roots; this result is precisely the main result of GAUSS' research on the division of the circle. Combining this result with the case of bisection and the general integral multiplication, ABEL could summarize his investigations on the division of the lemniscate:

> "The value of the function $\phi\left(\frac{m\omega}{n}\right)$ can be expressed by *square roots* whenever n is a number of the form 2^n or a prime number of the form $1+2^n$ or a product of multiple numbers of these two forms."[37]

Two aspects of ABEL'S result merit attention. First, ABEL'S argument hinges on the factors $2^{m_0}, 2^{n_1}+1, \ldots, 2^{n_k}+1$ of n to be relatively prime because he wanted to decompose any number m' into its residues modulo these factors,

$$\phi\left(\frac{m_0}{2^{n_0}} + \frac{m_1}{2^{n_1}+1} + \cdots + \frac{m_k}{2^{n_k}+1}\right) = \phi\left(\frac{m'}{2^{n_0}\left(2^{n_1}+1\right)\ldots\left(2^{n_k}+1\right)}\right).$$

If two of the *Fermat primes* were identical, the decomposition would no longer be possible.[38] ABEL'S deductions immediately leading to the stated theorem contain tacitly the distinctness of the *Fermat primes*, but it could have been explicitly included in the statement.

Second, the result states a sufficient condition of geometrical constructibility and says absolutely nothing of the necessity of this condition. The same can be said of GAUSS' stated result on the division of the circle. However, GAUSS also stated that the division of the circle would lead to precisely his equations and would therefore not be possible with ruler and compass unless the number of parts were of the prescribed form. Similarly, ABEL could have stated that division of the lemniscate was *only* possible if n was a product of a power of 2 and distinct *Fermat primes*.[39] However, the proof of such a statement would go beyond the types of questions which ABEL asked concerning these classes of equations.

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37 "*La valeur de la fonction* $\phi\left(\frac{m\omega}{n}\right)$ *peut être exprimée par* des racines carrées *toutes les fois que* n *est un nombre de la forme* 2^n *ou un nombre premier de la forme* $1+2^n$*, ou même un produit de plusieurs nombres de ces deux formes.*" (N. H. Abel, 1828b, 168).

38 Consider writing e.g. $\frac{3}{25}$ as $\frac{a+b}{5}$ with $a, b \in \mathbb{Z}$.

39 For a proof using more modern techniques, see (M. Rosen, 1981).

16.4 Perspectives on inversion

The present chapter has documented ABEL'S inversion of elliptic integrals into elliptic functions and the intimately related step of extending the resulting function to allow complex values of the argument.

This extension to the complex domain which laid the foundation for ABEL'S research on elliptic functions was based on a formal substitution. Although a rigorous foundation for complex integration was being undertaken in the period, the evidence suggests that ABEL based his inversion on an imaginary substitution in the Eulerian tradition which presumed the existence of the function and sought to discover/construct its values.

The step toward considering the inverse function of general elliptic integrals (of the first kind) was probably motivated by the division problem for the lemniscate to which ABEL was led by GAUSS' remark in the *Disquisitiones arithmeticae*. In the process of solving the division problem for the lemniscate, ABEL solved the associated equation using a technical and rather *ad hoc* approach. Later, in the *Mémoire sur une classe particulière* (see chapter 7), ABEL approached the same set of problems but from a more conceptual approach in which he had clearly grasped the very essential property of the equations.

ABEL'S tailored deductions relied extensively on manipulations of formulae; in particular, ABEL'S addition theorems and his characterization of the roots of the equation $\phi(\alpha) = \phi(\beta)$ served him as important tools. When it came to the investigations on the solubility of the division problems, number theoretic arguments inspired by GAUSS' *Disquisitiones arithmeticae* were also used.

Chapter 17

Steps in the process of coming to "know" elliptic functions

As already described in the previous chapter, N. H. ABEL (1802–1829) introduced his elliptic functions by means of a formal inversion of the elliptic integrals. In order to make these new objects *known*, however, this definition appears to have been quite insufficient. ABEL, himself, was not explicit about the problem — but a comparison of ABEL'S approach with A.-M. LEGENDRE'S (1752–1833) highly numerical approach suggests that ABEL'S purely formal definition based on the formal inversion was lacking in certain respects. For instance, in ABEL'S approach, it would be difficult to compute particular values of ABEL'S elliptic functions purely from the definition. Therefore, he developed means of representing his new functions by existing objects and the objects which he chose were — not surprisingly — infinite series and products.

17.1 Infinite representations

In the first part of the *Recherches*,[1] a large portion of the text is occupied with highly technical and formula-based manipulations which aim at describing the elliptic function ϕ (and the derived functions f and F) in infinite products and series. Two characteristic and interesting examples pertaining to the expansion in doubly infinite sums are discussed below.

Based on the multiplication problem described above, ABEL had found that

$$\phi\left((2n+1)\,\beta\right) = \frac{P_{2n+1}}{Q_{2n+1}} \tag{17.1}$$

in which $\deg P_{2n+1} = (2n+1)^2$ and $\deg Q_{2n+1} = (2n+1)^2 - 1$. Furthermore, $\frac{P_{2n+1}}{x}$

[1] (N. H. Abel, 1827b).

was a polynomial in x^2 and so was Q_{2n+1}. Thus, with

$$P_{2n+1}(x) = ax^{(2n+1)^2} + \cdots + bx \text{ and}$$
$$Q_{2n+1}(x) = cx^{(2n+1)^2-1} + \cdots + d,$$

ABEL found that the sum of the roots of the equation (17.1) would equal $\frac{c}{a}\phi((2n+1)\beta)$. Furthermore, he already knew the complete solution of the equation (17.1), and thus he obtained

$$\phi((2n+1)\beta) = A \sum_{m=-n}^{n} \sum_{\mu=-n}^{n} (-1)^{m+\mu} \phi\left(\beta + \frac{m\omega + \mu\bar{\omega}i}{2n+1}\right). \tag{17.2}$$

Similarly for the products of the roots of (17.1),

$$\phi((2n+1)\beta) = B \prod_{m=-n}^{n} \prod_{\mu=-n}^{n} \phi\left(\beta + \frac{m\omega + \mu\bar{\omega}i}{2n+1}\right).$$

These formulae invited the limit process $n \to \infty$, and it is interesting to see how ABEL carried it out.

17.1.1 Determination of the coefficient A

In order to determine the constant A of (17.2), ABEL wanted to insert a particular value for β and he chose $\beta = \frac{\omega}{2} + \frac{\bar{\omega}}{2}i$. However, this is a singular value (a pole) of ϕ, and ABEL applied a limit argument in the following form. First, using the relation

$$\phi\left(\alpha + \frac{\omega}{2} + \frac{\bar{\omega}}{2}i\right) = -\frac{i}{ec}\frac{1}{\phi(\alpha)} \tag{17.3}$$

derivable from the addition formulae, ABEL found that for $(m,\mu) \neq (0,0)$,

$$\phi\left(\frac{m\omega + \mu\bar{\omega}i}{2n+1} + \frac{\omega}{2} + \frac{\bar{\omega}}{2}i\right) + \phi\left(-\frac{m\omega + \mu\bar{\omega}i}{2n+1} + \frac{\omega}{2} + \frac{\bar{\omega}}{2}i\right) = 0.$$

Consequently, the sum reduced to the term corresponding to $(m,\mu) = (0,0)$. For the last term, ABEL found that

$$\begin{aligned} A &= \lim_{\beta \to \frac{\omega}{2} + \frac{\bar{\omega}}{2}i} \frac{\phi((2n+1)\beta)}{\phi(\beta)} = \lim_{\alpha \to 0} \frac{\phi\left((2n+1)\left(\frac{\omega}{2} + \frac{\bar{\omega}}{2}i + \alpha\right)\right)}{\phi\left(\frac{\omega}{2} + \frac{\bar{\omega}}{2}i + \alpha\right)} \\ &\underset{\text{by (17.3)}}{=} \lim_{\alpha \to 0} \frac{\phi(\alpha)}{\phi((2n+1)\alpha)} = \frac{1}{2n+1} \end{aligned}$$

where tacit applications of the differentiability of ϕ and of the *Rule of l'Hospital* are involved.

17.1.2 Infinite sums

In order to express $\phi(\alpha)$ by an infinite series, ABEL set $\beta = \frac{\alpha}{2n+1}$. Thus,

$$\phi(\alpha) = \phi((2n+1)\beta) = \frac{1}{2n+1} \sum_{m=-n}^{n} \sum_{\mu=-n}^{n} (-1)^{m+\mu} \phi\left(\beta + \frac{m\omega + \mu\bar{\omega}i}{2n+1}\right).$$

Following a string of manipulations designed to group the terms of the right hand side, ABEL reduced the problem to the search for the limit of the double sum

$$\sum_{m=0}^{n-1} \sum_{\mu=0}^{n-1} (-1)^{m+\mu} \psi(m,\mu) \tag{17.4}$$

with

$$\psi(m,\mu) = \frac{1}{2n+1} \frac{2\phi\left(\frac{\alpha}{2n+1}\right)\zeta\left(\frac{\varepsilon_\mu}{2n+1}\right)}{\phi^2\left(\frac{\alpha}{2n+1}\right) - \phi^2\left(\frac{\varepsilon_\mu}{2n+1}\right)}$$

$$\text{in which } \zeta(x) = f(x)F(x) \text{ and } \varepsilon_\mu = \left(m + \frac{1}{2}\right)\omega + \left(\mu + \frac{1}{2}\right)\bar{\omega}i.$$

In order to find the limit of (17.4), ABEL remarked,

> "one attempts to put the preceding quantity [here (17.4)] on the form
>
> $$P + v,$$
>
> in which P is independent of n and v is a quantity which has the limit zero, because then the quantity P is exactly the limit which is sought."[2]

ABEL had a candidate in mind for the expression P when he defined

$$\theta(m,\mu) = \frac{2\alpha}{\alpha^2 - \varepsilon_\mu^2}$$

and

$$\psi(m,\mu) - \theta(m,\mu) = \frac{2\alpha}{(2n+1)^2} R_\mu.$$

His candidate was the double sum $\sum_{m=0}^{\infty} \sum_{\mu=0}^{\infty} (-1)^{m+\mu} \theta(m,\mu)$ and he proceeded in the following way in obtaining this limit.

For each value of m, ABEL argued, the difference was

$$\sum_{\mu=0}^{n-1} (-1)^\mu (\psi(m,\mu) - \theta(m,\mu)) = 2\alpha \sum_{\mu=0}^{n-1} \frac{(-1)^\mu R_\mu}{(2n+1)^2},$$

[2] *"il faut essayer de mettre la quantité précédente sous la forme*

$$P + v,$$

où P est indépendant de n, et v une quantité qui a zéro pour limité, car alors la quantité P sera précisément la limite dont il s'agit." (N. H. Abel, 1827b, 156).

and he claimed and proved that the right hand side was "of the form $\frac{v}{2n+1}$". In the process, ABEL made use of the result that

$$\phi(\alpha) = \alpha + A\alpha^3 + \dots$$

because ϕ was an odd function and $\phi'(0) = f(0)F(0) = 1$.

ABEL'S next step concerned the sum of θ. He found that

$$\sum_{\mu=n}^{\infty} (-1)^{\mu}\,\theta(m,\mu) = \frac{v}{2n+1}$$

and therefore, for each m,

$$\sum_{\mu=0}^{n-1} (-1)^{\mu}\,\psi(m,\mu) = \sum_{\mu=0}^{\infty} (-1)^{\mu}\,\theta(m,\mu) + \frac{v_m}{2n+1}.$$

When he summed these, ABEL found

$$\sum_{m=0}^{n-1}\sum_{\mu=0}^{n-1} (-1)^{m+\mu}\,\psi(m,\mu) = \sum_{m=0}^{n-1} (-1)^{m} \left(\sum_{\mu=0}^{\infty} (-1)^{\mu}\,\theta(m,\mu)\right) + \sum_{m=0}^{n-1} \frac{v_m}{2n+1}$$

and, as ABEL wrote,

$$\sum_{m=0}^{n-1} \frac{v_m}{2n+1} = \frac{nv}{2n+1} = \frac{v}{2},$$

in which "v is a quantity which has zero for its limit".[3] Consequently, ABEL had found a way of expressing $\phi(\alpha)$ as a double infinite sum, e.g.

$$\phi(\alpha) = \frac{1}{ec}\sum_{m=0}^{\infty}\sum_{\mu=0}^{\infty} (-1)^{m+\mu} \left(\frac{(2\mu+1)\varpi}{\left(\alpha-\left(m+\frac{1}{2}\right)\omega\right)^2+\left(\mu+\frac{1}{2}\right)^2\varpi^2} - \frac{(2\mu+1)\varpi}{\left(\alpha+\left(m+\frac{1}{2}\right)\omega\right)^2+\left(\mu+\frac{1}{2}\right)^2\varpi^2}\right). \tag{17.5}$$

ABEL did not stop after having obtained the expansion in infinite series (17.5). Instead, he used similar methods to search for expressions for $\phi(\alpha)$ involving infinite products. ABEL also invested an effort in obtaining expressions involving only one infinite series and transcendental objects in the terms. Among the formulae which he obtained, the following was probably the simplest:

$$\phi(\alpha) = \frac{2}{ec}\frac{\pi}{\varpi}\sum_{k=0}^{\infty} (-1)^k \frac{\varepsilon^{2k+1} - \varepsilon^{-(2k+1)}}{r^{2k+1} - r^{-(2k+1)}}$$

where

$$\varepsilon = \exp\left(\alpha\frac{\pi}{\varpi}\right) \text{ and } r = \exp\left(\frac{\omega\pi}{2\varpi}\right).$$

Thus, as described, ABEL used his multiplication formulae to obtain rather simple infinite representations for elliptic functions. When he passed to the infinite limit, his arguments did not conform to the strict standards of rigor, which he had advocated in the theory of series. Further perspectives on ABEL'S motivations for searching for infinite expressions and his methods of obtaining them are described and discussed in the subsequent sections.

3 (N. H. Abel, 1827b, 161).

17.2 Elliptic functions as ratios of power series

In the *Précis*,[4] listed among the established facts of elliptic functions, ABEL observed that elliptic functions of the first kind (which he now denoted λ, see below) were expressible as the ratio of two convergent power series,

$$\lambda\left(\theta\right)=\frac{\theta+a_1\theta^3+a_2\theta^5+\dots}{1+b_2\theta^4+b_3\theta^6+\dots}. \tag{17.6}$$

With the notation

$$\begin{aligned}\phi\left(\theta\right)&=\theta+a_1\theta^3+a_2\theta^5+\dots\text{ and}\\ f\left(\theta\right)&=1+b_2\theta^4+b_3\theta^6+\dots,\end{aligned} \tag{17.7}$$

ABEL claimed that the functions ϕ and f (which are not to be confused with the functions of the same names in the *Recherches*) satisfied the linked functional equations

$$\begin{aligned}\phi\left(\theta'+\theta\right)\phi\left(\theta'-\theta\right)&=\phi^2\left(\theta\right)f^2\left(\theta'\right)-\phi^2\left(\theta'\right)f^2\left(\theta\right)\text{ and}\\ f\left(\theta'+\theta\right)f\left(\theta'-\theta\right)&=f^2\left(\theta\right)f^2\left(\theta'\right)-c^2\phi^2\left(\theta\right)\phi^2\left(\theta'\right).\end{aligned} \tag{17.8}$$

The sign of the first equation is actually wrong, see below. ABEL also mentioned the same result in his letter to LEGENDRE but he never published any demonstration of it.[5]

In order to see how ABEL came to this expression, the following reconstruction may be suggested based on ABEL'S sparse hints.

In his comments published in the second volume of the *Œuvres*, M. S. LIE (1842–1899) has presented a reconstruction of ABEL'S reasoning based on the same sources, papers and manuscripts. LIE indicated how ABEL'S manuscript notes — given the power series expansion (17.7) — could be interpreted as steps toward determining the remaining coefficients $a_1, a_2, \dots, b_2, b_3, \dots$. However, I interpret the notes slightly differently and infer from them a suggestion of how ABEL came to claim the series expansion (17.7), itself, by use of the expansion in two *Maclaurin series*. I am confident that the following reconstruction is close to ABEL'S original argument.

Derivation of functional equations. In the *Précis*, ABEL had presented the following consequence of the addition formulae,

$$\lambda\left(\theta'+\theta\right)\lambda\left(\theta'-\theta\right)=\frac{\lambda^2\left(\theta'\right)-\lambda^2\left(\theta\right)}{1-c^2\lambda^2\left(\theta\right)\lambda^2\left(\theta'\right)}.$$

Supposing that $\lambda\left(\theta\right)$ was written as

$$\lambda\left(\theta\right)=\frac{\phi\left(\theta\right)}{f\left(\theta\right)} \tag{17.9}$$

4 (N. H. Abel, 1829d).

5 (Abel→Legendre, Christiania, 1828/11/25. N. H. Abel, 1902a, 82).

for some functions ϕ and f, ABEL obtained

$$\frac{\phi(\theta'+\theta)\,\phi(\theta'-\theta)}{f(\theta'+\theta)\,f(\theta'-\theta)}=\frac{\frac{\phi^2(\theta')}{f^2(\theta')}-\frac{\phi^2(\theta)}{f^2(\theta)}}{1-c^2\frac{\phi^2(\theta)\phi^2(\theta')}{f^2(\theta)f^2(\theta')}}=\frac{\phi^2(\theta')\,f^2(\theta)-\phi^2(\theta)\,f^2(\theta')}{f^2(\theta)\,f^2(\theta')-c^2\phi^2(\theta)\,\phi^2(\theta')}$$

and by comparing numerators and denominators, the functional equations (17.8) result with the exception of the change of sign in the numerator.[6] This is evidently a mistake in ABEL'S paper — repeated in the *Œuvres* — as can be seen by setting $\theta = 0$ and observing that because $\lambda(0) = 0$, $\phi(0)$ must also be zero.

Except for this small error, this part of the argument is completely straight-forward and fits well with ABEL'S other manipulations and the formulae which he presented in the *Précis*. In order to obtain the series expansions, we may get a clue from one of ABEL'S notebooks in which he proceeded to differentiate the equations (17.8).[7]

Coefficients of ϕ and f. First, we observe from the second functional equation by letting $\theta = \theta' = 0$ that

$$f^2(0) = f^4(0)\,,\text{ i.e. } f^2(0) = 1.$$

To be precise, $f(0) = 0$ was also a possibility but that would produce

$$f^2(x) = 0$$

for all x which is not a relevant case.

In the functional equations, letting $\theta = 0$ gives

$$\phi(\theta')\,\phi(-\theta') = -\phi^2(\theta')\,f^2(0)\,,\text{ i.e. } \phi(-\theta') = -\phi(\theta')\,,$$

and ϕ is therefore an odd function. Thus, the coefficients of all odd powers in a power series expansion must be zero. The same argument applied to the second functional equation produces

$$f(\theta')\,f(-\theta') = f^2(\theta')$$

and therefore, f is an even function.

Now, a few more coefficients may be determined, for instance $f(0)$ and $f''(0)$ which can be found by differentiating the relation

$$f(2\theta) = f^4(\theta) - c^2\phi^4(\theta)$$

twice and letting $\theta = 0$ to obtain $f^3(0) = 1$ and by the previous result, $f(0) = 1$.

To determine $f''(0)$ we differentiate again and insert $\theta = 0$ to obtain

$$f''(0) = 0.$$

6 As mentioned above.

7 (Abel, MS:351:C, 193), see also (N. H. Abel, 1881, II, 319).

The final coefficient which ABEL specified was $\phi'(0)$. It may be found directly by differentiation of (17.9)

$$\phi'(0) = \frac{d}{dx}\lambda(x) f(x)\bigg|_{x=0} = \lambda'(0) f(0) = 1.$$

Thus, summarizing the results, ABEL could claim by the use of an expansion in two *Maclaurin series* and detailed studies of the differential quotients that

$$\lambda(\theta) = \frac{\sum_{n=0}^{\infty} a_n \theta^{2n+1}}{\sum_{n=0}^{\infty} b_n \theta^{2n}}$$

with the further information

$$a_0 = 1, b_0 = 1, \text{ and } b_1 = 0.$$

In the letter to LEGENDRE, ABEL claimed that the coefficients were always polynomial functions of the modulus c^2. This claim can also be seen to be an easy consequence of the defining functional equations and can be proved by induction.

Convergence of ϕ and f. Furthermore, ABEL claimed — both in the *Précis* and in the letter — that the two series of (17.6) were always convergent. This is perhaps the most difficult of ABEL'S claims to obtain from the approach presented above. Furthermore, there are no hints of ABEL'S reasoning left neither in the notebooks, nor in his publications. He may have obtained some sort of indication of convergence from the approach described above, or he may simply have stated the convergence of the *Maclaurin series* as an unproven fact. It is, of course, also possible if not probable that ABEL had actually grasped an implicit concept of meromorphic functions as generalized rational functions.

Later, the expression of elliptic functions as the ratio of convergent power series was exactly the point which provoked K. T. W. WEIERSTRASS' (1815–1897) interest in research mathematics and — according to WEIERSTRASS, himself — convinced him that he wanted to become a mathematician.[8] WEIERSTRASS' approach centered on differential equations and contained a theorem explicitly stating the convergence of the power series equivalent to ϕ and f.

Further coefficients of ϕ and f. Besides the coefficients which were determined in the paper, ABEL'S manuscript also contains the series including the coefficients $a_1 = -\frac{1+c^2}{6}$ and $b_2 = -\frac{c^2}{12}$. On the same sheet of the notebook, the differential quotients $f'(x) = \frac{c^2}{3}x^3$, $f''(x) = -c^2x^2$, and $f'''(x) = -2c^2x$ can also be found which are only correct provided terms with higher powers of x have been neglected, e.g. by considering the local behaviour for small values of x. A further differentiation produces

$$f^{(4)}(0) = -2c^2$$

8 (Weierstrass→Lie, Berlin, 1882/04/10. N. H. Abel, 1902b, 104).

and

$$b_2 = \frac{f^{(4)}(0)}{4!} = -\frac{2c^2}{4!} = -\frac{c^2}{12}.$$

Thus, the calculations appear to be probes of ABEL'S result. The notebook does not contradict my interpretation, I believe, that ABEL obtained his representation of λ by means of two *Maclaurin series*. However, as LIE'S interpretation also illustrates, ABEL'S publications on the subject are too few and his notes to difficult to interpret to present any final opinion on the debate, in particular concerning the convergence of the series.

17.3 Characterization of ABEL's representations

Having presented an outline and discussion of the technical details of ABEL'S derivation of the representations, a few themes are summarized which will also be relevant to the discussion in subsequent chapters in part IV and in section 21.2.

17.3.1 ABEL's style of reasoning.

Characteristic of ABEL'S style in the *Recherches*, the derivation of the infinite representations such as (17.5) relies extensively on manipulations of formulae and is repeated afterwards for the functions f and F, although the arguments are highly similar. On a related theme, ABEL introduced symbols to denote most of his auxiliary and intermediate calculations. These facts are textual evidence of the formula-manipulating style in which ABEL'S *Recherches* is mainly written.

Brief comparison with L. EULER (1707–1783). As described, ABEL'S way of obtaining infinite expressions for the elliptic function ϕ closely resembles EULER'S methods. ABEL transformed an expression for $\phi(\alpha)$ into a form which contained a number (related to) n terms and then proceeded to split this expression into the sum of a part independent of n and a part which vanished with increasing values of n.

This is comparable to EULER'S derivation of the power series expansion of the exponential function in the *Introductio ad analysin infinitorum*.[9] There, EULER — cloaked in his language of infinitesimals — considered

$$\left(1+\frac{x}{n}\right)^n, \tag{17.10}$$

expanded it by the binomial formula, and let n grow to infinity to get an infinite number of terms. Simultaneously, this turned the expression into $\exp x$.

Compared to EULER'S expansion, ABEL'S original formula was valid for any value of n. The number n was only an auxiliary quantity later to disappear when he found limit expressions which were independent of n.

9 (L. Euler, 1748, 85–87).

Convergence of infinite expressions. As LIE commented in the notes, ABEL'S "methods in obtaining expressions for the functions $\phi(\alpha)$, $f(\alpha)$, $F(\alpha)$ in series and infinite products do not seem to us to be satisfactory in all details."[10] In particular, it is remarkable that the ardent rigorist of the binomial paper never even mentioned the word *convergence* in the *Recherches*. In the *Précis*, however, the convergence of the series (17.6) was stated as a fact without further explanation.

ABEL'S way of obtaining series expansions of elliptic functions essentially let the number of terms in a finite expression grow to infinity. ABEL'S concern for the convergence of the process was dealt with by the method of writing the expression as one part which was independent of n and another part which vanished with increasing n.

Thus, it appears, two different standards of rigor in the theory of series were involved in ABEL'S research using infinite series. In foundational issues, a strict adherence to A.-L. CAUCHY'S (1789–1857) program and the associated theoretical complex was advocated by ABEL. However, when it came to research on new groundbreaking objects, ABEL used the methods which he had learned from EULER and was content with observing that his results were sound.

17.3.2 The need for multiple representations

A final aspect which is revealing of the role played by representations of elliptic functions in ABEL'S works is the necessity of obtaining multiple representations. Understandably, functions introduced in such an indirect way as the inversion of a non-elementary integral needed some other means of numerical determination and this is one of the roles played by representations in ABEL'S theory.

Convergence. As already noticed repeatedly, ABEL was not very explicit about the convergence of his infinite representations. This may have been a reason for not relying on any single representation but deriving multiple and various representations in the hope that at least some of them would prove adequate in particular instances.

Applications. A connected motivation for multiple representations could also be the ambition of multiple applications (within pure mathematics). In the following chapter, an instance where infinite representations play a central role in the proof of a theorem will be described. ABEL and his contemporaries would have hoped and expected to find many similar instances.

Aesthetics. The third aspect of the discussion is less technical and more of a personal and contextual nature which can only be appreciated in a broader time scale. In section 21.2, where the discussion of representations is taken up again, it will also become

10 (N. H. Abel, 1881, II, 306).

clear that the preferred definitions and representation vary over time and between mathematical traditions.

In ABEL'S theory of elliptic functions, the objects were introduced as inversions of elliptic integrals. The largely Eulerian tradition to which ABEL'S *Recherches* generally belongs emphasized representations of transcendental functions by infinite (power) series and infinite products. On the way to obtaining these representations, ABEL also came across the functional equations (17.8) and proved that elliptic functions were doubly periodic and could be written as the ratio of power series. All these representations and key results were later assumed as definitions of the concept *elliptic function* depending on the setting and context in which they were introduced.

17.4 Conclusion

One major achievement of the search for representations was, of course, that based on formulae such as (17.5), approximations to $\phi(\alpha)$ could be computed with any degree of accuracy. Another, and equally interesting — but less anticipated — result was that infinite expressions, themselves, could play a role in the development of the theory. In the *Recherches*, this aspect remained little cultivated but in subsequent papers, ABEL occasionally applied infinite expressions, even to answer algebraic questions (see next chapter).

In the *Recherches*, ABEL did more than solve the division problem for the lemniscate. While the lemniscate provided a clear question to which he produced a clear answer, the other part of the paper dealt with problems of more intrinsic nature and provided answers which are now hardly recognizable as answers because the questions which they answer have faded in importance.

Chapter 18

Tools in ABEL's research on elliptic transcendentals

In order to illustrate how N. H. ABEL (1802–1829) worked with elliptic transcendentals, the two most important topics are described in some details below. In order to make the presentation coherent, special emphasis is given to some of ABEL'S papers which illustrate important points concerning the types of tools involved by ABEL in his research on elliptic transcendentals.

18.1 Transformation theory

With A.-M. LEGENDRE'S (1752–1833) systematization of the theory of elliptic integrals, the transformations which he devised to reduce one integral to the simplest one in its class were powerful and important tools. ABEL approached the theory of transformations in his *Recherches* but his main contributions were spelled out in a number of articles written as a reaction to results announced by C. G. J. JACOBI (1804–1851) in the journal *Astronomische Nachrichten*.

Competition with JACOBI. An important theme in a large part of the ABEL-related research has been his rivalry with the contemporary German mathematician JACOBI.[1] As noted in connection with the idea of inverting elliptic integrals into elliptic functions, ABEL realized that he was in the middle of a rivalry when he became aware of JACOBI'S published announcements in H. C. SCHUMACHER'S (1784–1873) *Astronomische Nachrichten*.[2] The present section describes ABEL'S contribution to the theory in which JACOBI had initially been the most interested. By studying ABEL'S technical machinery in some details, his means of dealing with elliptic functions become clearer. Most interestingly, ABEL employed largely *algebraic methods* in his research

1 See also chapter 2 and section 16.2.5.

2 In a letter to SCHUMACHER, HANSTEEN described how ABEL, upon learning of these papers, became very pale and had to rush to the nearest bakery for a dram; see (Holst, 1902, 89) and (E. Andersen, 1975, 104).

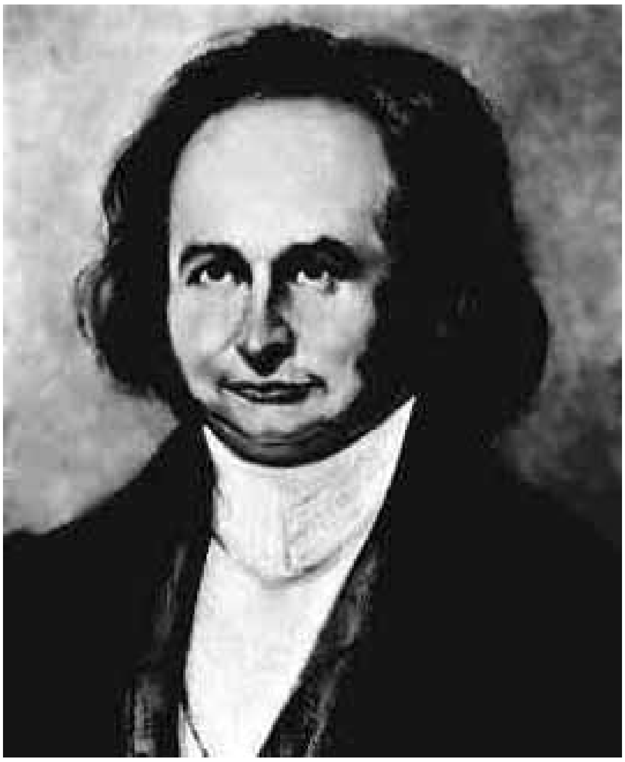

Figure 18.1: CARL GUSTAV JACOB JACOBI (1804–1851)

but also put the machinery of infinite representations to good use in obtaining an important theorem in transformation theory. After a spell of shorter papers which complemented his two major papers, the *Recherches* and the *Précis*,[3] ABEL left the theory of transformations alone — and he soon died, of course. Transformation theory was not ABEL'S major purpose but it was a topic which provoked the interest of some of his most important contemporaries.

In his announcements in the *Astronomische Nachrichten*, JACOBI communicated only the results and not the methods which he had employed to deduce them. The first biographies of ABEL have invested an effort in calling attention to ABEL'S priority in proving the results. In particular, C. A. BJERKNES (1825–1903) responded to L. KÖNIGSBERGER'S (1837–1921) account of the discovery of elliptic functions by arguing that JACOBI only obtained his proofs after learning of ABEL'S inversion.[4] However, since the turn of the twentieth century, historians and mathematicians have agreed that although JACOBI initially obtained his results through an ingenious but unrigorous heuristic, he probably developed the inversion of elliptic integrals on his own, possibly inspired by reading through ABEL'S *Recherches*.[5] To indicate the hectic character of the events, some key dates of the rivalry have been presented in table 18.1.

3 (N. H. Abel, 1827b; N. H. Abel, 1828b) and (N. H. Abel, 1829d), respectively.
4 (Bjerknes, 1880; Koenigsberger, 1879).
5 (Mittag-Leffler, 1907; Ore, 1954; Pieper, 1998).

The central question of transformation theory. ABEL picked up the theme of transformations of elliptic integrals from LEGENDRE. In ABEL'S words, the central problem was posed with variations on multiple occasions, for instance in the following way:

> "To find all the possible cases in which one can satisfy the differential equation
>
> $$\frac{dy}{\sqrt{(1-y^2)\left(1-c_1^2y^2\right)}} = a\frac{dx}{\sqrt{(1-x^2)\left(1-c^2x^2\right)}}$$
>
> by an *algebraic equation* between the variables x and y and supposing the moduli c and c_1 less than unity and the coefficient a either real or imaginary."[6]

Expressed in the notation of LEGENDRE'S differentials, the above question asks for every possible way of transforming x algebraically into y in such a way that the integral with modulus c_1 transformed into the integral with modulus c.

18.1.1 ABEL's response to JACOBI's announcements

ABEL first published in the *Astronomische Nachrichten* in 1828;[7] in a lengthy paper, he demonstrated how the theory which he had developed in his *Recherches* could answer a question raised by JACOBI. In the paper — which is entitled *Solution d'un problème général concernant la transformation des fonctions elliptiques* —,[8] ABEL began by describing key results concerning the inverse of elliptic integrals deduced in the *Recherches*. With the notation

$$\Delta(x) = \sqrt{(1-c^2x^2)\left(1-e^2x^2\right)}$$

and the inversion

$$\theta = \int_0 \frac{dx}{\Delta(x)} \text{ and } x = \lambda(\theta),$$

ABEL presented the highlights of the *Recherches* in two theorems which summarize (16.3) and (16.8), respectively. First, he expressed the addition theorem for the elliptic function of the first kind λ,

$$\lambda\left(\theta \pm \theta'\right) = \frac{\lambda(\theta)\,\Delta(\theta') \pm \lambda(\theta')\,\Delta(\theta)}{1-c^2e^2\lambda^2(\theta)\,\lambda^2(\theta')}.$$

Second, ABEL described the conditions on the arguments which ensured that the function took identical values,

$$\lambda(\theta) = \lambda\left(\theta'\right) \text{ if and only if } \theta' = (-1)^{m+m'}\theta + m\omega + m'\omega' \tag{18.1}$$

---

6 "*Trouver tous les cas possibles où l'on pourra satisfaire à l'équation différentielle:*

$$\frac{dy}{\sqrt{(1-y^2)\left(1-c_1^2y^2\right)}} = a\frac{dx}{\sqrt{(1-x^2)\left(1-c^2x^2\right)}}$$

par une équation algébrique *entre les variables x et y, en supposant les modules c et c_1 moindre que l'unité et le coeffcient a réel ou imaginaire.*" (N. H. Abel, 1829a, 33).

7 (N. H. Abel, 1828d). The paper is dated 27 May 1828.

8 (ibid.).

in which the semi-periods were determined by

$$\frac{\omega}{2} = \theta\left(\frac{1}{c}\right) \text{ and } \frac{\omega'}{2} = \theta\left(\frac{1}{e}\right).$$

ABEL expressed the central properties of this theorem:

> "This theorem is generally valid no matter whether the quantities e and c are real or imaginary. In the paper cited above [*Recherches*], I have proved it in the case where e^2 is negative and c^2 is positive. [...] The quantities ω, ω' always have an imaginary ratio. Otherwise, they have the same role in the theory of elliptic functions as the number π has in the theory of circular functions."[9]

Now, in order to address the transformation problem, ABEL observed that the method of indeterminate coefficients could be applied. This method amounted to approaching the problem by introducing two power series with indeterminate coefficients and using the defining equations to obtain relations among the terms. However, as ABEL critically remarked, this method would lead to extremely cumbersome calculations, and ABEL proposed a simpler and more direct one. Below, this method is briefly described.

Rational transformations. With the notation and basic results set up, ABEL turned to a question which he proposed and ascribed great importance for the theory of elliptic functions. He was interested in finding all the possible ways in which the differential equation

$$\frac{dy}{\sqrt{\left(1-c_1^2y^2\right)\left(1-e_1^2y^2\right)}} = \pm a\frac{dx}{\sqrt{\left(1-c^2x^2\right)\left(1-e^2x^2\right)}} \tag{18.2}$$

could be satisfied in which y was an algebraic function of x. In the paper, ABEL limited his considerations to rational functions $y = \psi(x)$ because the general question "at first seems too difficult".[10]

ABEL'S first result in this situation was an algebraic one, not so different from results obtained in his paper on *Abelian equations*.[11] By a string of manipulations, ABEL found that if the equation (18.2) was satisfied, the roots of the equation $\psi(x) = y$ had the remarkable property of being related in a very specific way: if $\lambda(\theta)$ represented one of the roots, any other root of the equation would be representable as $\lambda(\theta+\alpha)$ where α was a constant, i.e.

$$y = \psi(\lambda(\theta)) = \psi(\lambda(\theta+\alpha)).$$

9 *"Ce théorème a lieu généralement, quelles que soient les quantités e et c, réelles ou imagainaires. Je l'ai démontré pour le cas où e^2 est négatif et c^2 positif dans le mémoire cité plus haut [...]. Les quantités ω, ω' sont toujours dans un rapport imaginaire. Elles jouent d'ailleurs dans la théorie des fonctions elliptiques le même rôle que le nombre π dans celle des fonctions circulaires."* (N. H. Abel, 1828d, 366).

10 (ibid., 365).

11 See chapter 7.

Obviously, only finitely many roots could exist, and the value of α could be obtained from (18.1),

$$\alpha = \mu\omega + \mu'\omega' \tag{18.3}$$

in which μ, μ' were rational constants. However, the degree of the equation $y = \psi(x)$ might surpass the number of different values produced in this fashion and a new group corresponding to a new value α_2 of α might be necessary. ABEL found that a certain number ν (which he did not describe in any detail) existed such that all the roots of the equation $y = \psi(x)$ would be representable by the different values of the expression

$$\lambda\left(\theta + \sum_{n=1}^{\nu} k_n\alpha_n\right)$$

when $k_1, \ldots, k_\nu$ took all integer values. However, the different values could also be represented as (possibly changing the values of $\alpha_1, \alpha_2, \ldots$)

$$\lambda(\theta)\,,\ \lambda(\theta+\alpha_1)\,,\ \ldots,\ \lambda(\theta+\alpha_{m-1})$$

in which $\alpha_1, \ldots, \alpha_{m-1}$ were still rational linear combinations of ω and ω' (as in 18.3). ABEL then wrote the rational function $\psi(x) = \frac{p(x)}{q(x)}$ with no common divisors and obtained[12]

$$p(x) - q(x)\,y = A\prod_{n=0}^{m-1}\left(x - \lambda(\theta+\alpha_n)\right).$$

The constant A was of the form $A = f - gy$ with f, g constants. ABEL'S next step was to find an expression for A and he did so by first imposing a limiting assumption and gradually relaxing it. First, ABEL considered the case in which both p and q were polynomials of the first degree. In this case (e.g. by Euclidean division),

$$y = \frac{f' + fx}{g' + gx},$$

and ABEL found

$$dy = \frac{fg' - f'g}{(g'+gx)^2}\,dx.$$

When he inserted this into the original differential equation, its dependence on dy disappeared. Consequently, ABEL could conclude that the differential equation in this case implied either of the three solutions

$$
\begin{array}{lll}
\text{I.}\, y = ax, & c_1^2 = \dfrac{c^2}{a^2}, e_1^2 = \dfrac{e^2}{a^2}, \text{ or} & \\[2ex]
\text{II.}\, y = \dfrac{a}{ec}\dfrac{1}{x}, & c_1^2 = \dfrac{c^2}{a^2}, e_1^2 = \dfrac{e^2}{a^2}, \text{ or} & \\[2ex]
\text{III.}\, y = m\dfrac{1 - x\sqrt{ec}}{1 + x\sqrt{ec}}, & c_1 = \dfrac{1}{m}\dfrac{\sqrt{c}-\sqrt{e}}{\sqrt{c}+\sqrt{e}}, e_1 = \dfrac{1}{m}\dfrac{\sqrt{c}+\sqrt{e}}{\sqrt{c}-\sqrt{e}}, & a = \dfrac{im}{2}(c-e)\,.
\end{array}
$$

12 Here I write $\alpha_0 = 0$ for brevity.

Next, ABEL returned to the results which he had previously established and now let f' and g' denote the coefficients of x^{m-1} in p and q. Then, by comparing the coefficients, ABEL found

$$f' - g'y = -(f - gy)\underbrace{\sum_{n=0}^{m-1} \lambda(\theta + \alpha_n)}_{=\phi(\theta)}.$$

Consequently, when he isolated y, ABEL found

$$y = \frac{f' + f\phi(\theta)}{g' + g\phi(\theta)}$$

which would serve to determine y as a function of x in all but those particular cases where $\phi(\theta)$ reduced to a constant.

In order for y to be a rational function of x, ABEL observed that the new function $\phi(\theta)$ must likewise be rational in x. ABEL set out to investigate the circumstances under which this would be the case. By computing and combining values of the elliptic function λ, he found that $\phi(\theta)$ was *always* a rational function of x and that it was given by

$$\phi(\theta) = (1-k)x + \frac{k'' - k'}{ec}\frac{1}{x} + \sum_{n=1}^{m-1} \frac{2x\Delta(\alpha_n)}{1 - e^2c^2\lambda^2(\alpha_n)x^2}$$

in which k, k', k'' were constants which were either zero or one. Based on this representation, ABEL separated three cases corresponding to various combinations of the values of k, k', k''.

In the end, after his technical manipulations in the various cases, ABEL found that the differential equation under consideration

$$\frac{dy}{\sqrt{(1-y^2)(1-e_1^2y^2)}} = \pm\frac{a\,dx}{\sqrt{(1-x^2)(1-e^2x^2)}} = \pm a\,d\theta$$

was satisfied precisely if a, e_1, and y were given by

$$a = k\prod_{m=1}^{n-1}\lambda\left(\frac{m}{n}\omega\right), y = k\prod_{m=0}^{n-1}\lambda\left(\theta + \frac{m\omega}{n}\right), \text{and}$$

$$e_1 = e^n\left(\prod_{m=0}^{n-1}\lambda\left(\frac{2m+1}{2n}\omega\right)\right)^2$$

where n was an arbitrary integer and the constants k and ω were given by

$$1 = k\prod_{m=0}^{n-1}\lambda\left(\frac{2m+1}{2n}\omega\right) \text{ and } \frac{\omega}{2} = \int_0^1 \frac{dx}{\sqrt{(1-x^2)(1-e^2x^2)}}.$$

Following this characterization of the solutions to the problem, ABEL first translated the result into the trigonometric language employed by LEGENDRE and then announced a number of "remarkable theorems on elliptic functions"[13] which are of less relevance in the present context.

[13] (N. H. Abel, 1828d, 385).

Summary: an algebraic proof. As described, ABEL'S deduction consisted of five steps: First, ABEL set up his notation and definitions and introduced important results from the *Recherches*. Second, ABEL found that if $\lambda(\theta(x))$ is a root, i.e. if $y = \psi(\lambda(\theta(x)))$, then any other root has the form $\lambda(\theta(x)+\alpha)$. Next, the constant α could be determined and a general representation of the roots can be given — possibly involving multiple "orbits" corresponding to $\alpha_1, \alpha_2, \ldots$. Fourth, the relation between y and x could be spelled out. Eventually, it could be necessary to consider a number of cases in order to describe these relations and deduce formulae of particular interest.

A few broader points should also be observed. First, ABEL'S proof made central use of the properties of elliptic *functions* which had been deduced in the *Recherches*. In particular, the solution of the equation $\lambda(x) = \lambda(y)$ — which originated from the double periodicity of the function λ — became very instrumental in the present context just as it had been in the solution of the division problem (see section 16.3). Second, the approach which ABEL took may well be described as an *algebraic* one; it relied on algebraic tools such as specific knowledge of the roots of certain polynomial equations, division of polynomials, and considerations of the rationality of certain functions. These are tools which were also present in ABEL'S purely algebraic researches on solubility (see part II). However, ABEL also adopted another approach to the same question.

Counting the possible numbers of transformations. In another paper — this time published in CRELLE'S *Journal* and motivated by another of JACOBI'S papers —,[14] ABEL gave the theory of transformation a slightly different turn. He continued the path laid out in the *Astronomische Nachrichten* and made frequent references to the paper described above, but in the *Journal*, ABEL wanted to count and enumerate the different transformations. ABEL considered a rational transformation of x into y of a certain prime degree $2n+1$ and found — by employing algebraic tools similar to those described above — that $12(2n+2)$ different transformations corresponding to $12(n+1)$ different values of the transformed modulus were generally possible. ABEL remarked that for certain particular values of the modulus c, the number of transformations might degenerate. This notion of arguments carried out "in general" will be discussed further in section 19.3 and chapter 21.

ABEL'S determination of the number of transformations spurred a reaction from LEGENDRE who believed that it was at odds with JACOBI'S determination of the degree of the so-called *modular equation*. JACOBI had claimed that for a transformation of prime degree $2n+1$, $2n+2$ values of the transformed modulus were possible. Thus, ABEL'S value was six times JACOBI'S number of transformations. However, as ABEL argued in a letter to LEGENDRE, JACOBI had indeed solved an equation with $2n+2$ different roots but each root of this equation could also produce five other values for

14 (N. H. Abel, 1828e); JACOBI'S paper is (C. G. J. Jacobi, 1828).

the transformed modulus and ABEL'S result was valid.[15]

18.1.2 An additional note

In a subsequent issue of the *Astronomische Nachrichten*, ABEL inserted a note which added a different deduction of the main result of the previous one.[16] Whereas ABEL had initially employed direct and detailed manipulations to obtain the characterization of transformations, he now used one of the infinite representations which he had also obtained in the *Recherches*.

From the *Recherches*, ABEL imported the expansion of the auxiliary function f in an infinite product and manipulated it into the current setting in which it produced

$$\lambda(\alpha) = A \times \psi\left(\alpha\frac{\pi}{\bar{\omega}}\right) \times \prod_{n=1}^{\infty}\left(\psi\left((n\omega+\alpha)\frac{\pi}{\bar{\omega}}\right)\psi\left((n\omega-\alpha)\frac{\pi}{\bar{\omega}}\right)\right)$$

with A a constant and

$$\psi(x) = \frac{1-e^{-2x}}{1+e^{-2x}}.$$

Furthermore, the periods ω and $\bar{\omega}$ were also related in ABEL'S usual way to the modulus c. ABEL now inserted $\alpha = \theta + \frac{m}{n}$ for $m = 0, 1, \ldots, n-1$ and multiplied these expressions together to obtain the central formula

$$\prod_{m=0}^{n-1}\lambda\left(\theta+\frac{m\omega}{n}\right) = A^n\psi\left(\delta\frac{\pi}{\bar{\omega}_1}\right)\prod_{m=0}^{\infty}\left(\psi\left((\omega_1+\delta)\frac{\pi}{\bar{\omega}_1}\right)\psi(\omega_1-\delta)\frac{\pi}{\bar{\omega}_1}\right)$$

with

$$\delta = \frac{\bar{\omega}_1}{\bar{\omega}}\theta \text{ and } \frac{\omega_1}{\bar{\omega}_1} = \frac{1}{n}\frac{\omega}{\bar{\omega}}. \tag{18.4}$$

The important idea which ABEL utilized now was to relate this formula to the similar one for the transformed function λ' corresponding to the modulus c_1, the periods $\bar{\omega}$ and $\bar{\omega}_1$, and the constant A_1. ABEL found that

$$\lambda'\left(\frac{\bar{\omega}_1}{\bar{\omega}}\theta\right) = \frac{A_1}{A^n}\prod_{m=0}^{n-1}\lambda\left(\theta+\frac{m\omega}{n}\right)$$

whenever the modulus c_1 was such that the periods were related by (18.4).

This time, we see how the knowledge of an infinite representation of the associated function f helped ABEL make statements about the transformation of elliptic functions. This is particularly interesting because it illustrates a different approach from the more algebraic one which he had initially taken. With direct access to a representation of the function f, ABEL could employ a mixture of finite and infinite results to obtain he characterization of the conditions of rational transformations.

15 (Abel→Legendre, Christiania, 1828/11/25. N. H. Abel, 1902a, 79).
16 (N. H. Abel, 1829a).

18.2 Integration in logarithmic terms

Another problem which figures significantly in ABEL'S approach to and research on higher transcendentals was the question of integration in more elementary forms. In chapter 15, it was described how mathematicians attacked the study of elliptic integrals although these were non-elementary. One of the approaches adopted was to relate a number of elliptic integrals by elementary functions or to investigate situations in which the integration could indeed be effected in elementary (or finite) terms. A similar idea was pursued by ABEL in his investigations on what he called "theory of integration". Thus, ABEL'S understanding of this notion differed from the present one in the sense that it was highly formal or algebraic and did not concern a numerical interpretation of the integral. Such an interpretation was, of course, part of A.-L. CAUCHY'S (1789–1857) complete program of rigorization and became very important in the 19th century mainly in the efforts to answer the challenges raised by Fourier series.

Reminiscences of the *Collegium mémoire*. The first evidence of ABEL'S interest in the theory of integration (in finite terms) originates from descriptions of a paper which is no longer extant. As was already described in section 2.3, ABEL had hoped to embark on his European Tour shortly after the application was sent to the Collegium of the University in 1824. Before that time, in March 1823, ABEL presented a manuscript to the *Collegium academicum* through C. HANSTEEN (1784–1873). It concerned "a general presentation of the possibility of integrating all possible differential formulae"[17]. The manuscript was given to professors HANSTEEN and S. RASMUSSEN (1768–1850) for their professional evaluation. Their review was positive but no means of publishing the paper were at hand and it was subsequently lost. However, from ABEL'S published research, we may get an impression of what it could have contained. ABEL'S notebooks contain a number of entries related to the question of integration in finite terms; in particular, a manuscript for a large memoir on the theory of elliptic transcendentals from this perspective has been included in the *Œuvres*.[18] Nevertheless, the present description focuses on his main publication on the subject which occurred in the *Journal* in 1826.[19]

The local context of ABEL'S work on integration in finite terms was mainly related to the same theme as his research in the *Paris memoir* (see chapter 19, below). However, it also included such issues as the reduction of all elliptic integrals to four basic kinds which ABEL undertook in his manuscripts and which had been a corner stone of LEGENDRE'S theory of elliptic integrals.[20] In the 1840s, mainly through the works

17 "*en almindelig Fremstilling af Muligheden at integrere alle mulige Differential-Formler*" (N. H. Abel, 1902d, 4).
18 (N. H. Abel, [1825] 1839b).
19 (N. H. Abel, 1826d).
20 (N. H. Abel, [1825] 1839b, 101); for LEGENDRE'S theory, see section 15.3.

of J. LIOUVILLE (1809–1882), the theory of integration in finite terms established itself as an independent theory investigated for its own results. In this context, the theory and ABEL'S contribution to it have been well described in J. LÜTZEN'S biography of LIOUVILLE.[21] Referring to LÜTZEN'S description, a presentation of ABEL'S argument and a brief discussion of relevant points of ABEL'S contribution are included below.

18.2.1 Characterization by continued fractions

In the paper from 1826,[22] ABEL investigated conditions under which the integral

$$\int \frac{\rho\, dx}{\sqrt{R}} \tag{18.5}$$

could be reduced to the logarithmic expression

$$\log \frac{p + q\sqrt{R}}{p - q\sqrt{R}}. \tag{18.6}$$

The article first dealt with this question of reduction, but as ABEL ultimately noticed, the answer obtained was actually the answer to a more general question. ABEL noted that in case the integral (18.5) could be represented by logarithmic functions in *any* way, it would always have a representation of the form (18.6). ABEL promised a proof of this assertion but never published one; it was eventually given by P. L. CHEBYSHEV (1821–1894).[23]

A non-empty class. ABEL found by direct differentiation that for

$$z = \log \frac{p + q\sqrt{R}}{p - q\sqrt{R}},$$

he would have

$$dz = \frac{pq\, dR + 2\,(p\, dq - q\, dp)\, R}{(p^2 - q^2 R)\,\sqrt{R}}.$$

Writing dz in the form

$$dz = \frac{M\, dx}{N\sqrt{R}} \text{ with}$$

$$M = pq\,\frac{dR}{dx} + 2\left(p\frac{dq}{dx} - q\frac{dp}{dx}\right) R \text{ and} \tag{18.7}$$

$$N = p^2 - q^2 R, \tag{18.8}$$

he had thus found that for such values of M and N,

$$\int \frac{M\, dx}{N\sqrt{R}} = \log \frac{p + q\sqrt{R}}{p - q\sqrt{R}}.$$

ABEL concluded:

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21 (Lützen, 1990, chapter IX).
22 (N. H. Abel, 1826d).
23 (Chebyshev, 1853).

"From this it follows, that in the differential $\frac{\rho\,dx}{\sqrt{R}}$ an infinitude of rational functions ρ can be found which make this differential integrable by logarithms; furthermore, this is done by an expression of the form $\log\left(\frac{p+q\sqrt{R}}{p-q\sqrt{R}}\right)$."[24]

Thus, ABEL had proved that the class of differentials which were integrable by logarithms was non-empty.

Delineation of the class. It was the converse of this result, that ABEL really wanted to investigate in the paper published in CRELLE'S *Journal*. He formulated the problem of determining *all* differentials of the form $\frac{\rho\,dx}{\sqrt{R}}$ which could be integrated in the logarithmic form

$$\log \frac{p+q\sqrt{R}}{p-q\sqrt{R}}. \tag{18.9}$$

This problem can be interpreted as another instance of a *problem of delineation* for a class of objects, in this case the class of objects integrable in the logarithmic form (18.9).[25]

With $\rho = \frac{M}{N}$ an entire function, ABEL took similar steps as above (18.7 and 18.8) and found the relation

$$\frac{M}{N} = \frac{2\frac{dp}{dx} - p\frac{dN}{N\,dx}}{q}.$$

Then followed a series of reductions to obtain a simple description of the relations between M, N, and R. Because $\frac{M}{N}$ was an entire function of x, it followed from this equation that $p\frac{dN}{N\,dx}$ was also an entire function of x,

$$N = \prod_{k=0}^{n} (x+a_k)^{m_k}.$$

Reduction into partial fractions implied that

$$\frac{dN}{N\,dx} = \sum_{k=0}^{n} \frac{m_k}{x+a_k}$$

and because $p\frac{dN}{N\,dx}$ was to be entire, ABEL could write

$$p = p_1 \prod_{k=0}^{n} (x+a_k)$$

in which p_1 was an entire function. As a consequence of the relation $N = p^2 - q^2R$, ABEL found

$$\underbrace{\prod_{k=0}^{n} (x+a_k)^{m_k}}_{=N} = \underbrace{p_1^2 \prod_{k=0}^{n} (a+a_k)^2}_{=p^2} - q^2 R$$

[24] "*Daraus folgt, daß sich in dem Differential* $\frac{\rho\,dx}{\sqrt{R}}$, *für die rationale Function* ρ *unzählige Formen finden lassen, die dieses Differential* durch Logarithmen *integrabel machen, und zwar durch einen Ausdruck von der Form* $\log\left(\frac{p+q\sqrt{R}}{p-q\sqrt{R}}\right)$." (N. H. Abel, 1826d, 186).

[25] See chapter 21.

and because R did not contain any square factors and p and q could be assumed relatively prime, ABEL found $m_0 = m_1 = \cdots = m_n = 1$ and obtained the factorization

$$R = R_1 \prod_{k=0}^{n} (x + a_k) = R_1 N.$$

with R_1 an entire function. With this result, ABEL had found a reduced characterization in the form

$$p_1^2 N - q^2 R_1 = 1 \text{ and } \frac{M}{N} = p_1 q \frac{dR}{dx} + 2\left(p\frac{dq}{dx} - q\frac{dp}{dx}\right) R_1.$$

Considerations of degrees. ABEL'S next step was to investigate the consequences of the first part of the characterization obtained above,

$$p_1^2 N - q^2 R_1 = 1. \tag{18.10}$$

As he remarked, the equation could be solved by the method of indeterminate coefficients but this approach would be extremely cumbersome and not lead to any general conclusion. Instead, he proposed a different approach. Before embarking on his novel approach, ABEL introduced the notations δP to denote the degree of the (rational) function P and EP to denote the *entire part* of P, i.e.

$$u = Eu + u' \text{ with } \delta u' < 0.$$

Judging from the detailed introduction of these concepts, ABEL did not assume them to be familiar to his readers. Concerning these new concepts, ABEL easily proved the following lemma:

Lemma 3 *If the functions u, v, z are related by*

$$u^2 = v^2 + z$$

and $\delta z < \delta v$, then

$$Eu = \pm Ev.$$

□

ABEL now returned to the equation $p_1^2 N - q^2 R_1 = 1$ and applied his new result (lemma 3). ABEL immediately obtained

$$\delta\left(p_1^2 N\right) = \delta\left(q^2 R_1\right)$$

and consequently

$$\begin{aligned} 2\delta p_1 + \delta N &= 2\delta q + \delta R_1, \text{ i.e.} \\ \delta(NR_1) &= 2(\delta q + \delta R_1 - \delta p_1) \end{aligned}$$

and because $NR_1 = R$, ABEL had found that the highest power in R had to be an even number. ABEL wrote $\delta N = n - m$ and $\delta R_1 = n + m$ and generalized the study of the equation (18.10) to the equation

$$p_1^2 N - q^2 R_1 = v \tag{18.11}$$

in which v was an entire function with $\delta v < \frac{\delta N + \delta R_1}{2} = n$.

ABEL then wrote

$$R_1 = Nt + t' \text{ with } t = E\frac{R_1}{N} \text{ and } \delta t' < 0.$$

As a consequence of the assumptions, ABEL found that $\delta t = 2m$ and he wrote t in the form

$$t = t_1^2 + t_1'$$

in which $\delta t_1' < m$. With these conventions, ABEL had remodelled the equation (18.11) into

$$\begin{aligned} v &= p_1^2 N - q^2 \left(Nt + t'\right) = \left(p_1^2 - q^2 t\right) N - q^2 t' \\ &= \left(p_1^2 - q^2 t_1^2\right) N - q^2 \left(t_1' N + t'\right). \end{aligned}$$

After rewriting the equation as

$$\left(\frac{p_1}{q}\right)^2 = t_1^2 + \frac{v}{Nq^2} + t_1' + \frac{t'}{N},$$

ABEL observed that

$$\delta\left(\frac{v}{Nq^2} + t_1' + \frac{t'}{N}\right) < m = \delta t_1,$$

and he applied lemma 3 to obtain

$$E\left(\frac{p_1}{q}\right) = \pm E t_1 = \pm t_1.$$

This meant, that ABEL had found a relation between p_1 and q of the form

$$p_1 = t_1 q + \beta \text{ in which } \delta\beta < \delta q.$$

Through a sequence of similar, very explicit manipulations, ABEL transformed the equation (18.11) into the form

$$s_1 \beta^2 - 2r_1 \beta\beta_1 - s\beta_1^2 = v \tag{18.12}$$

in which

$$\delta r_1 = \frac{1}{2}\delta R = n,\ \delta\beta_1 < \delta\beta,\ \delta s < n, \text{ and } \delta s_1 < n.$$

His investigations now turned toward solving the equation (18.12). ABEL did so by observing that the process used above could be iterated producing a sequence of relations similar to (18.12). After $n-1$ iterations, he found the relation

$$s_n\beta_{n-1}^2 - 2r_n\beta_{n-1}\beta_n - s_{n-1}\beta_n^2 = (-1)^{n-1}v,$$
$$\text{in which } \delta\beta_n < \delta\beta_{n-1}.$$

Because the sequence of degrees was decreasing, it would eventually produce $\delta\beta_m = 0$, i.e. $\beta_m = 0$, and the final relation would then become

$$s_m\beta_{m-1}^2 = (-1)^{m-1}v.$$

Using this information, ABEL ascended the chain of $\beta, \beta_1, \ldots, \beta_m$ in the reverse order each time finding expressions for β_{n-1} of the form

$$\beta_{n-1} = 2\mu_n\beta_n + \beta_{n+1}.$$

By solving these relations for the first term of the chain β, ABEL found an expression for $\frac{\beta}{\beta_1}$ as a finite continued fraction.

In order to answer the question of logarithmic integration of the original differential, ABEL next investigated the consequences for the radical $\sqrt{R}$. He found from his earlier results that by assuming m infinite, the expansion of $\sqrt{R}$ would be

$$\sqrt{R} = t_1 + \cfrac{1}{2\mu + \cfrac{1}{2\mu_1 + \cfrac{1}{2\mu_2 + \ldots}}}.$$

Here, ABEL noticed in a footnote that the equality of $\sqrt{R}$ and its continued fraction should not be interpreted as a numerical equality except in those situations where the continued fraction has a value.

Finally, ABEL translated an earlier assumption that one among the quantities $s_1, s_2, \ldots$ should be independent of x into the property that the continued fraction for $\sqrt{R}$ should be *periodic*. The assumption on $s_1, s_2, \ldots$ had been introduced to ensure the solubility of the equations, and it thus amounted to a criterion for the possibility of integrating $\frac{\rho\,dx}{\sqrt{R}}$ in logarithmic terms. ABEL summarized his investigations as a complete criterion of logarithmic integrability stating that for polynomials ρ, the integration

$$\int \frac{\rho\,dx}{\sqrt{R}} = \log\frac{y+\sqrt{R}}{y-\sqrt{R}} \tag{18.13}$$

could be effected if and only if the expansion of $\sqrt{R}$ into continued fractions was periodic. In the affirmative case, the function y was determined by the first period of the continued fraction for $\sqrt{R}$.

Summary. ABEL'S investigations concerning integration on the logarithmic form (18.13) serves to illustrate some interesting aspects. First, ABEL'S interest in the problem of integrating differentials in logarithmic forms reveals the position of his research within a tradition of reducing complicated integrals to simpler ones. At the same time, the way he attacked the problem was rather novel. In his approach, ABEL applied the program which he had presented in his notebook research on solubility of equations and did not search "by divination" for an integration of the specific form (see section 8.1). Instead, he took upon himself to establish the precise conditions under which the integration would be possible. Second, ABEL employed highly algebraic tools involving polynomials, degrees of rational functions, and considerations of dependencies among quantities to reach his conclusion. At the point, where his argument came to involve an infinite representation in the form of an expansion of $\sqrt{R}$ into a continued fraction, he stressed that the equality should be interpreted as a formal one suited for determining the involved quantities.

18.3 Conclusion

In the present chapter, two major examples of ABEL'S work with elliptic transcendentals have been described in order to illustrate the tools which he employed. In particular, ABEL'S recurring use of algebraic methods has been documented. This algebraic approach to the theory of higher transcendentals was a general theme in ABEL'S approach and it will be described further in the following chapter. At points where he involved infinite expressions, they were often regarded as algebraic equalities — the precise conditions for convergence were rarely addressed. However, as noted, infinite representations could sometimes improve the deductions considerably.

1827.06.12	JACOBI dated his first letter to SCHUMACHER
1827.08.02	JACOBI dated his second letter to SCHUMACHER
1827.09	Extracts from JACOBI'S two letters to SCHUMACHER were published in the *Astronomische Nachrichten* (C. G. J. Jacobi, 1827b).
1827.09.20	The issue of A. L. CRELLE'S (1780–1855) *Journal* containing the first part of ABEL'S *Recherches* appeared.
1827.11.18	JACOBI dated his *Demonstratio* which was published in the *Astronomische Nachrichten* (C. G. J. Jacobi, 1827a).
1828.01.25	JACOBI dated his one page addition to ABEL'S *Recherches*.
1828.02.12	ABEL sent the second part of the *Recherches* to CRELLE.
1828.04.02	JACOBI dated his first letter inserted in CRELLE'S *Journal*.
1828.05.26	The issue of CRELLE'S *Journal* with the second part of ABEL'S *Recherches* and its note reacting to JACOBI was published
1828.07.21	JACOBI dated his second letter inserted in CRELLE'S *Journal*.
1828.10.03	JACOBI dated his third letter inserted in CRELLE'S *Journal*.
1828.12.03	The issue of CRELLE'S *Journal* containing ABEL'S investigation on the number of transformations appeared.
1829.01.11	JACOBI dated his fourth and final letter inserted in CRELLE'S *Journal*.

Table 18.1: Important dates in the ABEL-JACOBI-rivalry

Chapter 19

The *Paris memoir*

N. H. ABEL'S (1802–1829) most famous result was first communicated in a paper which he delivered to the Parisian Academy of Science*Académie des Sciences* in 1826. The result which ABEL obtained in the so-called *Paris memoir* was a rather technical one which dealt with the integration of algebraic differentials.[1] In its original, it was formulated in the typical style of ABEL, his predecessors, and many of his contemporaries but during the century ABEL'S result was recast in a quite different language and in another mathematical structure.[2] In modern mathematics, ABEL'S result is typically considered a part of algebraic geometry; readers who wish to see a presentation of the result from such a modern perspective can consult e.g. (Shafarevich, 1974).

Besides presenting the main results, the present rendering of ABEL'S *Paris memoir* aims at describing the central tools which ABEL employed in his reasoning. The focus on tools facilitates a continuation of the comparison with the methods involved in ABEL'S purely algebraic works and also serves to support the discussion of the immediate reception of ABEL'S results taken up in section 19.5. ABEL'S arguments in the *Paris memoir* were conducted in a style heavily dependent on explicit manipulations of formulae. In section 19.5.1, his subsequent announcements of the main results and the much clearer sketches of proof contained therein are described and discussed.

19.1 ABEL's approach to the *Paris memoir*

ABEL'S *Paris memoir* represents a pivotal point in his mathematical production. Many investigations which ABEL had previously undertaken for their own sake and brought to interesting conclusions were surpassed by the main result of the *Paris memoir*. The *Paris memoir* was three-fold monumental: it was the culmination of a line of research which ABEL had undertaken for years, it contained the result which brought him widespread fame in the nineteenth century, and yet it provided this result with an incredibly long and cumbersome proof.

1 ABEL'S result is also discussed in e.g. (Cooke, 1989; J. Gray, 1992).
2 For a brief discussion of styles, see chapter 21.

19.1.1 Set to work in Paris

ABEL arrived in Paris on July 10, 1826. For quite some time, he had worked on the study of functions whose differentials satisfy certain algebraic conditions. In a letter to C. HANSTEEN (1784–1873) written shortly after his arrival, ABEL explained how he had postponed introducing himself to the *Institut de France* until his mastery of the French language had improved. He continued the letter:

> "Furthermore, and in particular, I want to complete the memoir I am working on and which I intend to present to the Institute. When it is complete, which will soon be the case, I will go there. The memoir has come out very well and contains many new things which I believe merit attention. It is the first draft of a theory of an infinitude of transcendental functions. — I nourish the hope that the Academy [*Académie des Sciences*] will have it printed in the Mémoires des savants étrangers."[3]

Finally, on October 30, 1826, ABEL presented his memoir to the *Institut de France*. Three days later ABEL sent an article for publication in J. D. GERGONNE'S (1771–1859) *Annales* in which he presented his research on simultaneous solutions to two polynomial equations.[4] This paper contained an elaboration of one of the main tools of ABEL'S *Paris memoir*; it is discussed in section 19.3, below. When he left France on 29 December 1826,[5] ABEL had still not received any reaction from the *Institut* concerning his memoir and, in fact, he was never to receive one. ABEL'S *Paris memoir* was misplaced before G. LIBRI (1803–1869) was commissioned with its printing. It eventually occurred in the *Mémoires présentés par divers savants* in 1841. The fate and reception of ABEL'S *Paris memoir* are briefly described in section 19.4.

19.1.2 Tools in ABEL's toolbox

The *Paris memoir* — when seen together with some of ABEL'S other publications described in chapter 18 — provides new insights into the toolbox of the creative, young mathematician. As could be expected of a mathematician devoted to algebra, the compartments for results concerning polynomials and equations are remarkably well equipped.

1. ABEL used algebraic deductions and EUCLID'S (∼295 B.C.) algorithm in ways similar to those which he had already employed in his research on algebraic

3 "*Desuden vil jeg først og fremst have en Afhandling færdig som jeg arbeider paa og som jeg vil forelægge Institutet. Naar denne, hvilket snart skeer, er færdig gaar jeg derhen. Denne Afhandling er lykkets mig særdeles godt, og indeholder meget nyt og som jeg troer værdig Opmærksomhed. C'est la prémière ébauche d'une théorie d'une infinité de fonctions transcendantes. — Jeg har det Haab at Academiet vil lade den trykke i Mémoires des savants étrangers.*" (Abel→Hansteen, Paris, 1826/08/12. N. H. Abel, 1902a, 40).

4 (N. H. Abel, 1827a).

5 (Lange-Nielsen, 1927, 65).

solubility. One central example was a tacitly employed result which has been presented in lemma 4, below.

2. Another algebraic result was employed by ABEL to the effect that for any real polynomial without multiple roots, such as $p(x) = \prod_{k=1}^{n}(x - x_k)$,
$$\sum_{k=1}^{n} \frac{x_k^{\alpha}}{p'(x_k)} = \begin{cases} 0, \text{if } \alpha \leq n-2 \text{ and} \\ 1, \text{if } \alpha = n-1. \end{cases}$$
This result, which is a consequence of the so-called *Lagrange interpolation* is discussed in section 19.3.

3. ABEL also borrowed results concerning primitive roots and congruences from C. F. GAUSS' (1777–1855) *Disquisitiones arithmeticae*. Again, we have already noted how well acquainted ABEL was with this book by GAUSS.

4. Finally, ABEL used expansions into series of decreasing powers to great effect. Such expansions had been forcefully employed in the 17th century, and ABEL certainly considered the procedure well established. In ABEL'S work it was combined with a particular emphasis on the coefficient of x^{-1}, the coefficient which elsewhere, with A.-L. CAUCHY (1789–1857), became known as the *residue*.

Most of these issues are addressed in some details in section 19.3 after ABEL'S use of them in the *Paris memoir* has been described.

19.1.3 The presentational style of the *Paris memoir*

When compared to the other works in ABEL'S corpus, the style of the *Paris memoir* stands out in a number of respects. When compared to the subsequent partial announcements of results contained in the *Paris memoir* (see section 19.5.1), a pattern becomes discernible. At the textual level, ABEL'S papers fell between two traditions, one mainly based on algebraic manipulations and derivations of formulae and an emerging one returning to the Euclidean norm of definitions, theorem statements, and proofs (see chapter 21). In this continuum of styles, the *Paris memoir* belongs to the manipulation based tradition with its long, tedious, and very explicit derivations of explicit formulae. The theorems which I have extracted (Main Theorems I and II, 16 and 17) are reconstructions, and reformulating ABEL'S main results in the "If ..., then ..." structure of modern theorem-based mathematics is by no means an easy and trivial task; the translation from ABEL'S explicit manipulative style to the structure of theorems is not a bijection, it requires interpretation.

19.1.4 ABEL's notational innovations

1. ABEL used a summation shorthand
$$\Sigma F x = F x_1 + F x_2 + \cdots + F x_n$$

An important lemma

Lemma 4 *Let* $\chi(y) = 0$ *be an* irreducible *equation of degree* n *and let* $\theta(y)$ *be an equation of degree* $n-1$*. Then* y *can be expressed rationally in* χ, θ*.* □

PROOF (PROOF OF LEMMA 4) By the Euclidean algorithm, there exist polynomials q, r such that

$$\chi = q\theta + r$$

where $\deg r < \deg\theta$. Since χ is irreducible and $\deg q = \deg\chi - \deg\theta = 1 > 0$, $\deg r > 0$. Thus, there exist *numbers* s, t such that

$$q(y) = sy + t.$$

Consequently,

$$y = \frac{q(y) - t}{s} = \frac{\frac{\chi(y)-r}{\theta(y)} - t}{s},$$

and y has been expressed rationally in χ, θ. ■

Box 9: An important lemma

which apparently was innovative with him. As is evident from even this example, both the index over which the summation is to be performed and the upper summation limit are implicit in the shorthand version.

2. For a rational function Fx, ABEL let ΠFx denote the coefficient of $\frac{1}{x}$ in the series expansion of Fx in decreasing powers of x. Designating the 'same' object as the residue which CAUCHY studied from an emerging perspective of his calculus of residues, ABEL'S Π corresponds to CAUCHY'S $\mathcal{E}$.

3. ABEL also introduced the operation h on algebraic functions which represented a general *degree* of algebraic functions.

4. In the ultimate example of hyperelliptic integrals, ABEL introduced the notation EA and εA for A any real number to denote the integer and remaining part, $A = EA + \varepsilon A$ ($EA \in \mathbb{Z}$ and $0 \leq \varepsilon A < 1$). It is worth remarking that ABEL did not — in the *Paris memoir* considered as a whole — apply this notation consistently. Until the ultimate section, he preferred the verbal formulation 'the greatest integer contained in the number'.

All these notational innovations enabled ABEL to comprehend, master, and manipulate objects in a precise way which had hitherto been difficult to obtain.

19.2 The contents of ABEL's Paris result and its proof

ABEL'S objective in the memoir was to study integrals of the form

$$\int f(x,y)\,dx$$

in which x and y were related by some algebraic equation (19.1) and f was a rational function. Such integrals provided a way of generalizing *elliptic integrals*; any elliptic integral could be written in the form above. However, it was not through a direct study of *one* such integral that something new was to be learned, but by studying relations — arising from the additional equation (19.3) — among a number of such integrals.

The contents of the *Paris memoir* can be structured into several results and their primary applications:

1. The establishment of Main Theorem I on integration of certain sums of algebraic differentials by elementary functions,

2. The establishment of Main Theorem II on the number of *independent* integrals of algebraic differentials, and

3. Application of Main Theorem II to the *simplest* case, the case of hyperelliptic integrals.

In the following, the results and methods of first two of these three parts will be described; as will the other instances where ABEL presented his findings on related issues. ABEL'S reasoning is rather cumbersome and not completely flawless. Some of the subsequent objections and comments — primarily by P. L. M. SYLOW (1832–1918) — are referred to in the course of the presentation. However, despite the reservations, ABEL'S original argument is presented to illustrate how he ingeniously used the tools at his disposal. Even if the contents and purpose of ABEL'S arguments can seem to evade attention, his various tools and the contents of the *Paris memoir* are subsequently summarized.

To various degrees of authenticity, ABEL'S argument has been described from the viewpoint of the application to hyperelliptic integrals, see e.g. (Brill and Noether, 1894; Cooke, 1989). However, as will be discussed in section 19.5.1, the chronology and internal logical structure suggests that the results of the *Paris memoir* were indeed prior to and to some extent independent of the applications to this (afterwards) immensely important special case.

19.2.1 Main Theorem I

In the *Paris memoir*,[6] ABEL dealt with two quantities x and y related through an irreducible polynomial equation such as

$$\chi(y) = 0, \tag{19.1}$$

where χ is a polynomial in y whose coefficients are polynomial functions of x,

$$\chi(y) = \sum_{k=0}^{n} p_k(x)\, y^k. \tag{19.2}$$

This relation (19.2) implicitly introduced n functions $y^{(1)}, \ldots, y^{(n)}$ of x corresponding to the n roots the equation would have for any particular value of x.

Introducing another (later to be specialized) equation in y whose degree was one less than χ,

$$\theta(y) = \sum_{k=0}^{n-1} q_k(x)\, y^k = 0, \tag{19.3}$$

ABEL formed the product

$$r = \prod_{k=1}^{n} \theta\left(y^{(k)}\right), \tag{19.4}$$

by inserting the n different solutions of (19.2) into (19.3) and multiplying the n results. The coefficients $q_0, \ldots, q_{n-1}$ could contain some indeterminate quantities $a_1, \ldots, a_N$, and r was found to be an entire function of x and $a_1, \ldots, a_N$ by methods "imported" from the theory of equations.

In order to focus attention, ABEL split the product r into parts dependent on and independent of the indeterminate quantities

$$r = F_0(x)\, F(x), \tag{19.5}$$

where only F depended on $a_1, \ldots, a_N$. ABEL then considered the equation

$$F(x) = 0, \tag{19.6}$$

which would provide expressions for its roots $x_1, \ldots, x_\mu$ in terms of $a_1, \ldots, a_N$,

$$F(x) = \prod_{k=1}^{\mu} (x - x_k).$$

These roots would become very important in the ensuing deductions.[7]

6 (N. H. Abel, [1826] 1841).

7 At this point it might be fruitful to summarize ABEL'S results this far. He knew that r, defined from the polynomials θ and χ, was an entire function of x and the indeterminates $a_1, \ldots, a_N$. Any root of the equation $r(x) = 0$ corresponded to a value y for which $\theta(y) = 0$ by obvious inspection. However, $r(x) = 0$ also meant either $F_0(x) = 0$ or $F(x) = 0$. The former case represented an equation independent of the indeterminates $a_1, \ldots, a_N$, whereas the latter introduced a relationship between x and the indeterminates. Thus, with given indeterminates, $r(x) = 0$ would mean either that x belonged to the set $\{x_1, \ldots, x_\mu\}$ or that $F_0(x) = 0$. To each x_k in $\{x_1, \ldots, x_\mu\}$ corresponded a value of y, which ABEL termed y_k, such that $\theta(y_k) = 0$.

After these algebraic operations, ABEL now for the first time employed the calculus in differentiating the equation (19.6) above. ABEL wrote the differentiation as

$$F'(x)\ dx + \partial F(x) = 0, \tag{19.7}$$

where $F'(x)$ represents the differential of F with respect to x and $\partial F(x)$ represents the differential of F with respect to all the indeterminates. This relationship was a fundamental one, and ABEL immediately put it to use. He introduced the differential

$$dv = \sum_{k=1}^{\mu} f(x_k, y_k)\ dx_k,$$

where f was a rational function. This differential was the real object of concern in these investigations. Through a sequence of deductions employing the theory of equations (see example below), ABEL reasoned that dv was a rational function of the parameters $a_1, \ldots, a_N$. Therefore, its integral v would have to be expressible by algebraic and logarithmic functions of these parameters[8],

$$v = \sum_{k=1}^{\mu} \int f(x_k, y_k)\ dx_k = \text{algebraic and logarithmic terms.}$$

This result is what I have termed *Main Theorem I*.

Theorem 16 (Main Theorem I) *Under the present assumptions, the sum*

$$\sum_{k=1}^{\mu} \int f(x_k, y_k)\ dx_k$$

can be expressed by algebraic and logarithmic functions of the parameters $a_1, \ldots, a_N$. □

An example of ABEL'S use of the theory of equations. In order to see that dv was indeed a rational function of the parameters, ABEL first claimed that the simultaneous equations (19.1) and (19.3) expressed y_k as a *rational* function of x_k,[9]

$$y_k = \rho(x_k).$$

Rearranging the equation (19.7) then produced

$$f(x, y)\ dx = -\frac{f(x, \rho(x))}{F'(x)} \partial F(x) = \phi_2(x).$$

Of this function ϕ_2, ABEL observed that it was obviously rational in x and the parameters. Thus, dv could be rewritten as

$$dv = \sum_{k=1}^{\mu} \phi_2(x_k) \tag{19.8}$$

8 The integral of any rational function was of course expressible by rational and logarithmic terms.

9 In order to see that ρ is rational as claimed, please observe that $\deg \theta = \deg \chi - 1$. See the proof of lemma 4 in box 9.

where ϕ_2 was a rational function of the parameters and its explicit argument[10]. However, because the right hand side of (19.8) was both rational and symmetric in the roots $x_1, \ldots, x_\mu$ of the equation (19.6), dv could be expressed rationally in the coefficients of F by a basic theorem which ABEL knew from his theory of equations (see section 5.2.4). However, the coefficients of F were supposed to depend rationally on the parameters, and the claim had been demonstrated.

19.2.2 An explicit expression for v

ABEL summarized the results of the Main Theorem I and introduced the way forward with these words:

> "Previously, we have demonstrated how it is always possible to form the rational differential dv. However, as the indicated method will generally be very long and nearly impractical for slightly complicated functions, I will give another [method] by which one will immediately obtain the expression of the function v in all possible cases."[11]

The expression for v, which ABEL obtained in "all the possible cases" was of the following form (we shall comment on the particulars and the notation below)

$$v = C - \Pi\phi(x) + \sum_{\nu=1}^{\alpha} \nu \frac{d^{\nu-1}\phi_1(x)}{dx^{\nu-1}}. \tag{19.9}$$

First, we will pay some attention to ABEL'S arguments in order to illustrate how they relate to the deduction of Main Theorem I and how they introduce tools which would become important in deducing Main Theorem II (see below).

A sequence of manipulations — in which ABEL again made important use of his knowledge from the theory of algebraic equations — led ABEL to express the sought after differential in the following form

$$dv = -\sum \frac{R_1(x)}{A \cdot F'(x) \cdot \prod_{k=1}^{\alpha}(x-\beta_k)^{\nu_k}}, \tag{19.10}$$

where R_1 was an entire function and A was a constant. The next step was the reduction of this expression. ABEL first revised the notation and wrote

$$dv = -\sum_{k=1}^{\mu} \frac{R_2(x_k)}{F'(x_k)} - \sum_{k=1}^{\mu} \frac{R_3(x_k)}{\theta_1(x_k) \cdot F'(x_k)}, \tag{19.11}$$

[10] ABEL chose to denote this function ϕ_2, although no ϕ_1 had been introduced at this point. This might suggest that the logical order in ABEL'S head of these sections had been reversed in the written version. See below.

[11] *"Nous avons montré dans ce qui précède comment on peut toujours former la différentielle rationelle dv; mais comme la méthode indiquée sera en général très-longue, et pour des fonctions un peu composées, presque impractible, je vais en donner une autre, par laquelle on obtiendra immédiatement l'expression de la fonction v dans tous les cas possibles."* (N. H. Abel, [1826] 1841, 150).

where the auxiliary θ_1 had been introduced by

$$\theta_1(x) = A \prod_{k=1}^{\alpha} (x - \beta_k)^{\nu_k},$$

and

$$\begin{aligned} &R_1(x) = \theta_1(x) R_2(x) + R_3(x), \text{ with} \\ &\deg R_3 < \deg \theta_1. \end{aligned} \tag{19.12}$$

The introduction of the additional auxiliary functions R_2 and R_3 was made to ease the study of the quotient $\frac{R_1}{\theta_1}$, which because of (19.10) was at the centre of ABEL'S interest.

Implicitly using the method of *Lagrange interpolation*,[12] ABEL reduced the first term of (19.11),

$$\sum_{k=1}^{\mu} \frac{R_2(x_k)}{F'(x_k)} = \Pi \frac{R_2(x)}{F(x)}$$

where the symbol Π was introduced in the following way:

> "Thus, in designating by $\Pi F_1 x$ the coefficient of $\frac{1}{x}$ in the development of any function $F_1 x$ according to decreasing powers of x, one will get [...]"[13]

Because of (19.12), the reduction could be written

$$\sum_{k=1}^{\mu} \frac{R_2(x_k)}{F'(x_k)} = \Pi \frac{R_1(x)}{\theta_1(x) F(x)}.$$

ABEL'S reduction of the remaining term of (19.11) was marred by a faulty calculation which has been noticed and elaborated by SYLOW in his notes in the *Œuvres*.[14] ABEL claimed — by an argument based on expansion into partial fractions — that

$$\frac{R_3(x)}{\theta_1(x)} = \sum_{k=1}^{\alpha} v_k \frac{d^{v_k - 1}}{d\beta^{v_k - 1}} \left(\frac{R_3(\beta)}{\theta_1^{(v_k)}(\beta) \cdot (x - \beta)} \right)_{\beta = \beta_k}. \tag{19.13}$$

However, during his deductions concerning partial fractions he had mistakenly placed the factor $(x - \beta)$ in the denominator instead of in the numerator.[15] This flaw permeated the ensuing calculations, and a simple counter example could serve to demonstrate that the result claimed in (19.13) is only valid in some very particular cases.

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12 See below.

13 "*En désignant donc par* $\Pi F_1 x$ *le coefficient de* $\frac{1}{x}$ *dans le développement d'une fonction quelconque* $F_1 x$*, suivant les puissances descendantes de* x*, on aura [...]*" (ibid., 155).

14 (Sylow in N. H. Abel, 1881, II, 295–296).

15 The mistake is indeed ABEL'S which can be seen from the fact that it occurs in the manuscript of the *Paris memoir*.

ABEL and the method of *Lagrange interpolation*. Without any specific reference, ABEL employed a result to the effect that

$$\sum_{k=1}^{n} \frac{p(x_k)}{\chi'(x_k)} = \begin{cases} 0 \text{if } \deg p < n-1 \\ 1 \text{if } \deg p = n-1 \end{cases}$$

for any normed polynomial p where $x_1, \ldots, x_n$ are the roots of the polynomial equation $\chi(x) = 0$, i.e.

$$\chi(x) = \prod_{k=1}^{n} (x - x_k). \tag{19.14}$$

The tool behind this result is known today as *Lagrange interpolation*, and — in various forms — it played central roles in ABEL'S arguments in the *Paris memoir*. *Lagrange interpolation* is used to demonstrate that for any polynomial such as (19.14),

$$\frac{1}{\chi(x)} = \sum_{k=1}^{n} \frac{1}{(x - x_k)\chi'(x_k)}. \tag{19.15}$$

Expansion into partial fractions using *Lagrange interpolation*. The method of expanding a quotient of polynomials into partial fractions was well established in the 18th century once it was known that the denominator could be decomposed into a product of linear and quadratic terms. The generality of the method thus rested essentially on the *Fundamental Theorem of Algebra*, and the proof of the latter theorem was often seen mainly as a prerequisite in rigorously founding this established practice (cf. GAUSS).

The central trick in expanding a quotient into partial fractions is closely related to the method of *Lagrange interpolation*. If the polynomials are

$$f_1(x) \text{ and}$$
$$f_2(x) = \prod_{k=1}^{n} (x - x_k),$$

where the roots of f_2 are distinct, *Lagrange interpolation* (19.15) yields

$$\frac{f_1(x)}{f_2(x)} = \sum_{k=1}^{n} \frac{f_1(x)}{(x - x_k) f_2'(x_k)}.$$

Thus, when applied in the integral calculus, this formula reduces the integration of a fraction to the integration of n fractions, the denominator of each of which only contains a first degree polynomial.

ABEL'S result is faulty when the denominators have multiple roots. For situations in which the polynomial f_2 has multiple roots, the procedure can be extended to accommodate this case as well. The flawed result which ABEL used (see above) was for a general rational function

$$\frac{f_1(x)}{f_2(x)}$$

with

$$f_2(x) = \prod_{k=1}^{n} (x - x_k)^{m_k},$$

the fraction could be expanded as

$$\frac{f_1(x)}{f_2(x)} = \sum_{k=1}^{n} \sum_{m=1}^{m_k} \frac{A_k^m}{(x - x_k)^m}$$

where

$$A_k^m = \frac{d^{m_k-1-m} p_k}{\Gamma(m_k + 1 - m)\ d\beta^{m_k-1-m}},$$
$$p_k = \frac{\Gamma(m_k - 1)\ f_1(\beta_k)}{f_2^{(m_k)}(\beta_k)}.$$

However, ABEL'S formulae for the coefficients were wrong; they should have been

$$A_k^m = \frac{1}{\Gamma(m_k + 1 - m)} \frac{d^{m_k-m}}{d\beta^{m_k-m}} \left(\frac{(x-\beta)^{m_k} R_3(x)}{\theta_1(x)} \right)_{\beta=\beta_k},$$

as SYLOW pointed out.[16] Incidentally, this formula was very similar to ABEL'S starting point:

> ""[...] one will get
>
> $$A_1 = \frac{d^{\nu-1}p}{\Gamma\nu \cdot d\beta^{\nu-1}},\ A_2 = \frac{d^{\nu-2}p}{\Gamma(v-1)\, d\beta^{\nu-2}},\ \ldots,\ A_\nu = p,$$
>
> where
>
> $$p = \frac{(x-\beta)^\nu R_3 x}{\theta_1 x}$$
>
> for $x = \beta$; [...]"[17]

16 (Sylow in N. H. Abel, 1881, II, 295).

17 "*[...] on aura*

$$A_1 = \frac{d^{\nu-1}p}{\Gamma\nu \cdot d\beta^{\nu-1}},\ A_2 = \frac{d^{\nu-2}p}{\Gamma(v-1)\, d\beta^{\nu-2}},\ \ldots,\ A_\nu = p,$$

où

$$p = \frac{(x-\beta)^\nu R_3 x}{\theta_1 x}$$

pour $x = \beta$*; [...]*" (N. H. Abel, [1826] 1841, 156).

The ensuing step was, however, unwarranted as ABEL claimed that

$$p = \frac{\Gamma(\nu+1) R_3(\beta)}{\theta_1^{(\nu)}(\beta)}.$$

One can guess how ABEL came to the latter belief by applying the rule of G.-F.-A. DE L'HOSPITAL (1661–1704) v times to the definition of p as both numerator and denominator vanish. In the above presentation, the problem which ABEL'S deduction suffered from is hidden in the notation. First of all, ABEL'S way of suppressing the subscript k has made the β and x appear symbolically similar, although x is a true variable whereas $\beta_1, \ldots, \beta_\alpha$ are the roots of a certain polynomial. This distinction is at the core of SYLOW'S objection to ABEL'S argument.[18] However, with a minor adjustment to the definitions, ABEL'S final product (19.9) of the argument could be allowed.

19.2.3 Main Theorem II

After the first four sections of the *Paris memoir*, ABEL had thus obtained a formula which was essentially (apart from the corrections indicated above) the following,

$$v = \sum_{k=1}^{\mu} \psi(x_k) = \sum_{k=1}^{\mu} \int f(x_k, y_k)\, dx_k = C - \Pi\phi(x) + \sum_{\nu=1}^{\alpha} \nu \frac{d^{\nu-1}\phi_1(x)}{dx^{\nu-1}}.$$

This expression allowed him to commence a study of the number of free parameters which would eventually lead to the second main theorem — the celebrated *Abelian Theorem*. To follow his argument, we need to backtrack a little to properly understand the use of the eliminant equation $r = 0$ (see page 352).

ABEL'S trick was first to study the consequences of one further assumption concerning the factor F_0 of r containing the indeterminate quantities. ABEL assumed that F_0 had α distinct zeros,

$$F_0(x) = \prod_{k=1}^{\alpha} (x - \beta_k)^{\mu_k};$$

an assumption which — provided F_0 is not a constant — introduced α linear interrelations among the coefficients $q_0, \ldots, q_{n-1}$ of the auxiliary polynomial $\theta(y)$ (19.3).[19] ABEL found the fact that the coefficients of $\theta(y)$ formed a non-independent set to be — in general — a contraction of the original hypothesis which required nothing of the coefficients $q_0, \ldots, q_{n-1}$. Consequently, he concluded that $F_0(x)$ had to be a constant and $r(x)$ could not — in general — contain any factor independent of the auxiliary quantities.

Under this assumption, ABEL proceeded to describe the various functions involved. The important outcome of these investigations was that a certain function $f_2(x)$ introduced much earlier in the investigations, reduced to unity, thereby providing the result that $f(x, y)\,\chi'(y)$ equated the entire function $f_1(x, y)$.

18 (Sylow in N. H. Abel, 1881, II, 295).

19 See box 19.2.3.

Linear interdependence of $q_0, \ldots, q_{n-1}$ In order to see how ABEL obtained the linear interrelations among the coefficients of $\theta(y)$, we notice from combining the factorization (19.5) with the definition of r (19.4),

$$\begin{aligned} r(x) &= F(x) \cdot F_0(x) \\ &= \prod_{k=1}^{n} \theta\left(y^{(k)}(x)\right) = \prod_{k=1}^{n} \sum_{m=0}^{n-1} q_m(x) \left(y^{(k)}(x)\right)^m. \end{aligned}$$

Thus, since $r(\beta_1) = F(\beta_1) \cdot 0 = 0$, some k must exist for which $\theta\left(y^{(k)}(\beta_1)\right) = 0$, i.e.

$$\sum_{m=0}^{n-1} q_m(x) \left(y^{(k)}(x)\right)^m = 0.$$

This relation is a linear interdependence among the $q_0, \ldots, q_{n-1}$ in which the $\left(y^{(k)}\right)^m$, here serving as coefficients, are functions of x.

Box 10: Linear interdependence of $q_0, \ldots, q_{n-1}$

ABEL'S way of proving $f(x,y)\chi'(y)$ to be an entire function. ABEL'S investigations leading to the result that $f(x,y)\chi'(y)$ was an entire function progressed along complicated and tedious arguments. At the very outset of the paper, ABEL had split the rational function of x in the following way

$$f(x,y)\chi'(y) = \frac{f_1(x,y)}{f_2(x)}, \tag{19.16}$$

where $f_2(x)$ was an entire function of x independent of y. By combining this with the important partial differentiation (19.7), ABEL found

$$\begin{aligned} f(x,y)\,dx &= \frac{f_1(x,y)}{f_2(x) \cdot \chi'(y)}\,dx \\ &= -\frac{1}{F_0(x) \cdot F'(x) \cdot f_2(x)} \sum_{k=1}^{n} \frac{f_1(x,y_k)}{\chi'(y_k)} \frac{r}{\theta(y_k)} \partial\theta(y_k). \end{aligned}$$

Later, during his investigations leading to the *Main Theorem I*, ABEL had studied the function

$$f_1(x,y)\frac{r}{\theta(y)}\partial\theta(y)$$

which he had chosen to write as

$$f_1(x,y)\frac{r}{\theta(y)}\partial\theta(y) = R'(y) + R(x)\,y^{n-1},$$

where $R'(y)$ indicated an entire function of x and y in which no powers of y beyond the $(n-2)$'nd occur, and $R(x)$ was an entire function of x independent of y. By use of the method of *Lagrange interpolation* described above and the factorization of r (19.5), ABEL found

$$R(x) = F_0(x) \cdot F(x) \cdot \sum_{k=1}^{n} \frac{f_1(x, y_k)}{\chi'(y_k)} \frac{\partial\theta(y_k)}{\theta(y_k)}.$$

Under the present assumption $F_0(x) = 1$, ABEL concluded that

$$\phi_1(x) = \frac{1}{f_2^{(m)}(x)} \sum_{k=1}^{n} \frac{f_1(x, y_k)}{\chi'(y_k)} \log\theta(y_k),$$

and consequently, the sum of integrals took the form (cmp. equation 19.9)

$$\sum \int \frac{f_1(x,y)\,dx}{f_2(x)\,\chi'(y)} = C - \Pi \sum \frac{f_1(x,y)}{f_2(x)\,\chi'(y)} \log\theta(y) \\ + \sum m \frac{d^{m-1}}{d\beta^{m-1}} \left(\frac{1}{f_2^{(m)}(\beta)} \sum \frac{f_1(\beta, B)}{\chi'(B)} \log\theta(B) \right). \quad (19.17)$$

After inspecting the results of certain simple assumptions concerning f_2, ABEL argued:

> "In the equation (19.17), the right-hand-side is in general a function of the quantities a, a', a'', etc. If one supposes this function equal to a constant, certain relations among these quantities thus generally result; but there are also certain cases for which the right-hand-side reduces to a constant no matter what the values of the quantities a, a', a'', etc. are. We investigate this case:
>
> From this it is evident that the function $f_2 x$ must be constant, because in the contrary case the right-hand-side necessarily contains the quantities $a, a', a'' \ldots,$ with respect to the arbitrary values of these quantities."[20]

ABEL'S argument here seems a little roundabout; in the cause of argument he introduced an important assumption — that v now reduces to a constant. He argued that unless f_2 then also reduced to a constant, the right hand side of (19.17) would involve the auxiliary quantities, whereas the left hand side — which was nothing but $v = \sum \int f(x,y)\,dx$ by (19.16) — was now a constant. Thus, unless f_2 was constant, certain relations among the indeterminates $a, a', a'', \ldots$ would result — a contradiction. In the end, ABEL had obtained a representation of the constant v in the following form

$$\sum \int \frac{f_1(x,y)\,dx}{\chi'(y)} = C - \sum \Pi \frac{f_1(x,y)}{\chi'(y)} \log\theta(y).$$

If we pause for a second to consider what the consequences of this new hypothesis, the constancy of v, are, we can suggest some motivation for this at first sight rather unnatural assumption which will be elaborated during the uncovering of the remaining

20 "*Dans le formule (43) [here* (19.17)*], le second membre est en général une fonction des quantités a, a', a'', etc. Si on le suppose égal à une constante, il en résultera donc en général certaines relations entre ces quantités; mais il y a aussi certaines cas pour lesquels le second membre se réduit à une constante, quelles que soient d'ailleurs les valeurs des quantités a, a', a'', etc. Cherchons ces cas: D'abord il est évident que la fonction $f_2 x$ doit être constante, car dans le cas contraire le second membre contiendrait nécessairement les quantités $a, a', a'' \ldots$, vu les valeurs arbitraires de ces quantités.*" (N. H. Abel, [1826] 1841, 161).

details. If v is a constant, say $v = 0$, the basic objects of the inquiry satisfy

$$0 = \sum \int f(x_k, y_k)\, dx_k.$$

The end product of the present section of the paper is the *Abelian Theorem* (*Main Theorem II*) states something about exactly such sums of related integrals.

ABEL next expanded the relevant function, whose derivative was rational in x by (19.8), according to decreasing powers of x,

$$\sum \frac{f_1(x,y)}{\chi'(y)} \log \theta(y) = R \log x + \sum_{k=0}^{\infty} A_k x^{\mu_0 - k}, \tag{19.18}$$

where R was "a function of x independent of a, a', a'', etc.,"[21] $A_0, A_1, \ldots$ were independent of x, and μ_0 designated an integer.[22]

If expression (19.18) were to express a constant (independent of the indeterminates $a, a', a'', \ldots$), ABEL observed that since these quantities occurred in $A_0, A_1, \ldots$, the second term corresponding to these had to vanish. And since he was only concerned with the coefficient of $\frac{1}{x}$, his conclusion was that $\mu_0 < -1$. He expressed this using a newly introduced notational advance in the following sentence:

> "This done, in designating by the symbol hR the highest exponent of x in the development of any function R of this quantity following decreasing powers, it is evident that μ_0 will be equal to the largest integer contained in [less than or equal to] the numbers
>
> $$h\frac{f_1(x,y')}{\chi' y'}, h\frac{f_1(x,y'')}{\chi' y''}, \ldots h\frac{f_1\left(x, y^{(n)}\right)}{\chi' y^{(n)}}.$$
>
> It is necessary that all these numbers must be less than the unit taken negatively."[23]

21 "*R étant une fonction de x indépendante de* a, a', a''*, etc.*" (ibid., 161).

22 The version printed in the *Savants étrangers* read at this point

$$R \log x = \begin{cases} A_0 x^{\mu_0} + A_1 x^{\mu_0 - 1} + \ldots \\ +A_{\mu_0} + \dfrac{A_{\mu_0+1}}{x} + \dfrac{A_{\mu_0+2}}{x^2} + \ldots \end{cases}$$

In the collected works (Sylow in N. H. Abel, 1881, II, 296), SYLOW commented: "*C'est évidemment une faute d'écriture, ou d'Abel ou de* Libri." After the original manuscript has been recovered, it has become evident that the misprint is indeed due to LIBRI.

23 "*Cela posé, en désignant par le symbole hR le plus haut exposant de x dans le développement d'une fonction quelconque R de cette quantité, suivant les puissances descendantes, il est clair que* μ_0 *sera égal au nombre entier le plus grand contenu dans les nombres:*

$$h\frac{f_1(x,y')}{\chi' y'}, h\frac{f_1(x,y'')}{\chi' y''}, \ldots h\frac{f_1\left(x, y^{(n)}\right)}{\chi' y^{(n)}};$$

il faut donc que tous ces nombres soient inférieurs à l'unité prise négativement." (N. H. Abel, [1826] 1841, 161).

Thus, for the individual terms of the sum, which were in general just algebraic functions of x, the 'degree' hR needed not be an integer, whence the conclusion

$$h\frac{f_1(x,y_k)}{\chi'(y_k)} < -1. \tag{19.19}$$

The investigation now turned to algebraic manipulations of these new symbols.

Determination of the most general form of the function $f_1(x,y)$. ABEL put his new tool to immediate use. From the general formula

$$h\frac{R_1}{R_2} = hR_1 - hR_2,$$

he derived from (19.19) the inequalities

$$hf_1(x,y_k) < h\chi'(y_k) - 1,$$

which he claimed made "it easy to deduce the most general form of the function $f_1(x,y)$ in each particular case."[24] ABEL'S argument — but not its result — has been found unrigorous at this point, see e.g. SYLOW'S notes, the paper by ELLIOT, and below.[25] However, it is worth following the steps of his argument to see how he went about it.

Because

$$\chi'(y_k) = \prod_{m\neq k}(y_k - y_m),$$

ABEL obtained

$$h\chi'(y_k) = \sum_{m\neq k} h(y_k - y_m),$$

and when the $y_1, \ldots, y_k$ were ordered according to decreasing degrees,

$$hy_k \geq hy_m \text{ if } k \leq m,$$

he found "in general, except for certain particular cases which he did not consider:"[26]

$$h(y_k - y_m) = hy_{\min(k,m)}.$$

The analogy with the ordinary degree operator makes the above-mentioned particular cases easy to illustrate. For instance, if we have two monic polynomials of the same degree, the degree of their difference is strictly less than either of the original degrees,

$$\deg\left(\left(x^2+x-1\right) - \left(x^2-x+2\right)\right) = \deg(2x-3) = 1.$$

24 "*De ces inégalités on déduira facilement dans chaque cas particulier la forme la plus générale de la fonction $f_1(x,y)$.*" (N. H. Abel, [1826] 1841, 162).

25 (Sylow in N. H. Abel, 1881, II, 296–297) and (Elliot, 1876, 404–406).

26 "*Alors on aura, en général, excepté quelques cas particuliers que je me dispense de considérer:*" (N. H. Abel, [1826] 1841, 162).

Later, in chapter 21, we shall have more to say on this notion of 'in general, except for certain particular cases.'

Writing $f_1(x,y)$, which was by construction an entire function of y, in the way

$$f_1(x,y) = \sum_{m=0}^{n-1} t_m(x)\, y^m,$$

ABEL concluded

$$ht_m y_k^m < h\chi'(y_k) - 1 \text{ for } k = 1,\dots,n \text{ and } m = 0,\dots,n-1.$$

Based on the identity

$$h(t_m y^m) = ht_k + mhy,$$

this evolved into

$$ht_m + mhy < h\chi'(y_k) - 1.$$

ABEL now combined the information contained in the equations () obtaining

$$h\chi'(y_k) - mhy_k - 1 = (n-m-k)\, hy_k + \sum_{u=1}^{k-1} hy_u - 1.$$

Since $y_1,\dots,y_n$ were assumed to be ordered according to decreasing degrees, the minimal value among these (over $k = 1,\dots,n$) was obtained for $k = n-m$, resulting in the value

$$\min_{k=1,\dots,n} \left(h\chi'(y_k) - mhy_k - 1\right) = h\chi'(y_{n-m}) - mhy_{n-m} - 1 = \sum_{u=1}^{n-m-1} hy_u - 1.$$

Therefore, because ht_m was an integer,

$$ht_m = \sum_{u=1}^{n-m-1} hy_u - 2 + \varepsilon_{n-m-1}, \tag{19.20}$$

where $0 < \varepsilon_{n-m-1} \leq 1$.

Grouping of roots according to their degree. The next step in ABEL'S analysis was to write

$$hy_1 = \frac{m_1}{\mu_1} \text{ with } (m_1, \mu_1) = 1$$

from which he obtained

$$hy_1 = hy_2 = \cdots = hy_{\mu_1} = \frac{m_1}{\mu_1}$$

by an argument involving tools from the theory of equations. In his investigations on algebraic solubility of equations, ABEL had proved (see e.g. chapter 8) that if an equation (here $\chi(y) = 0$) was satisfied by an expression such as $y = Ax^{\frac{m_1}{\mu_1}}$, an entire sequence of distinct roots could be obtained by substituting for $x^{\frac{1}{\mu_1}}$ in y the result of $x^{\frac{1}{\mu_1}}$ multiplied with the different μ_1'th roots of unity. Therefore, the roots $y_1, \ldots, y_n$ fell into sequences with equal degrees,

$$\tau\text{'th sequence: } hy_{k_\tau+1} = hy_{k_\tau+2} = \cdots = hy_{k_\tau+n_\tau\mu_\tau} = \frac{m_\tau}{\mu_\tau},\ (m_\tau, \mu_\tau) = 1,$$

$$n = \sum_{\tau=1}^{\varepsilon} n_\tau \mu_\tau.$$

When focusing his attention on an root y_m belonging to the τ'th sequence

$$m = k_\tau + 1 + \beta, \text{ with } 0 \leq \beta \leq k_{\tau+1} - k_\tau,$$

ABEL found from (19.20)

$$\begin{aligned}
ht_{n-m} = ht_{n-k_\tau-\beta-1} &= \sum_{u=1}^{k_\tau+\beta} hy_u - 2 + \varepsilon_{k_\tau+\beta} \\
&= \sum_{\alpha=1}^{\tau-1} \sum_{\tau=1}^{k_{\alpha+1}-k_\alpha} hy_{k_\alpha+\tau} + \sum_{\tau=1}^{\beta} hy_{k_\tau+\tau} - 2 + \varepsilon_{k_\tau+\beta} \\
&= \sum_{\alpha=1}^{\tau-1} (k_{\alpha+1} - k_\alpha) \frac{m_\alpha}{\mu_\alpha} + \beta \frac{m_\tau}{\mu_\tau} - 2 + \varepsilon_{k_\tau+\beta} \\
&= \sum_{\alpha=1}^{\tau-1} n_\alpha m_\alpha + \beta \frac{m_\tau}{\mu_\tau} + \varepsilon_{k_\tau+\beta} - 2
\end{aligned} \tag{19.21}$$

Number-theoretic arguments to determine the form of $f_1(x, y)$. The product $\varepsilon_{k_\tau+\beta} \cdot \mu_\tau$, for which ABEL introduced a special symbol[27] $A_{\tau,\beta}$, was found to be the least positive number ζ for which

$$\mu_\tau \mid \beta m_\tau + \zeta. \tag{19.22}$$

In order for us to see that $A_{\tau,\beta}$ has the prescribed property, it suffices to first observe from (19.21) that

$$\beta \frac{m_\tau}{\mu_\tau} + \varepsilon_{k_\tau+\beta} = \frac{\beta m_\tau + A_{\tau,\beta}}{\mu_\tau}$$

is an integer because t_m is an entire function. Next, if we assume

$$\frac{\beta m_\tau + \zeta}{\mu_\tau} = K < K' = \frac{\beta m_\tau + A_{\tau,\beta}}{\mu_\tau},$$

we obtain

$$(K' - K)\, \mu_\tau = A_{\tau,\beta} - \zeta = \mu_\tau \varepsilon_{k_\tau+\beta} - \zeta$$

27 Actually ABEL would write $A_\beta^{(\gamma)}$ for this quantity, but I have chosen to move indices into subscripts.

and thus

$$\varepsilon_{k_\tau+\beta} = \left(K' - K\right) + \frac{\zeta}{\mu_\tau} > 1$$

which is a contradiction. Thus $K = K'$ and $A_{\tau,\beta}$ is the smallest number such that (19.22) holds. This condition of minimality, which ABEL just noticed as a matter of fact, was soon (see below) invoked and found to be of great use.

When ABEL spelled out the results obtained above for the first sequence $\tau = 1$, he found

$$ht_{n-\beta-1} = -2 + \beta\frac{m_1}{\mu_1} + \frac{A_{1,\beta}}{\mu_1} = -2 + \frac{\beta m_1 + A_{1,\beta}}{\mu_1}.$$

For 'small' β (starting with $\beta = 0$), the right hand side is obviously negative, because $A_{1,\beta} < \mu_1$ by the definition of $\varepsilon_{k_\tau+\beta}$

$$\frac{A_{1,\beta}}{\mu_1} = \varepsilon_\beta \leq 1$$

and the other term vanishes for $\beta = 0$. Consequently, some $\beta' \geq 0$ existed such that

$$ht_{n-\beta-1} < 0 \text{ for } \beta = 0, \ldots \beta' \tag{19.23}$$

which obviously meant that

$$t_{n-\beta-1} = 0 \text{ for } \beta = 0, \ldots, \beta'. \tag{19.24}$$

The general form of the function $f_1(x, y)$ in the light of these results thus became

$$f_1(x, y) = \sum_{k=0}^{n-\beta'-1} t_k(x)\, y^k \tag{19.25}$$

in which β' was the largest integer less than $\frac{\mu_1}{m_1} + 1$,

$$\beta' = \left\lfloor \frac{\mu_1}{m_1} + 1 \right\rfloor.$$

ABEL'S way of obtaining this ultimate description of β' was found by SYLOW to miss certain particular cases.[28]

Based on the expression (19.25) which ABEL had obtained for the function $f_1(x, y)$, he concluded:

> "A function like $f_1(x, y)$ always exists when β does not surpass $n - 1$."[29]

It is difficult to see exactly what ABEL meant by this phrase, which seems to infer that the existence of the function was deduced from the representation (19.25). However, on a logical basis, the existence of the function $f_1(x, y)$ had been presupposed in the decomposition of $f(x, y)\,\chi'(y)$, see (19.16).

28 (Sylow in N. H. Abel, 1881, II, 298).

29 "*Une fonction telle que $f_1(x, y)$ existe donc toujours à moins que β' ne surpasse $n - 1$.*" (N. H. Abel, [1826] 1841, 166).

Investigation of the complementary case $\beta' \geq n$. In order to study what happened if $\beta' \geq n$, ABEL wrote

$$\frac{\mu_1}{m_1} + 1 = n + \varepsilon, \varepsilon \geq 0$$

and obtained for the inverse fraction only two possibilities

$$\frac{m_1}{\mu_1} = \frac{1}{n-1} \text{ or } \frac{m_1}{\mu_1} = \frac{1}{n}.$$

In both cases, ABEL claimed, the integral $\int f(x,y)\, dx$ would be expressible in algebraic and logarithmic terms. His argument proceeded from claiming that the equation $\chi(y) = 0$ was *linear* in x,

$$\chi(y) = P(y) + xQ(y).$$

If this was the case, the integrand in $\int f(x,y)\, dx$ was quickly seen to be a rational function, and the result was thus well known. However, as SYLOW has observed,[30] in the case left unnoticed above, the conclusion of linearity does not hold, and the deduction thus suffers from this incompleteness.

Back on track: the case $\beta' \leq n-1$. Returning to the more complicated case, ABEL noticed: "Thus, except for this case [$\beta' \geq n$], the function $f_1(x,y)$ always exists"[31] and he went on to elaborate the consequences of the hypothesis $\beta' \leq n-1$. He began by reducing the study of the equation

$$\sum \int \frac{\sum_{m=0}^{n-\beta'-1} t_m y^m}{\chi'(y)}\, dx = C \tag{19.26}$$

to the study of the individual terms

$$\sum \int \frac{x^k y^m\, dx}{\chi'(y)}.$$

His next and decisive step was to begin considering the $ht_m + 1$ coefficients in the polynomial t_m. He found that the function $f_1(x,y)$ contained

$$\sum_{m=0}^{n-\beta'-1} (ht_m + 1) = \sum_{m=0}^{n-\beta'-1} ht_m + n - \beta' = \sum_{m=0}^{n-2} ht_m + n - 1 \tag{19.27}$$

coefficients and chose to designate this number of coefficients by γ. Once this number had been introduced, it became ABEL'S first objective to derive other general formulae for it and to study certain particular cases. Once these investigations had been concluded, ABEL again returned to (19.26), remarking that it was even valid in certain cases not included in the deduction:

30 (Sylow in N. H. Abel, 1881, II, 298).
31 "*Excepté ce cas donc, la fonction $f_1(x,y)$ existe toujours*" (N. H. Abel, [1826] 1841, 167).

> "The formula (59) [here (19.26)] is generally valid for all values of the quantities $a, a', a'', \dots$ whenever the function r does not have a factor of the form $F_0 x$; in that case, it is also valid if $F_0 x$ and $\frac{\chi' y}{f_1(x,y)}$ vanish for the same value of x."[32]

Algebraic manipulations pertaining to the number γ. ABEL'S tedious manipulations of the expression for γ made critical use of a result in the line of GAUSS' theory of moduli and primitive roots — referred to by ABEL'S as "the theory of numbers"[33] — as presented in GAUSS' *Disquisitiones arithmeticae*.[34] ABEL found that because for the τ'th sequence m_τ and μ_τ were relatively prime, and $A_{\tau,\beta} \equiv -\beta m_\tau \pmod{\mu_\tau}$,

$$\sum_{\beta=0}^{n_\tau \mu_\tau - 1} A_{\tau,\beta} = n_\tau \left(\sum_{k=0}^{\mu_\tau - 1} k \right) = n_\tau \frac{\mu_\tau (\mu_\tau - 1)}{2}.$$

By combining this with other previously established formulae, ABEL found

$$\gamma = 1 + \sum_{\tau=1}^{\varepsilon} n_\tau \mu_\tau \left[\sum_{v=1}^{\tau-1} n_v m_v + \frac{m_\tau n_\tau - 1}{2} \right] - \sum_{\tau=1}^{\varepsilon} \frac{n_\tau (m_\tau + 1)}{2}. \tag{19.28}$$

At this point, two particular cases were noticed mainly as examples of how to calculate with the formula (see below).

ABEL'S examples of calculating γ. After deducing the formula (19.28) by algebraic and number theoretic manipulations, ABEL gave some examples of how it could be used in particular cases to determine γ.

1. If all the roots $y_1, \dots, y_n$ have the same degree ($\varepsilon = 1$)

   $$hy_1 = \cdots = hy_n = \frac{m_1}{\mu_1},$$

   the expression for γ reduced to

   $$\gamma = 1 + n_1 \mu_1 \frac{m_1 n_1 - 1}{2} - \frac{n_1 (m_1 + 1)}{2}.$$

   If, furthermore, $\mu_1 = n = n_1$, corresponding to the situation in which all the roots $y_1, \dots, y_n$ involve n'th roots of x, it reduced further into

   $$\gamma = (n-1) \frac{m_1 - 1}{2}.$$

[32] *"La formule (59) a généralement lieu pour des valeurs quelconques des quantités $a, a', a'', \dots$ toutes les fois que la fonction r n'a pas un facteur de la forme $F_0 x$; mais dans ce cas elle a encore lieu, sinon $F_0 x$ et $\frac{\chi' y}{f_1(x,y)}$ s'évanouissement pour une même valeur de x."* (ibid., 169).

[33] (ibid., 168)

[34] (C. F. Gauss, 1801). See also the discussion in section 5.3.

2. If all the roots $y_1, \ldots, y_n$ have integer degrees,

$$hy_1, \ldots, hy_n \in \mathbb{Z},$$

and

$$n_1 = \cdots = n_\varepsilon = 1,$$

the formula became ($\varepsilon = n$)

$$\begin{aligned}\gamma &= 1 + \sum_{\tau=1}^{n} \left(\sum_{\nu=1}^{\tau-1} m_\nu + \frac{m_\tau - 1}{2} \right) - \sum_{\tau=1}^{n} \frac{m_\tau + 1}{2} \\ &= 1 + \sum_{\tau=1}^{n} \sum_{\nu=1}^{\tau-1} m_\nu - n = 1 - n + \sum_{\tau=1}^{n} (n - \tau)\, m_\tau.\end{aligned}$$

It is interesting to consider the *usefulness* of these examples. It appears that the examples were both chosen because the assumptions made therein corresponded to particularly interesting cases and because they illustrate cases, in which the rather complicated formula (19.28) — which looked even more complicated in ABEL'S notation than in my modern one — reduced to extremely simple forms. The first class of equations considered (in which all degrees were equal) contains equations such as

$$\chi(x, y) = y^n - p(x) = 0$$

in which p is a polynomial. The second assumption (all roots have integer degrees) applies to equations of the form

$$\chi(x, y) = \prod_k (y - p_k(x)) = 0.$$

The indeterminates $a, a', a'', \ldots$. ABEL chose to designate by α the number of indeterminates $a, a', a'', \ldots$ and ventured to investigate the relationships between the roots $x_1, \ldots, x_\mu$ and the indeterminates $a_1, \ldots, a_\alpha$. To the α indeterminates corresponded α equations

$$\theta(y_\tau) = 0 \text{ for } \tau = 1, \ldots, \alpha$$

which were linear in the indeterminates (see box 19.2.3, above). These equations "in general" served to express the indeterminates rationally in $x_1, \ldots, x_\alpha$ and $y_1, \ldots, y_\alpha$. Only in cases of multiple roots would the equations not suffice. In such cases ABEL involved the calculus which could be used to produce a set of α independent equations for determining $a_1, \ldots, a_\alpha$. When ABEL divided $F(x)$ by $\prod_{\tau=1}^{\alpha}(x - x_\tau)$, he obtained another equation

$$F_1(x) = \frac{F(x)}{\prod_{\tau=1}^{\alpha}(x - x_\tau)} = 0$$

which was of degree $\mu - \alpha$, has as its roots $x_{\alpha+1}, \ldots, x_\mu$ and whose coefficients were rational functions of $x_1, \ldots, x_\alpha$ and $y_1, \ldots, y_\alpha$. Thus, ABEL concluded from *Main Theorem I* that any sum such as $\sum_{k=1}^{\alpha} \psi_k(x_k)$ could be expressed by a known function v and a similar sum of functions,

$$\sum_{k=1}^{\alpha} \psi_k(x_k) = v - \sum_{k=\alpha+1}^{\mu} \psi_k(x_k). \tag{19.29}$$

The relation $\gamma = \mu - \alpha$. The expression (19.29) at first might seem like a mere repetition of the *Main Theorem I*, but as ABEL stressed, the number of terms on the right hand side ($\mu - \alpha$) shows remarkable features. The stress put on the number γ is certainly one of the important aspects of ABEL'S paper, and it has received widespread mathematical interest not least after G. F. B. RIEMANN (1826–1866) transformed it into a coherent concept of *genus*, see section 19.3. For now, we focus our attention completely on ABEL'S argument and the inner logic of the paper.

As he had done above, ABEL again divided his argument by whether r has a factor independent of the indeterminates or not. He started with the latter case, $F_0(x) = 1$ for which he found that all the coefficient functions $q_0, \ldots, q_{n-1}$ were arbitrary and their $hq_k + 1$ coefficients had to correspond to the indeterminates,

$$\alpha = \sum_{k=1}^{n-1} (hq_k + 1) = \sum_{k=1}^{n} hq_k + n - 1.$$

Obtaining a corresponding formula for the other case, in which $F_0(x) \neq 1$, proved much more tedious. In general, ABEL claimed, the equation

$$r(x) = F_0(x) F(x) \tag{19.30}$$

would impose hF_0 conditions, but particular forms for y can eliminate some of these conditions.[35] If the number of conditions imposed by (19.30) is $hF_0 - A$, the number of indeterminates could be counted as

$$\alpha = \sum_{k=1}^{n-1} (hq_k + 1) - (hF_0 - A). \tag{19.31}$$

On the other hand, ABEL easily obtained from the definitions

$$hr = hF_0 + hF = hF_0 + \mu$$

and

$$hr = h\left(\prod_{k=1}^{n} \theta(y_k)\right) = \sum_{k=1}^{n} h\theta(y_k),$$

35 Here, a remarkably clear juxtaposition of "in general" and "particular cases" was given.

which allowed him to rewrite (19.31) as

$$\begin{aligned}\alpha &= \sum_{k=1}^{n-1} hq_k + n - 1 - (hr - \mu) + A \\ &= \sum_{k=1}^{n-1} hq_k - \sum_{k=1}^{n} h\theta\,(y_k) + n - 1 + A + \mu. \end{aligned} \tag{19.32}$$

Therefore, ABEL turned to algebraically manipulating the "degrees" $h\theta\,(y_k)$ along the same lines as had been followed in describing γ above.

Obviously, from the inequality

$$h\theta\,(y) \geq h\,(q_m y^m) \text{ for each } m = 0, \dots, n-1,$$

and the formula

$$h\,(q_m y^m) = hq_m + mhy,$$

ABEL obtained

$$h\theta\,(y_k) \geq hq_m + mhy_k \text{ for } k = 1, \dots, n.$$

Designating by ρ_τ the index of the maximal value of $h\,(q_m y^m)$ within the τ'th sequence of roots, ABEL employed the same machinery which had served him before, although this time in a slightly different notational dressing. Summing the excesses $\varepsilon_{\tau,k}$ within the τ'th sequence,

$$\sum_{k=1}^{n_\tau \mu_\tau - 1} \varepsilon_{\tau,k} = C_\tau,$$

ABEL found by the manipulations and number theoretic results

$$\mu - \alpha \geq \gamma - A + \sum_{\tau=1}^{\varepsilon} C_\tau,$$

or less specifically ($C_\tau \geq 0$)

$$\mu - \alpha \geq \gamma - A.$$

However, the inequality in (19.33) was actually an equality,

$$\mu - \alpha = \gamma - A, \tag{19.33}$$

as ABEL deduced by another tedious sequence of manipulations.

Specializing the relation expressed (19.33) to the other case (in which $F_0\,(x) = 1$) led to the result that if r did not contain any factors independent of the indeterminate quantities, then

$$\mu - \alpha = \gamma.$$

ABEL concluded these investigations by considering situations in which the coefficients $q_0, \dots, q_{n-1}$ were subjected to some kinds of conditions. Again arguing very "generally", ABEL could claim that the result (19.33) would not generally be substantially altered, although the constant A should reflect the additional conditions.

In the paper's eighth section, ABEL applied the results obtained thus far to calculate γ in an example for which $n = 13$.

$\mu - \alpha$ independent of α. In the previous argument, ABEL had reached an expression such as

$$hq_m = hq_{\rho_1} + M_m \text{ for } m = 0, \ldots, n-1$$

in which M_m is independent of hq_{ρ_1}. Thus, counting the number of coefficients in $q_0, \ldots, q_{n-1}$ yields as an upper limit for α

$$\alpha \leq nhq_{\rho_1} + \sum_{m=0}^{n-1} M_m.$$

ABEL could therefore write

$$\alpha = nhq_{\rho_1} + M,$$

in which M was independent of hq_{ρ_1}. Therefore, taken together with (19.32), $\mu - \alpha$ was found to be independent of the value of α.

ABEL summarized these results in an announcement of the central *Main Theorem II*:

> "The equation (74) [here (19.29)] enables us to express a sum of any number of given function of the form ψx by a sum of a particular number of functions. The last number can always be supposed equal to μ which — in general — will be its smallest value."[36]

In summary, the *Main Theorem II* can be expressed as follows:

Theorem 17 (Main Theorem II) *Under the present assumptions,*

$$\sum_{k=1}^{\tau} \int^{x_k} f(x_k, y_k)\, dx_k = \sum_{k=1}^{\gamma} \int^{z_k} f(x_k, y_k)\, dx_k + v$$

in which γ is independent of τ and $x_1, \ldots, x_\tau$, $z_1, \ldots, z_\gamma$ are given algebraically in $x_1, \ldots, x_\tau$ and v is an algebraic and logarithmic function. □

As his final act before turning toward the applications of this result (see discussion in section 19.5.2, below), ABEL generalized the theorem to apply to linear combinations of integrals with rational coefficients.

19.3 Additional, tentative remarks on ABEL's tools

As mentioned in section 19.1.2 and described at length above, ABEL'S *Paris memoir* is full of interesting applications of algebraic tools. In the present section, some brief perspectives on five of the most important tools are offered.

36 *"L'équation (74) nous met donc en état d'exprimer une somme d'un nombre quelconque de fonctions données, de la forme ψx, par une somme d'un nombre déterminé de fonctions. Le dernier nombre peut toujours être supposé égal à γ , qui, en général, sera sa plus petite valeur."* (N. H. Abel, [1826] 1841, 185).

The theory of residues and the expansion in decreasing power series. One of ABEL'S tools was to focus attention on the coefficient of $\frac{1}{x}$ in the expansion of the function $f(x)$ according to decreasing powers of x (see page 355). Judged by ABEL'S way of introducing the operation which he denoted Πfx, the expansion of the function f into series of decreasing powers was not considered problematic; my best guess is that it was considered a formal operation or a well established fact. However, simultaneously, CAUCHY was attributing new importance to the same object although in a completely different theoretical environment when he laid the foundations for his new calculus of *residues* in a series of papers in the *Exercises de mathématiques*.[37] From the quote on page 306, we know that ABEL bought and studied these installments. Thus, this mathematical object acquired importance in two distinct theories possibly from two very distinct approaches. However, when we include another of ABEL'S tools, we may get the impression that ABEL'S Π might stand closer to CAUCHY'S residues.

***Lagrange interpolation* and ABEL'S *Annales*-paper.** ABEL'S made repeated use of a result which derives from the process of *Lagrange interpolation*. In its original form, *Lagrange interpolation* concerned the problem of fitting a polynomial of degree $n-1$ through n specified points in the plane $\{(x_k, f(x_k))\}_{k=1}^n$. J. L. LAGRANGE (1736–1813) had attacked this problem in 1795 and presented the polynomial

$$P_n(x) = \sum_{k=1}^{n} f(x_k) \frac{\omega(x)}{(x - x_k)\, \omega'(x_k)}$$

as the solution;[38] the function $\omega(x)$ was defined by

$$\omega(x) = \prod_{k=1}^{n} (x - x_k)$$

and its derivative consequently satisfied

$$\omega'(x_k) = \prod_{m \neq k} (x_k - x_m).$$

This result can easily be proved and it immediately leads to the applications which ABEL made of it in the *Paris memoir*. Interestingly, CAUCHY pursued the same result with his new theory of residues and explicitly referred to the process as *Lagrange interpolation*.[39] In a short paper which ABEL published in GERGONNE'S *Annales de mathématiques*,[40] he deduced the same result by elimination in two polynomial equations. These remarks are meant to indicate that *Lagrange interpolation* was a powerful tool which was being investigated from numerous perspectives in the early nineteenth

37 See e.g. (A.-L. Cauchy, 1826).
38 (Kolmogorov and Yushkevich, 1992–1998, III, 262).
39 (A.-L. Cauchy, 1826, 35–37).
40 (N. H. Abel, 1827a).

century. I believe that the problem of elimination can be considered sufficient inspiration for ABEL to treat *Lagrange interpolation* but the very central role which it together with the operator Π played in the *Paris memoir* (see above) may also suggest that ABEL had actually picked up his tools from CAUCHY'S new theory of residues although he did not adapt the full theory.

The degree-operator. ABEL'S arguments about the number of independent integrals γ relied extensively on ways of determining the number of independent (or free) coefficients. In turn, their determination was based on an extended degree operator which could apply to the implicitly defined algebraic functions with which he dealt. ABEL introduced the fractional degree operator hR as the highest exponent in a development of R according to decreasing powers. As was the case with the residue-operator, no explicit considerations as to the validity of this definition are presented and it appears to be a well known, formal trick. ABEL presented the basic rules for the degree operator and when he wanted to apply it to differences between to expressions among $y_1, \ldots, y_n$, he insisted that these expressions be ordered according to their degree. However, as observed on page 362, particular cases could still arise in which the identity

$$h\left(y_m - y_k\right) = \max\left\{hy_m, hy_k\right\} \tag{19.34}$$

did not hold. Such cases were apparently peculiar situations of little interest, and ABEL dismissed them by claiming that the equation (19.34) would hold "in general". In his comments,[41] SYLOW made a considerable effort in clarifying ABEL'S arguments and in particular in revising his deduction of the properties of γ by making explicit some of the assumptions which ABEL had not made when he simply argued "in general". The notion of equations being "generally valid" will be addressed further in chapter 21 where it was be interpreted and explained based on the notion of *formula based mathematics*.

The genus. The number which ABEL denoted γ expressed the number of independent integrals related to a particular algebraic differential. As we have seen, ABEL'S deduction of the invariance of the number γ was cumbersome and hampered by certain points where it was not completely clear and rigorous. Furthermore, although his arguments were highly explicit they did not immediately produce a way of generally computing the number γ. In the subsequent decades, it became an extremely prominent mathematical problem to rigorously establish the basis for ABEL'S theorems and to investigate the number γ further. Eventually, RIEMANN presented an approach based on multi-sheeted surfaces and introduced the name *genus* and the symbol p for ABEL'S γ.[42] The further description of RIEMANN'S theory is, unfortunately, way be-

41 (N. H. Abel, 1881, II).

42 (B. Riemann, 1857).

Figure 19.1: GEORG FRIEDRICH BERNHARD RIEMANN (1826–1866)

yond the present scope.[43] However, in the present context, it serves to demonstrate that ABEL'S ideas were pursued and rigorized over the ensuing decades.

Birth of a new concept: algebraic functions. The final aspect which will be tentatively discussed here concerns the introduction of a new concept of *implicitly defined algebraic functions*. In his research on the solubility of equations, ABEL had — for obvious reasons — only been interested in investigating *explicit* algebraic functions which could serve as solutions for equations. However, in the *Paris memoir*, *implicit* algebraic functions were the primary objects of study and ABEL developed a number of tools for their investigation. Later, implicitly given algebraic functions (or just algebraic functions) became very important objects of mathematics and — as BRILL remarks — ABEL may be seen as the initiator of the theory of algebraic functions; in particular, he proved a very important theorem in the theory.[44] Later, J. LIOUVILLE (1809–1882) also adopted — following ABEL but departing from his French precursors — the implicitly given algebraic functions into his theory of integration in finite terms.[45] In summary, we are faced with an introduction of a new set of objects, and ABEL'S highly formula based investigations of these objects and developments of tools for their study can be interpreted as a way of getting to know and express results about this new branch.

43 One rather recent, very brief sketch is given in (Houzel, 1986, 310–313).
44 (Brill and Noether, 1894, 212).
45 (Lützen, 1990, 370).

19.4 The fate of the *Paris memoir*

ABEL'S *Paris memoir* had a strange and adventurous fate which had certain influences on ABEL'S career.[46] After ABEL had delivered his *memoir* to the *Académie des Sciences*, the Academy commissioned A.-M. LEGENDRE (1752–1833) and CAUCHY to present a report on it. The manuscript soon landed on CAUCHY'S desk, where it withered until 1829. After having learned of ABEL'S untimely death from a communication on 22 June 1829 by LEGENDRE,[47] CAUCHY finally took the time to write the report which was dated 29 June 1829.[48] CAUCHY explicitly noticed in his report how ABEL treated implicitly defined algebraic functions and that this was a necessary requirement for his theorems to be true.[49] This is further indication that the introduction of implicitly given algebraic functions was not completely obvious and standard.

Manuscript lost and found. Based on CAUCHY'S positive report, the *Académie des Sciences* decided to include the ABEL'S *Paris memoir* in the *Mémoires présentés par divers savants* once a new copy had been prepared. Furthermore, ABEL was — posthumously and jointly with C. G. J. JACOBI (1804–1851) — awarded the *Grand prix* of the *Académie des Sciences* in 1830. However, the manuscript was misplaced — possibly as a result of the turbulent events in Paris in 1830 — as a Norwegian enquiry would realize in 1832 when B. M. HOLMBOE (1795–1850) was beginning to prepare the first edition of ABEL'S *Œuvres*. Consequently, the *Œuvres* appeared in 1839 without the *Paris memoir*. Apparently, this provoked some reaction from the [Académie des Sciences]Académie which commissioned LIBRI with the job of seeing it through print and, as noted, it was published in 1841.

While preparing the second edition of ABEL'S collected works, M. S. LIE (1842–1899) in 1874 obtained permission to consult the original manuscript supposedly held in the archives of the *Académie des Sciences*.[50] However, the manuscript was again nowhere to be found in the archives and again had to be considered lost. Consequently, the *Paris memoir* was included in the second edition of ABEL'S collected works but with the version printed by the French Academy in 1841 as the source. In the twentieth century, a copy of the manuscript was first located in Rome by P. HEEGAARD (1871–1948) in 1942 before V. BRUN (1885–1978) [51] succeeded in finding the majority of ABEL'S original manuscript in Florence ten years later. Recently, in 2000, the final eight missing pages have been found and the whereabouts of the entire original manuscript of ABEL'S *Paris memoir* are known for the first time in 150 years.

46 The fate of the *Paris memoir* is described in most biographies of ABEL. Additionally, information for the present sketch is drawn from papers including (Brun, 1949; Brun, 1953; Lange-Nielsen, 1927; Lange-Nielsen, 1929).
47 (Lange-Nielsen, 1929, 14).
48 (Lange-Nielsen, 1927, 67).
49 CAUCHY'S report is reproduced in (ibid., 69).
50 (N. H. Abel, 1881, II, 294)
51 Information from (Scriba, 1980).

19.5 Reception of the *Paris memoir*

Some aspects of the reception of ABEL'S *Paris memoir* have already been touched upon above when it was noted how ABEL introduced a new class of objects into mathematical study. In the present section, some attention is paid to the primary application of the *Paris memoir* which dealt with the so-called *hyperelliptic integrals* .

19.5.1 ABEL's announcements of the *Paris memoir*

In response to the lacking reaction from the Parisian *Académie des Sciences*, ABEL went ahead and made public some of his results originally contained in the *Paris memoir*. He did so in two papers published in A. L. CRELLE'S (1780–1855) *Journal* in 1828 and 1829.[52] The first of these contained the application of the *Paris memoir* to hyperelliptic integrals and will be treated in some detail below. The second one entitled "*Démonstration d'une propriété générale d'une certaine classe de fonctions transcendantes*" had been signed by ABEL January 6, 1829 and was published in the second issue of the fourth volume of the *Journal* which appeared just days before ABEL'S death. In that paper, written in haste by an already ill-taken mathematician, ABEL presented the first main theorem of the *Paris memoir* and gave a short and direct proof based on the theorem of LAGRANGE (theorem 1) which had already served him well in the theory of equations. At the end of the two page proof, ABEL promised to present multiple applications of *Main Theorem I* on a later occasion.

19.5.2 Application to hyperelliptic integrals

The application to hyperelliptic integrals has been seen by some historians of mathematics as the true motivation for ABEL'S deduction of the *Abelian Theorem*.[53] Certainly, the so-called hyperelliptic integrals figured prominently among ABEL'S examples in the *Paris memoir* when he applied the results to integrals of the form

$$\int f\left(x, \sqrt{\phi_n(x)}\right) dx$$

in which f was a rational function and P_n was a polynomial of degree n. These integrals are separated from the greater class considered in the *Paris memoir* by corresponding to an equation of the form $\chi(x, y) = y^2 - \phi_n(x)$.

For ordinary elliptic integrals ($n = 3$ or $n = 4$), ABEL'S calculations in the *Paris memoir* showed that $\gamma = 1$ and thus, any sum of similar elliptic integrals could be reduced to a single elliptic integral of the same form and possibly some algebraic and logarithmic terms. For hyperelliptic integrals, ABEL found that $\gamma = \left\lfloor \frac{n-1}{2} \right\rfloor$ where $\lfloor x \rfloor$ denotes the integer value of x. Thus, for instance, if $n = 6$, any number of similar

52 (N. H. Abel, 1828c; N. H. Abel, 1829b).

53 See (Brill and Noether, 1894, 210–211) and the discussion in (Cooke, 1989).

hyperelliptic integrals could be reduced to *two* such integrals and simpler terms. This was also one of the main results of the paper *Remarques sur quelques propriétés générales d'une certaine sorte de fonctions transcendantes* which ABEL published in CRELLE'S *Journal* in 1828.[54]

ABEL'S result was picked up by JACOBI who praised it highly and suggested that it could be used as a form of generalized addition theorem for *hyperelliptic functions* . Thus, for hyperelliptic integrals, say, with X of degree 5,

$$\int_0^x \frac{dx}{\sqrt{X}} = \Phi_1(x) \text{ and } \int_0^x \frac{x\,dx}{\sqrt{X}} = \Phi_1(x),$$

the problem became to express the upper limits of the involved integrals as

$$x = \lambda(u, v) \text{ and } y = \lambda_1(u, v)$$

such that

$$\Phi(x) + \Phi(y) = u \text{ and } \Phi_1(x) + \Phi_1(y) = v.$$ [55]

This idea — which is a generalization of the addition theorems for trigonometric and elliptic functions — is called the (Jacobian) inversion problem and it attracted a great deal of attention in the 1830s and 1840s. Eventually, around 1850, it was solved independently by A. GÖPEL (1812–1847) and J. G. ROSENHAIN (1816–1887) who applied a generalization of JACOBI'S theory of theta functions for elliptic functions (see next chapter) to this case.[56] In the process, they were forced to accept that the functions were depended on two complex variables and had four periods. Thus, the search to generalize the theory of basic pattern of the theory of elliptic integrals even further forced mathematicians to face even more peculiar functions. The continuing extension of the theory of higher transcendentals posed new demands to the rigor and tools of the mathematician and thereby influenced the general development of mathematics in important ways.

19.6 Conclusion

In the present chapter, ABEL'S *Paris memoir* has been described in some details to facilitate a discussion of the tools which ABEL employed. Combining the characterization of the tools employed here with those described in the previous chapter, we are led to see that *algebraic* methods were extremely important in ABEL'S research on transcendentals. These methods range from results which he had originally used to study the solubility of equations to newly developed tools which, nevertheless, also often were based in polynomials, equations, considerations of rational dependence, and the like.

54 (N. H. Abel, 1828c).
55 (C. G. J. Jacobi, 1832b, 400); see also (Houzel, 1986, 311).
56 (Göpel, 1847; Rosenhain, 1851).

As described, ABEL'S deductions in the *Paris memoir* were extremely cumbersome and not always universally permitted. Generally, ABEL'S arguments in the *memoir* fell well inside the *formula based paradigm*: they were concerned with formulae, were carried by manipulations of formulae, and arguments which were not universally true but only true "in general" were acceptable. In chapter 21, these features will be shown to be intimately connected with an "old" paradigm which was gradually being replaced in the period. Thus, and because it was not available for 15 years, it is not surprising to find that ABEL'S proof in the *Paris memoir* was not so widely accepted and imitated as the statements or results to which they led.

Chapter 20

General approaches to elliptic functions

The present chapter serves to present the ideas which matured the theory of elliptic functions in the nineteenth century. In particular, attention is called to the variations in the ways of introducing elliptic functions because these variations are indicators of the changing conceptions of rigorous foundations. Moreover, it is interesting to follow, how results are turned into definitions to meet the changing standards of rigorous definitions.

20.1 ABEL's version of a general theory of elliptic functions

In his ultimate publication, the *Précis d'une théorie des fonctions elliptiques*,[1] N. H. ABEL (1802–1829) addressed the theory of elliptic functions from a more general perspective than the *Recherches*. Since the *Recherches* which dealt exclusively with elliptic functions of the first kind, ABEL had published a number of smaller papers on the theory of elliptic functions, some investigations on integration in finite form, and various announcements of his *Paris memoir*. ABEL had also been working on a continuation of the *Recherches* which he postponed to devote his energy to completing the *Précis*. The second *Recherches mémoire* was eventually published by M. G. MITTAG-LEFFLER (1846–1927) in 1902.[2] However, to focus on the most general of ABEL'S approaches, we shall limit the discussion to the *Précis*. In the *Précis*, these threads were woven together to present an exposition of the entire theory of elliptic functions which simultaneously addressed all three kinds.

1 (N. H. Abel, 1829d).
2 (N. H. Abel, 1902c).

Reiterating established knowledge. ABEL began his *Précis* — which he privately called the "knockout of C. G. J. JACOBI' (1804–1851)' — by iterating some of his previous results pertaining to elliptic functions of the first kind. With the radical

$$\Delta(x,c) = \sqrt{(1-x^2)(1-c^2x^2)},$$

he introduced his three kinds of elliptic integrals as

$$\tilde{\omega}(x,c) = \int \frac{dx}{\Delta(x)},$$
$$\tilde{\omega}_0(x,c) = \int \frac{x^2\,dx}{\Delta(x)}, \text{ and}$$
$$\Pi(x,c,a) = \int \frac{dx}{\left(1-\frac{x^2}{a^2}\right)\Delta(x,c)}.$$

One of the major tricks of the *Précis* was that once the elliptic integral of the first kind had been inverted

$$\theta = \int_0^{\lambda(\theta)} \frac{dx}{\Delta(x,c)},$$

this elliptic function could be used to describe the elliptic integrals of the second and third kind,

$$\tilde{\omega}_0(x,c) = \int \lambda^2(\theta)\,d\theta \text{ and } \Pi(x,c,a) = \int \frac{d\theta}{1-\frac{\lambda^2\theta}{a^2}}.$$

Thus, ABEL had essentially reduced the problem of inverting all three kinds of integrals and he could combine the study of all elliptic functions in knowledge about the elliptic functions of the first kind.

Obviously, among the key results which ABEL iterated for the elliptic function λ was its two periods and the complete solution of the equation $\lambda(\theta') = \lambda(\theta)$ which we have already encountered multiple times. Moreover, ABEL also presented various infinite representations of λ and investigated the conditions of transformations. Thus, all the key components of his previous approaches were included in the *Précis*, albeit in a more coherent and lucid form.

General properties of elliptic functions. The major new purpose of ABEL'S *Précis* was to investigate a new program of representation for elliptic functions. In the process, ABEL made important use of the insights which he had developed and presented in relation with his *Paris memoir*.

With two polynomial functions f (even) and ϕ (odd), ABEL defined

$$\psi(x) = f(x)^2 - \phi(x)^2\,\Delta(x)^2 \tag{20.1}$$

which was an even function and therefore could be split in factors as

$$\psi(x) = A\prod_{n=1}^{\mu}\left(x^2 - x_n^2\right).$$

In this situation, ABEL found by employing *Lagrange interpolation* that the sum of integrals of the third kind reduced to a logarithmic expression

$$\sum_{n=1}^{\mu} \Pi(x_n, a) = C - \frac{a}{2\Delta(a)} \log \frac{f(a) + \phi(a)\Delta(a)}{f(a) - \phi(a)\Delta(a)}. \tag{20.2}$$

ABEL extended this property of elliptic integrals of the third kind to analogous results for elliptic integrals of the first and second kind. For integrals of the second kind, ABEL observed that $\tilde{\omega}(x) = \lim_{a\to\infty} \Pi(x, a)$ whereas the logarithmic term vanished under this limit process,

$$\sum_{n=1}^{\mu} \tilde{\omega}(x_n) = C.$$

For integrals of the second kind, ABEL considered the expansion of (20.2) according to increasing powers of $\frac{1}{a}$ and compared coefficients of $\frac{1}{a^2}$ to conclude

$$\sum_{n=1}^{\mu} \tilde{\omega}_0(x_n) = C - p$$

where p was an algebraic function of $x_1, \ldots, x_\mu$.

Thus, ABEL used tools similar to those which he employed in the *Paris memoir* to deduce results which also bear similarities with the *Main Theorem I* (theorem 16).

A new program of representability. In the second chapter, ABEL suggested a very general question: He wanted to describe all integrals of algebraic differentials which could be expressed by algebraic, logarithmic, and elliptic functions. Thus, if compared with the *Paris memoir*, the elliptic functions were now accepted among the *basic* functions to which other higher transcendentals could be reduced. However, when he came to answer the question, he restricted himself to attack transformation problems and other relations among elliptic integrals. The more specific contents of his investigations are considered outside the present scope, although its presentation would probably reiterate the description of ABEL'S methods and tools which have been suggested in the previous chapters.

ABEL did not manage to complete his papers before he died. Nevertheless, his approach illustrated the fruitful influence which the results of *Paris memoir* could have on the theory of elliptic functions. However, because of his early death, it was left to ABEL'S contemporaries and competitors to outline the future development of the theory of elliptic functions.

20.2 Other ways of introducing elliptic functions in the nineteenth century

During the nineteenth century, the definitions of elliptic functions were turned upside down a number of times. In chapter 16, it has been described, how ABEL introduced elliptic functions (of the first kind) by a formal inversion of the corresponding elliptic integral and an extension to the complex domain. However, his contemporaries and successors soon found other ways of introducing elliptic functions more preferably. In the present context, it suffices to consider three different approaches.

JACOBI. With ABEL out of the competition, JACOBI'S influence on the theory of elliptic functions toward the end of the 1820s was overwhelming. JACOBI'S notation and means of introducing the functions became standardized through a number of publications starting with his *Fundamenta nova* which was the first monograph devoted to the study of the elliptic functions.[3] In the *Fundamenta nova*, JACOBI defined elliptic functions as inverses of elliptic integrals but beginning with a course taught in 1838, the changed the foundation to the so-called theta-functions. These were a particular set of four exponential series the simplest of which can be written as $\vartheta(x) = \sum_{n\in\mathbb{Z}} (-1)^n q^{n^2} e^{2nix}$.[4] Based on these series, JACOBI could introduce and investigate all parts of the theory of elliptic functions. Thus, JACOBI introduced series as the basic objects upon which everything else should be built. Other definitions by series, e.g. as ratios of power series were also being suggested and adopted.

J. LIOUVILLE (1809–1882). A completely different approach to the introduction of elliptic functions was taken by LIOUVILLE who chose to develop an entire theory for doubly periodic functions. Of such functions, he was able to deduce a number of results and eventually prove that they could be used to represent the inverses of elliptic integrals.[5] Thus, LIOUVILLE circumvented the approach of ABEL and JACOBI and investigated the concept of functions defined by what ABEL had deduced as a *property*. The strength of the approach was that eventually, the classes of elliptic functions and doubly periodic, meromorphic functions were found to coincide.

K. T. W. WEIERSTRASS (1815–1897). The final approach which I wish to mention was through differential equations. For instance, WEIERSTRASS introduced his function $\wp(u)$ as the solution to the differential equation

$$\left(\frac{dy}{du}\right)^2 = 4y^3 - g_2 y - g_3 \text{ for } g_2, g_3 \text{ constants}$$

which had a pole at $u = 0$.[6] Subsequently, WEIERSTRASS found means of obtaining more direct representations of his elliptic functions, for instance in the form

$$\wp(v) - \wp(u) = \frac{\sigma(u+v)\,\sigma(u-v)}{\sigma^2 u \cdot \sigma^2 v}$$

where the function σ could be expanded in an infinite product.[7]

3 (C. G. J. Jacobi, 1829).
4 (Houzel, 1986, 304–305).
5 (Lützen, 1990, 555).
6 (Houzel, 1986, 298).
7 (ibid., 306).

All these four definitions ultimately define the same objects and it is a major sign of strength of the theory over the century that so many different approaches had been described. Mathematicians were free to chose which of the definitions satisfied their requirements for usability and rigor; subsequently, the other representations could be deduced.

20.3 Conclusion

In the present part, ABEL'S approach to the theory of elliptic functions has been described from a number of perspectives. Based on a presentation of aspects of the theory of elliptic integrals in the eighteenth century, it has been illustrated, how ABEL introduced elliptic functions as formal inverses of elliptic integrals and extended the resulting function to the complex domain. The main inspiration which ABEL drew from C. F. GAUSS' (1777–1855) suggestion that the division problem from the circle could also be attacked for the lemniscate has also been described. With the formal inversion as his definition, ABEL sought for ways of representing his elliptic functions, and some aspects of this program such as the standards of rigor and the use and requirements of representations have been addressed. Subsequently, special attention has been devoted to illustrating certain aspects of the tools which ABEL applied in the theory of transcendentals. In particular, it has been documented, how ABEL made repeated and prolific use of algebraic methods resembling those which he had employed in his research on the solubility of equations. Finally, the changing roles of definitions and representations have been briefly debated in the present chapter.

In conclusion, ABEL'S research on elliptic functions and higher transcendentals was his most directly influential legacy. Culminating in the later formulation of JACOBI, ABEL'S *Paris memoir* suggested a problem which attracted mathematicians for decades and influenced the development of mathematics in the entire nineteenth century. In the next and final chapter, aspects of ABEL'S own research on elliptic functions will serve to illustrate how he sometimes worked within the *formula based paradigm*.

Part V

ABEL's mathematics and the rise of concepts

Chapter 21

ABEL's mathematics and the rise of concepts

In the preceding parts, I have presented and discussed the main parts of N. H. ABEL'S (1802–1829) mathematical production ranging from the theory of equations over the installation of rigor in the theory of series to the exploding field of elliptic and higher transcendentals. In addition, in each part, I have simultaneously addressed three broader themes: the rise of new questions with new kinds of answers, the change in the standards of doing mathematics, and a change in the objects and methods of mathematics.

In this concluding part, I unify these themes by arguing that they are signs of a rise of *concept based mathematics*. In four steps, it will be argued that a large part of the development in mathematics in the early nineteenth century can appropriately be analyzed from the perspective of a change in the objects with which mathematics dealt. Firstly, the change is introduced by defining and discussing the "paradigms" of *formula based mathematics* and *concept based mathematics*. Secondly, these concepts are employed to present a brief analysis of how the ways of introducing objects into mathematics developed in the early nineteenth century. Thirdly, the changing role of counter examples in the two paradigms is discussed in some details. Finally, some reservations to the analysis are presented before a conclusion is drawn. Due to the limited scope of ABEL'S mathematical production, my frame of interpretation can only be preliminary and I hope to develop it further through subsequent research by involving the works of other mathematicians.

21.1 From formulae to concepts

It seems fair to state that in the early nineteenth century, mathematics changed quite dramatically.[1] Any comparative and contextualized reading of the works of, say, L.

1 Whether or not the early nineteenth century is an apt periodization in the history of mathematics has been discussed, though. See e.g. (Mikulinsky, 1982; Otte, 1982).

EULER (1707–1783) and K. T. W. WEIERSTRASS (1815–1897) or G. F. B. RIEMANN (1826–1866) will reveal that the problems, methods, and styles of analytical mathematics developed immensely and changed fundamentally during the century from the 1750s to the 1850s. In my view, a large part of the change in mathematics in the period can be understood by analyzing a fundamental change in the basic objects of the combined discipline of algebra and analysis.

My description of changes in mathematics will — dictated by the scope of the previous parts — mostly deal with the development of the algebraic and analytic disciplines. In section 21.4, the applicability of the analyses outside these disciplines is briefly addressed.

It will be argued that mathematics in the eighteenth century was tied to *formulae* and that mathematicians worked within a framework which was — in essential ways — adapted to these objects. In the early nineteenth century, so it is argued, these basic objects were gradually replaced by *concepts* and the change was so fundamental that it influenced all layers of mathematical knowledge and knowledge production.

To allow for a more precise discussion, tentative definitions of the two styles (paradigms) of mathematics are given below. I have adopted the excessively broad *Kuhnian* term "paradigm" to include the entire mental horizon of the group of mathematicians who worked in the tradition. At the same time, I have introduced catch-word characterizations of the paradigms by terming them *formula based* and *concept based* . Below, the paradigms and their relations will be discussed further and certain relevant aspects of the preceding presentation of ABEL'S mathematics will be analyzed.

21.1.1 The Eulerian paradigm of *formula based mathematics*

By *formula based mathematics*, I mean to indicate a paradigm prevalent in the eighteenth century in which formulae were the carriers of mathematical knowledge. Formulae were both the results and the methods of mathematics, and mathematicians thought *about* and *in terms of* formulae. Mathematical results were derived through strings of explicit, formal manipulations of representations (formulae) and were stated in terms of new formulae.

The essential notion of *formula* can be thought of as representations of mathematical objects by symbols. However, such interpretations tend to be anachronistic and beside the point because — as I shall argue — the formulae were the *basic* objects of mathematics and only gradually became representations of other objects.[2]

In analysis, the primary occurrence of formulae was in the form of functions; the study of functions had been based on the study of their algebraic formulae. For these reasons, this paradigm could also have been named *function based mathematics* if it

2 In the seventeenth and part of the eighteenth century, formulae had also been representations of e.g. curves (see section 15.1). However, as described, they became the primary objects in EULER'S new version of analysis.

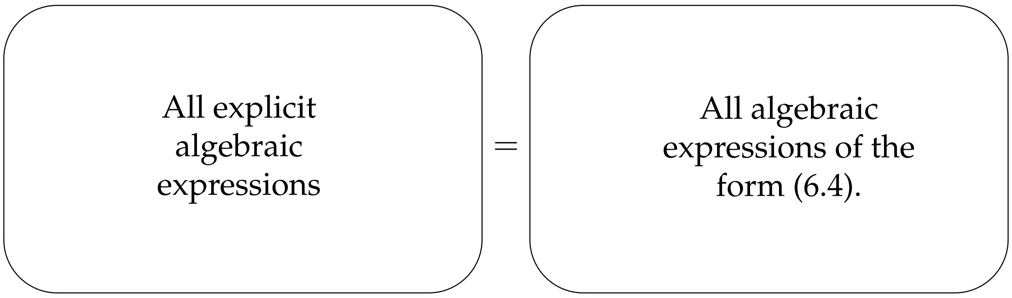

Figure 21.1: The equality of the concepts of explicit algebraic expressions and ABEL's normal form.

was only to apply in analysis. However, in the algebraic discipline such a description would be misguided precisely because not functions but formulae were at the centre of mathematical reasoning (see e.g. section 5.2). If any other name should be used for formula based mathematics, the term *Eulerian paradigm* might be well suited.

For the present purpose, formula based mathematics is best thought of in terms of e.g. EULER's introduction and manipulation of various algebraic expressions in analysis. ABEL's mathematics also frequently exhibits key characteristics of this paradigm, e.g. in his manipulations of formulae in the *Recherches* or the latter part of the binomial paper (see chapter 17 and 12, respectively). On both these occasions, ABEL based his deductions on sequences of step-wise manipulations of formulae to obtain results which were, themselves, formulae.

21.1.2 A new paradigm of *concept based mathematics*

The anti-thesis to formula based mathematics in the present context is termed *concept based mathematics*. In analogy with the formula based version, this paradigm emphasized thought in and about concepts by which I mean classes of objects. The concept based mathematics deals primarily with defining, representing, and relating concepts. The collection of objects which fall under a concept is called the *extension* or *domain* of the concept.

Typically, concept based mathematics could be concerned with e.g. *continuous functions*, *differentiable functions*, or *algebraically solvable equations*. The mathematical theorems dealing with concepts would then contain results relating these, e.g. by pointing out their differences or their overlaps or by relating one concept to another. In a truly concept based approach to mathematics, even representations become theorems relating concepts; ABEL's deduction of the normal form for (explicit) algebraic expressions stated that the two concepts were identical (see figure 21.1, section6.3, and below).

For concept based mathematics to be efficient, specific knowledge of the individual objects within a concept has to fade in importance. Individual objects would serve important roles as examples and counter examples but the definitions of the concepts must possess qualities which make them useful and central in the investigation of the concept. Such investigations, in turn, can benefit from the shift of focus onto concepts and produce results which were impossible (or very difficult) if only individual objects were considered. Thus, in order to analyze concept based mathematics, the role of definitions, representations, and arguments of relation and delineation of concepts become key points of enquiry.

21.1.3 The shift from formula based to concept based mathematics

The purpose of introducing the paradigms of formula based and concept based mathematics is to characterize the development in the early nineteenth century as a transition from the former to the latter. This transition manifests itself in various ways which interact with the changing basic objects of mathematics. The questions asked, the tools employed to answer these questions, and the types of answers which are possible and expected all change as consequences of this shift.

In the first half of the nineteenth century, some mathematicians were aware that their style of mathematics differed essentially from the standards of their time. ABEL expressed how heavy loads of computations could hamper the progress of research,[3] and E. GALOIS (1811–1832) described his own works as "analysis of analysis" which would reduce the hitherto dominating calculations to particular cases.[4] This awareness of the transition grew stronger during the century and towards the end of the nineteenth century, mathematicians became increasingly explicit about it. For instance, F. RUDIO (1856–1929) wrote:

> "The essential principle of the newer mathematical school, which is established by Gauss, Jacobi, and Dirichlet, is that whereas the older one sought to reach the goal by lengthy and complicated calculations (as even still in Gauss' Disquisitiones) and deductions — it comprises an entire field by avoiding those and applying a genius method in a main idea and simultaneously presents the end result in its highest elegance by a single strike. While the former [the older approach] after a long sequence eventually reached a fertile ground by progressing from theorem to theorem, the latter [the new approach] immediately produces a formula in which the complete sphere of truths of an entire field is compactly contained and only ought to be extracted and expressed. In the old way, one could also — if need be — prove theorems; but only now can the true nature of the entire theory be seen, its internal gears and wheels."[5]

3 (N. H. Abel, [1828] 1839, 217–218).

4 (Galois, 1831c, 11).

5 "*Das wesentliche Princip der neueren mathematische Schule, die durch Gauss, Jacobi und Dirichlet begründet ist, ist im Gegensatz mit der älteren, dass während jene ältere durch langwierige und verwickelte Rechnung (wie selbst noch in Gauss' Disquisitiones) und Deduktionen zum Zweck zu gelangen suchte, diese mit Vermeidung derselben durch Anwendung eines genialen Mittels in einer Hauptidee die Gesammtheit eines ganzen Gebietes umfasst und gleichsam durch einen einzigen Schlag das Endresultat in der höchsten Eleganz darstellt. Während jene, von Satz zu Satz fortschrei-*

RUDIO clearly expressed the transition; the *formula* which he describes as the product of the new approach is not a formula in the present sense but rather a *theorem*. A number of similar statements can be found by other late-nineteenth century mathematicians, e.g. C. F. KLEIN (1849–1925) who described how G. P. L. DIRICHLET (1805–1859) would avoid long computations in favor of acute logical analyses.[6]

This shift in the roles of formulae and concepts has been noticed and investigated from slightly different perspectives by historians of mathematics. In particular, it has been addressed by H. N. JAHNKE (⋆1948) and by D. LAUGWITZ (1932–2000), who pointed to the significant influence which RIEMANN had in bringing about the eventual change of paradigms.[7]

The basic objects of mathematics. The definitions of the paradigms suggest that the purported shift from formula based to concept based mathematics was a question of the size of the domains of mathematical results. Interpreted purely as a change in domains, the new approach could be seen as consisting of results which are simultaneously true for a number of objects of the old paradigm (formulae). However, there is more to the transition that this; it concerns a real and fundamental change from formulae to concepts as the basic objects of mathematics. In the one extreme, a manipulation of a particular algebraic formula might produce another algebraic formula which would then be a mathematical result. At the other end of the spectrum, a number of results developed in the nineteenth century pointed out the differences between important concepts such as continuous and differentiable functions or proved that particular classes of functions could be represented in particular ways. The ability to state and prove results for abstractly defined classes of objects is one of the main aspects of the rise of concept based mathematics. Similarly, the issues of *relating concepts* and *representing concepts* are two of the central topics in a fully fledged concept-based version of mathematics.

The techniques and questions of mathematics. Connected to the transition in the basic objects of mathematics, the techniques and questions of mathematics also underwent fundamental changes. The types of questions asked and the methods for answering them were not the same in the two paradigms. In the formula based paradigm, mathematical texts could be made up of long sequences of manipulations which transformed one formula into others or answered particular questions by de-

tend, nach einer langen Reihe endlich zu einigem fruchtbaren Boden gelangt, stellt diese gleich von vorn herein eine Formel hin, in welcher der vollständige Kreis der Wahrheiten eines ganzen Gebietes konzentriert enthalten ist und nur herausgelesen und ausgesprochen zu werden darf. Auf die frühere Art konnte man die Sätze zwar auch zur Not beweisen, aber jetzt sieht man erst das wahre Wesen der ganzen Theorie, das eigentliche innere Getriebe und Räderwerk." (F. Rudio, 1895, 894–895).

6 (Klein, 1967, 250).

7 See e.g. (Jahnke, 1987) and (Laugwitz, 1999, 293–340). These are both very interesting works dealing with discussions similar to the present one but from slightly different perspectives.

veloping formulae which "solved" them. At times, concept based mathematics could apply the manipulation of "representations" provided that some representation result made it relevant. But more typically and interestingly, in concept based mathematics, statements about the extension of a concept grew to become the key results of mathematics. A particularly illustrative example of these questions has been presented in part II where ABEL'S research on the quintic first showed that not all polynomial equations were solvable, i.e. the concepts of *polynomial equation* and *algebraically solvable equations* were distinct, although related. Later in his research, ABEL'S proof of the algebraic solubility of *Abelian equations* was another almost prototypical concept based result, at least in its final formulation. This result showed that the concept of *Abelian equations* was contained in the concept of *algebraically solvable equations*. When he first encountered *Abelian equations* in connection with the division problem (see section 16.3), ABEL'S argument relied extensively on his particular knowledge of the individual objects and was thus much more formula based.

The styles of mathematics. Not surprisingly, the changing techniques of mathematics manifested themselves at the textual level. Because formulae had been the carriers of knowledge and argument in the formula based paradigm, mathematical publications relied extensively on the powers of formulae and mathematical texts could be dominated by strings of explicit manipulations of formulae. Eventually, a conclusion could be stated in the form of a theorem. In the concept based paradigm, a Euclidean style with its emphasis on definitions, theorems, and proofs became the customary style of written mathematics. This presentational style emphasized the precise statement of assumptions and the internal relations between concepts and theorems.

A revolution? When a change of paradigms is involved, the question of *revolutions* naturally arises. According to the recent debate, revolutions in mathematics appears not to be the most apt scheme of interpreting the history of the discipline.[8] In particular, the requirements of incommensurability seems to prohibit revolutions appearing in mathematics because the truth status of mathematical statements apparently never changes. This also seems to apply to the change of paradigms discussed here. During and after the transitional period, mathematicians devoted an effort to reconstructing and re-interpreting the established knowledge to make it fit into the new system. This is particularly visible in analysis where A.-L. CAUCHY'S (1789–1857) deliberate redefinition of basic notions and priorities changed the status of certain results and perceptions. As a result, men like ABEL sought to refound the theory in such a way that absolute truth was retained by making explicit the domains of validity for the statements. This process can be called *critical revision* and its general success precludes revolutions in mathematics. In section 21.3, the role of counter examples in the early nineteenth century is invoked to shed some light on this discussion.

[8] See primarily the articles in (Gillies, 1992).

21.2 Concepts and classes enter mathematics

As the basic objects of mathematics went from formulae to concepts, new methods and standards for introducing the objects were developed and the internal purpose of mathematical research also changed.

21.2.1 Defining concepts

While an object in the formula based paradigm could be introduced by merely exhibiting its formula, the introduction of concepts into the concept based paradigm required more sophisticated methods. However, these methods were not necessarily new — a number of them had been around since the births of the Euclidean style of mathematics and Aristotelian logic in Ancient Greece. In the present context, two aspects of the new importance given to definitions deserve special attention. First, genetic definitions and nominal definitions are discussed and their interactions described. Second, the introduction of concepts with special properties through careful definitions is emphasized.

Genetic and nominal definitions. Concepts were often introduced by either genetic or nominal definitions. A genetic definition consists of prescribing the way the concept is constructed from other, simpler concepts whereas a nominal definition simply associates a name to something. Typical examples of a genetic definitions in the present material include EULER'S definition of functions and ABEL'S definition of explicit algebraic expressions (see sections 10.1 and 6.3, respectively). These two examples also illustrate a very important difference in defining concepts: EULER'S definition was purely nominal whereas ABEL put his definition to essential use in obtaining his normal form of explicit algebraic expressions.[9] Nominal definitions were being discussed in the early nineteenth century but the debate mainly centered on the ancient question whether or not nominal definitions implied the existence of any objects under the concept being defined.[10] The main objection against nominal definitions from a concept based paradigm could have been that they were not useful in obtaining knowledge of the concept being defined.[11]

Definition by desired property. The ultimate way of associating knowledge through definitions would be to let properties serve as definitions. In a sense, this is the final lesson of I. LAKATOS' (1922–1974) *Proofs and Refutations*: the polyhedra which satisfy the Eulerian formula are collected as a concept and called Eulerian polyhedra and

9 See e.g. (Laugwitz, 1999, 311) and section 6.3.

10 Among the mathematicians involved were GERGONNE and OLIVIER. See e.g. (Otero, 1997, 74–81) and (Olivier, 1826c).

11 In (Grabiner, 1981b), GRABINER has similarly emphasized the role which CAUCHY'S new definitions played for his foundation for the calculus.

of those, the Eulerian formula is trivially true.[12] Nevertheless, such definitions can be extremely useful in order to investigate other properties. With CAUCHY'S fundamental shift towards arithmetic — rather than algebraic — equality, the numerical convergence of partial sums of series was given prime importance by using it to define *convergent series*.[13] Thus, a property of formal series — which could be numerically convergent or not — was used to define a concept which was subsequently promoted and investigated. A similar change went on in the theory of elliptic functions where ABEL'S original formal inversion of elliptic integrals was replaced by other definitions of elliptic functions. Many of the definitions of elliptic functions following ABEL'S original one turned properties — which were *results* in ABEL'S theory — into definitions. The motivations for this change in the status of properties of elliptic functions are many; rigor and theoretical applicability figure prominently among them.

21.2.2 Relating concepts

As a result of the transition, theorems about concepts and relating concepts came to dominate mathematics. Two types of relations among concepts were of principal importance: the representation of concepts and the determination of the extension of concepts.

Representing concepts. Mathematical symbolism and formulae had proved to be an extremely useful and powerful tool in developing theories in the formula based paradigm. In order to be able to continue this line of research into the concept based paradigm, representations of concepts became quite important. Central instances include ABEL'S classification of explicit algebraic expressions and the multitude of representations of elliptic functions which he developed. A particularly revealing example of the benefits of representations was illustrated in section 18.1 where ABEL'S use of infinite representations in the theory of transformation was discussed. The study of concepts in their entirety and not the individual objects meant that statements concerning the impossibility of certain representations could also be made and proved. This is particularly true of ABEL'S proof of the insolubility of the quintic (see chapter 6) in which a representation of all explicit algebraic expressions was proved not to be sufficiently powerful to encompass the implicitly defined algebraic expression corresponding to a solution of the general fifth degree equation. The very same example also serves to illustrate the problem of distinguishing concepts.

Distinguishing concepts. With the focus on concepts, it also became an important question to determine whether two concepts were identical or differed in their extensions. One of the very best examples is the debate which during the nineteenth

12 (Lakatos, 1976).

13 See section 11.1.

century separated the concepts of *continuous* and *differentiable* functions by constructing ever more pathological functions belonging to the former concept but not to the latter one.[14] The process of investigating concepts can often be thought of as a dialectic effort alternating between limiting and extending the domain of the concept. In pointing out the existence of objects within a concept and differences between concepts, examples and counter examples became very important mathematical tools. A number of similar uses of examples and counter examples can also be found in ABEL'S works. The most conspicuous example is in the theory of equations where ABEL'S proof on the quintic interpreted as limiting the class of solvable equations is precisely in this line of results. The use of counter examples as limitations on concepts is a quite modern one which is only meaningful within the concept based paradigm (see section 21.3).

Delineating concepts. One type of questions concerning the relation between concepts is so important that it deserves special attention; I have called it *delineation of concepts*. This notion refers to a set of questions which concern the precise characterization of the extension of a concept by some external and applicable criterion. In other words, these questions ask for a (feasible) method of determining whether a given particular object falls within the extension of a concept or not. In analogy with the steps of limiting and extending the extension of concepts (figures 6.1 and 7.3, respectively), a graphical representation of the delineation of concepts is produced in figure 21.2.

ABEL'S unfinished research on a general theory of algebraic solubility was motivated by precisely this problem of determining whether or not a given equation could be solved algebraically. Similarly, the search for complete criteria of convergence also sought to delineate the extension of convergent series once and for all. The search for delineation of solvable equations came to a fruitful conclusion when GALOIS' criterion was finally accepted as an answer. The complete determination of the concept of convergent series was never so successful; the only complete characterization obtained was the *Cauchy criterion* (see page 212) which did not fully meet the demand for being external and easy to apply.

21.3 The role of counter examples

It has been described how the problem of investigating the extension of concepts led to a particular use of examples and counter examples. Inspired by ABEL'S curious remarks about his "exception" to *Cauchy's Theorem* (see section 12.5), I suggest that counter examples played fundamentally different roles in the two paradigms discussed here.

14 See (K. Volkert, 1987; K. Volkert, 1989).

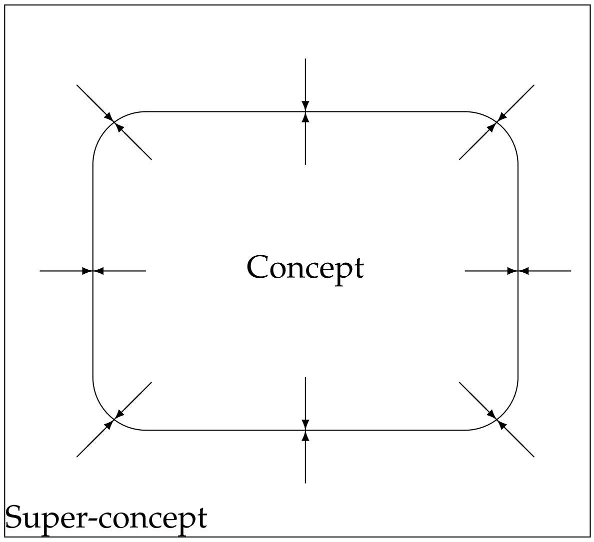

Figure 21.2: Delineating the border between a concept and its super-concept.

21.3.1 Theorems with exceptions

In his binomial paper, ABEL described how he found *Cauchy's Theorem* to "suffer exceptions" and I find it puzzling to investigate how theorems could possibly admit exceptions in the 1820s. First, however, the very phrasing of ABEL'S statement must be considered. Then, by way of recalling arguments carried out "in general", the connection between exceptions and the formula based paradigm opens up.

The authenticity of the wording. One may try to explain ABEL'S wording away as a result of his shyness and veneration for CAUCHY. For instance, in his criticism of L. OLIVIER, ABEL used the mild phrase "this part does not seem to be true" in the printed version rather than the more severe judgement "Mr. Olivier is seriously mistaken" which we find in ABEL'S notebooks.[15] This would suggest that exceptions were a milder form of criticism than outright counter examples or even paradoxes which were also terms found in ABEL'S vocabulary. Besides, the problem remains that we only have A. L. CRELLE'S (1780–1855) translation of ABEL'S original manuscript at our disposal and single words in an edited manuscript can easily be over-interpreted. Nevertheless, when CAUCHY eventually reacted to ABEL'S exception, he did so explicitly stating that he wanted to correct the statement of his theorem so that "it no longer admitted exceptions"[16] (see section 14.1.2). Thus, the word "exceptions" was chosen in this connection, and I believe that the following interpretation makes it plausible that ABEL actually meant that *Cauchy's Theorem* suffered an exception — or rather, a number of exceptions.

15 See section 13.2.

16 "*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.*" (A.-L. Cauchy, 1853, 31–32).

Arguments carried out "in general". In the formula based paradigm, a situation sometimes arose in which the formula carrying the mathematical argument did not apply in all (numerical) cases. A number of such examples have been described above starting with EULER'S awareness that peculiar numerical results could emerge if specific values were inserted in expressions which were formally equal.[17]

As J. V. GRABINER describes,[18] J. L. LAGRANGE (1736–1813) held a strong and lifelong belief in the concept of "the general" in mathematics. Not only could formulae which were valid "in general" be of high importance — a general approach and system in mathematics was also strived for. When LAGRANGE presented his argument that "all" functions could be expanded in *Taylor series*, he was also aware that this might indeed fail to be true for particular functions at particular points.[19] However, these instances where the general results failed to be true were particular, peculiar, and of little interest to mathematicians ascribing to the formula based paradigm.

In connection with ABEL'S *Paris memoir*, an even more elaborate case was presented. At a crucial point in his argument to determine the number μ of independent integrals, ABEL employed a generalized degree operator called h.[20] 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 of the two degrees,

$$\deg\left(P_1 + P_2\right) \overset{?}{=} \max\left\{\deg P_1, \deg P_2\right\}, \tag{21.1}$$

if $\deg P_1 = \deg P_2$. However, in the *Paris memoir*, ABEL was not interested in peculiarities and he simply argued that the equality corresponding to (21.1) was true "in general", i.e. with the exception of some particular cases of little interest (see page 362). Once the paradigms had shifted, the precise determination of the number μ (called the genus) and the investigation and exposure of the necessary assumptions became a hot topic of mathematics.

In a similar situation, ABEL concluded his summary of well known properties of elliptic functions in the *Précis* by the statement:

> "The formulae which have been presented above uphold with certain restrictions if the modulus c is arbitrary, real or imaginary."[21]

ABEL'S way of obtaining the important formulae — often through tedious manipulations of infinite representations — could result in particular cases for which the formulae degenerated or produced false results. However, these cases were few and did not constitute an obstacle to presenting the formulae.

17 See section 10.1.
18 See (Grabiner, 1981a, 317) or (Grabiner, 1981b, 39).
19 See e.g. (Lagrange, 1813, 29–30).
20 See section 19.3.
21 "*Les formules présentées dans ce qui précède ont lieu avec quelques restrictions, si le module c est quelconque, réel ou imaginaire.*" (N. H. Abel, 1829d, 245).

The number of exceptions. As indicated, in the formula based paradigm, results which suffered a few exceptions could still be very useful and the existence of exceptions did not immediately lead to the overthrow of theorems. This suggests an interesting way of interpreting the last part of ABEL'S famous footnote: Besides introducing his exception, ABEL also claimed that many similar functions existed. This indicates that the number of exceptions played a role. A similar remark can also be found in connection with CAUCHY'S example of a non-zero function whose *Maclaurin series* is the zero-function,[22]

$$f(x) = e^{-\frac{1}{x^2}}.$$

This function represented an exception to the general belief in the expansion in power series which laid at the heart of the Lagrangian approach to analysis.[23] In 1822 and 1829,[24] CAUCHY presented this example and observed how to construct other functions with the same property of not being represented by their *Maclaurin series* except at a single point.

Both these examples suggest that if theorems in the formula based paradigm contained a quantification as "for all ...", it might be necessary to introduce a statistical interpretation of the for-all quantification as K. VOLKERT has suggested.[25] Exceptions and their numbers were noticed but no clear distinction between refuted (false) theorems and theorems with exceptions can be drawn. Theorems could be valid even if they suffered exceptions as long as the known exceptions were not too many or too important.

Exceptions and the formula based paradigm. Thus, the argument is that exceptions did have a place in mathematics of the formula based paradigm. The highly computational deductions based on long sequences of manipulations with finite and infinite representations occasionally led to results which were (only) true "in general". Instead of discarding such results, they were accepted with the knowledge or intuition that they should not be uncritically applied. However, as this intuition and general understanding of mathematics shifted towards the concept based paradigm, exceptions became oddities — and counter examples became very powerful tools of argument in this new paradigm.

21.3.2 Counter examples and concepts

In the concept based paradigm, counter examples acquired a position much closer to their modern usage. As noted, counter examples are very instrumental in pointing out the differences between concepts and thereby helping to determine the extension

22 Strictly speaking, the function should also be defined at the origin, $f(0) = 0$. For a good discussion on this issue, see (Bottazzini, 1990, lxix).

23 See section 10.2.

24 (A.-L. Cauchy, 1822, 277) and (A.-L. Cauchy, 1829, 394–395).

25 (K. T. Volkert, 1986, 144–145).

of concepts. Used as tools of criticism, a theorem to which a counter example could be presented was certainly *false* in the concept based approach. There was no room for theorems with exceptions. In a sense, the concept based approach adhered to a viewpoint similar to the Lakatosian one that theorems with counter examples should either be discarded or modified to range over a smaller domain. There is an abundance of such applications of counter examples in the 1820s. ABEL presented one very elaborate example in his refutation of OLIVIER when he showed that *no* criterion of the proposed form could ever be constructed having the properties which OLIVIER had sought. However, the young mathematician who made the most use of counter examples in the 1820s and 1830s was probably DIRICHLET.

In 1829,[26] when DIRICHLET presented his famous result on the convergence of Fourier series, he started the paper with a scrutiny of an earlier paper by CAUCHY.[27] In particular, DIRICHLET criticized a point in the proof where CAUCHY had used an implicit assumption which DIRICHLET identified as follows: If the series $\sum a_n$ was convergent, any other series $\sum b_n$ such that $\lim \frac{b_n}{a_n} = 1$ would also be convergent. Against this argument, DIRICHLET presented the counter example

$$a_n = \frac{(-1)^n}{\sqrt{n}} \text{ and } b_n = \frac{(-1)^n}{\sqrt{n}} \left(1 + \frac{(-1)^n}{\sqrt{n}}\right)$$

of which the series $\sum a_n$ was convergent but the series $\sum b_n$ diverged. DIRICHLET described CAUCHY'S conclusion as "not permissible"[28] because it was easy to construct a counter example.

To DIRICHLET, the existence of one single, local counter example thus seems to have rendered the theorem false; in particular, we find none of the above remarks that "infinitely many similar counter examples may be found or constructed" in DIRICHLET'S papers.[29] In some instances, a counter example led DIRICHLET to dismiss the faulty theorems as *false* and begin his own deductions from other principles. In other situations, DIRICHLET drew inspiration from his counter examples to revise existing proofs in ways which later led to proof analysis.

Later in the nineteenth century, counter examples acquired their modern status as complete refutations of theorems. To a mathematician educated at one of the German universities in the second half of the nineteenth century, a theorem could absolutely not admit exceptions and the precise formulation of theorems and proofs had truly become one of the trademarks of mathematics.

ABEL'S use of counter examples seems to fall in both paradigms. As noted, sense can be made of ABEL'S exception to *Cauchy's Theorem* if it is interpreted in the formula

26 (G. L. Dirichlet, 1829, 120).
27 (A.-L. Cauchy, 1827). Actually, DIRICHLET referred to a paper published in 1823 in the *Mémoires de l'Académie des Sciences*; but no paper with these details can be found in CAUCHY'S *Œuvres*. Thus, it is here assumed that DIRICHLET actually meant (ibid.).
28 "*Mais cette conclusion n'est pas permise*" (G. L. Dirichlet, 1829, 158).
29 (G. L. Dirichlet, 1829; G. L. Dirichlet, 1837).

based paradigm. On the other hand, ABEL'S dismissal of OLIVIER'S criterion of convergence shows signs of a concept based refutation. There, a single counter example was invoked to refute OLIVIER'S claim and an elaborate analysis was employed to show that the concept of *tests of convergence* could not contain a criterion of a slightly generalized form.[30]

The role of mathematical intuition. Importantly, the changing status of counter examples also reflects a change in a mental entity which may be called *mathematical intuition*.[31] This intuition comprises the expertise, prejudices, and expectations of active mathematicians who have gained an insight into their objects and is thus part of T. S. KUHN'S (1922–1996) disciplinary matrix.

During the eighteenth century, mathematicians built up a high degree of insight into representations of functions, in particular into power series or other algebraic expressions. When this insight was formulated, it often took the form of formulae relating certain entities by means of algebraic notation and the formulae were considered to have aesthetic properties described as simplicity or degrees of symmetry. As illustrative examples, consider the solution formulae for general equations of low degree or the power series expansions of elementary transcendental functions. The art of mathematics also consisted of the trained ability to recognize patterns and manipulate representations to obtain various generalizations.

As a result of the change of paradigms, the contents and role of mathematical intuition also changed. A new kind of intuition emerged which helped mathematicians see differences and similarities between concepts and suggested ways of obtaining relations among concepts. As an indication of this change in intuition, mathematicians occasionally brought over intuitions from the old paradigm into the new one. This could lead them to generalize results into forms in which they were then no longer permitted. Thus, the changing intuitions are intimately connected with the process of *concept stretching* which LAKATOS has discussed as part of interpreting mathematical development.[32]

21.4 Conclusion

The analytical scheme of a transition from a formula based paradigm to one based on concepts has shown its applicability in interpreting events in the disciplines of algebra and analysis in the 1820s. In particular, the role of new definitions, the coexistence of theorems and exceptions, and the new problems of delineation have contributed to throwing ABEL'S mathematical production into perspective.

30 See chapter 13.
31 For a discussion of mathematical intuition, see also (K. T. Volkert, 1986).
32 E.g. (Lakatos, 1976).

It is beyond the present scope to analyze and speculate as to the causal reasons for the purported change of paradigms in analysis and algebra. Neither is it the present purpose to discuss at length the general applicability of this frame of interpretation. However, it must be noticed that the interpretation might be limited to the disciplines described here; in particular, it does not appear to be immediately applicable to geometry. With some right, one could argue that the transition could be interpreted simply as a maturing of the involved theories. Still, I believe that the simultaneous instances of the change of style as described above are sufficient to suggest that a general change in the modes of thought was involved.

New questions, new standards, new objects. In the presentation and analysis of ABEL'S mathematical production, three local themes were introduced. Based on the description of his works in algebra, I have argued that a new type of questions was being introduced into mathematics. These new questions were indicative of the fundamental change of paradigms. Concerning ABEL'S works in the foundations of analysis, it was illustrated how the change of basic definitions and standards of proof also reflected the new focus on concepts. Finally, a cross section of ABEL'S works on new transcendentals illustrated how these transcendental objects were being treated with the help of algebraic methods and also how the introduction of new objects led to important questions of representation.

Throughout the description and analysis of ABEL'S works, much attention has been paid to their mathematical contexts. The inspirations which ABEL drew from his predecessors and contemporaries have been described in order to illustrate how ABEL'S works grew continuously out of the mathematical contexts. At the same time, ABEL'S works were — at a number of points — remarkably novel and due attention has been paid to these aspects. To generalize, ABEL'S methods and the problems which he attacked were generally well established whereas the questions which he raised and the approaches which he took in attacking these problems were often new and ground-breaking. In connection with the fundamental transition, this manifested itself in the sense that ABEL had one foot firmly placed in each of the two paradigms.

Appendix A

ABEL's correspondence

In 1881, when ABEL's collected works were published in their second edition, SYLOW and LIE included some of ABEL's correspondence.[1] In 1902, a centennial *Festschrift* on ABEL's life, work, and correspondence appeared both in Norwegian and in French including transcriptions of all known letters to and from ABEL.[2] In the twentieth century, however, additional letters have been found, and for the convenience of the reader, the table **??** provides a list of all the letters pertaining to ABEL known to the author. All the letters appearing in the Norwegian version of the *Festschrift* were also included in the French version. Missing information for year, month, or date indicate that that particular information was not available in the letter.

Table A.1: Correspondence sorted by sender

1822/01/18	(Abel→Aas, Kristiania, 1822/01/18. In Kragemo, 1929, 49)
1822/01/25	(Abel→Aas, Kristiania, 1822/01/25. In ibid., 49–50)
1822/02/06	(Abel→Aas, Christiania, 1822/02/06. In ibid., 50)
1826/10/16	(Abel→Abel, Paris, 1826/10/16. In N. H. Abel, 1902a, 41–43)
1827/02/26	(Abel→Boeck, Berlin, 1827/02/26. In ibid., 55–56)
1827/01/15	(Abel→Boeck, Berlin, 1827/01/15. In ibid., 52–55)
1826/11/01	(Abel→Boeck, Paris, 1826/11/01. In ibid., 47–48)
1828/08/18	(Abel→Crelle, Christiania, 1828/08/18. In ibid., 67–73)
1827	(Abel→Crelle, Christiania, 1827. In ibid., 60–61)
1827/11/15	(Abel→Crelle, Christiania, 1827/11/15. In ibid., 61–62)
1826/03/14	(Abel→Crelle, Freiberg, 1826/03/14. In N. H. Abel, 1881, 266)
1827/11/15	(Abel→Crelle, Christiania, 1827/11/15. In ibid., 268)
1828/10/18	(Abel→Crelle, Christiania, 1828/10/18. In ibid., 269–270)
1828/10/18	(Abel→Crelle, Christiania, 1828/10/18. In Biermann, 1967, 27–29)
1828?	(Abel→Crelle, 1828?. In Biermann and Brun, 1958, 85)
1826/12/04	(Abel→Crelle, Paris, 1826/12/04. In N. H. Abel, 1902a, 50–51)
1826/03/14	(Abel→Crelle, Freyberg, 1826/03/14. In ibid., 21–22)

1 (N. H. Abel, 1881)
2 (N. H. Abel, 1902e; N. H. Abel, 1902f).

Table A.1: Correspondence sorted by sender (cont.)

1826/08/09	(Abel→Crelle, Paris, 1826/08/09. In N. H. Abel, 1902a, 38–39)
1826/08/09	(Abel→Crelle, Paris, 1826/08/09. In N. H. Abel, 1881, 267)
1826/12/04	(Abel→Crelle, Paris, 1826/12/04. In ibid., 268)
1824/03/02	(Abel→Degen, Christiania, 1824/03/02. In P. Heegaard, 1935, 33–37)
1824/03/02	(Abel→Degen, Christiania, 1824/03/02. In P. Heegaard, 1937, 1–5)
1828/09/22	(Abel→Fru Hansteen, Christiania, 1828/09/22. In N. H. Abel, 1902a, 67)
1828/07/29	(Abel→Fru Hansteen, Froland, 1828/07/29. In ibid., 65)
1828/08	(Abel→Fru Hansteen, Froland, 1828/08. In ibid., 65–66)
1828/07/21	(Abel→Fru Hansteen, Froland, 1828/07/21. In ibid., 63–64)
	(Abel→Fru Hansteen. In ibid., 62)
1827/03	(Abel→Fru Hansteen, Berlin, 1827/03. In ibid., 58–59)
1827/08/18	(Abel→Fru Hansteen, Christiania, 1827/08/18. In ibid., 60)
1826/01/16	(Abel→Fru Hansteen, Berlin, 1826/01/16. In ibid., 19–20)
1825?/12/08	(Abel→Fru Hansteen, Berlin, 1825?/12/08. In ibid., 12–13)
1828/11	(Abel→Fru Hansteen, Christiania, 1828/11. In ibid., 75–78)
[1826]/01/30	(Abel→Hansteen, Berlin, [1826]/01/30. In ibid., 20–21)
1826/03/29	(Abel→Hansteen, Dresden, 1826/03/29. In N. H. Abel, 1881, 263–265)
1826/05/28	(Abel→Hansteen, Grätz, 1826/05/28. In N. H. Abel, 1902a, 32)
1826/03/29	(Abel→Hansteen, Dresden, 1826/03/29. In ibid., 22–26)
1826/08/12	(Abel→Hansteen, Paris, 1826/08/12. In ibid., 39–41)
1825/12/05	(Abel→Hansteen, Berlin, 1825/12/05. In ibid., 9–12)
1823/06/15	(Abel→Holmboe, Kjøbenhavn, 1823/06/15. In ibid., 3–4)
1823/08/04	(Abel→Holmboe, Kjøbenhavn, 1823/08/04. In ibid., 4–8)
1828/06/29	(Abel→Holmboe, Froland, 1828/06/29. In ibid., 64–65)
1826/01/16	(Abel→Holmboe, 1826/01/16. In ibid., 13–19)
1823/08/03	(Abel→Holmboe, Copenhague, 1823/08/03. In N. H. Abel, 1881, 254–258)
1826/10/24	(Abel→Holmboe, Paris, 1826/10/24. In ibid., 259–261)
1826/12	(Abel→Holmboe, Paris, 1826/12. In ibid., 261–262)
1827/03/04	(Abel→Holmboe, Berlin, 1827/03/04. In ibid., 262)
1827/03/04	(Abel→Holmboe, Berlin, 1827/03/04. In N. H. Abel, 1902a, 56–58)
1826/12	(Abel→Holmboe, Paris, 1826/12. In ibid., 51–52)
1827/01/20	(Abel→Holmboe, Berlin, 1827/01/20. In ibid., 55)
1826/04/16	(Abel→Holmboe, Wien, 1826/04/16. In ibid., 26–31)
1826/10/24	(Abel→Holmboe, Paris, 1826/10/24. In ibid., 43–47)
1826/06/15	(Abel→Holmboe, Bolzano, 1826/06/15. In ibid., 33–37)
1825/09/15	(Abel→Holmboe, Kjøbenhavn, 1825/09/15. In ibid., 9)
1826/07/05	(Abel→Keilhau, Zurich, 1826/07/05. In ibid., 37)
1826/11/01	(Abel→Külp, Paris, 1826/11/01. In Hensel, 1903, 237–240)
1828/11/25	(Abel→Legendre, Christiania, 1828/11/25. In N. H. Abel, 1902a, 78–86)
1828/11/25	(Abel→Legendre, Christiania, 1828/11/25. In N. H. Abel, 1881, 271–279)
1823	(Abel→Olsen, Christiania, 1823. In Brun and Jessen, 1958, 22–23)

Table A.1: Correspondence sorted by sender (cont.)

1828/09/10	(Crelle→Abel, 1828/09/10. In N. H. Abel, 1902a, 66)
1828/05/18	(Crelle→Abel, 1828/05/18. In ibid., 62)
1829/04/08	(Crelle→Abel, Berlin, 1829/04/08. In ibid., 89–90)
1826/11/24	(Crelle→Abel, Berlin, 1826/11/24. In ibid., 48–50)
1829/05/10	(Crelle→Holmboe, Berlin, 1829/05/10. In N. H. Abel, 1902b, 97–98)
1840/05/15	(Crelle→Holmboe, Berlin, 1840/05/15. In ibid., 102)
1821/05/21	(Degen→Hansteen, Kjøbenhavn, 1821/05/21. In ibid., 93–96)
1830/07/24	(Det franske Institut→Abel's efterladte, Paris, 1830/07/24. In N. H. Abel, 1902a, 101)
1752	(Euler→Goldbach, 1752. In Euler and Goldbach, 1965)
1829	(Jacobi→Legendre, Potsdam, 1829. In Legendre and Jacobi, 1875)
1830/02/22	(Keilhau→Boeck, Froland, 1830/02/22. In N. H. Abel, 1902b, 98–100)
1781	(Lagrange→d'Alembert, Berlin, 1781. In Lagrange, 1867–1892, vol. 13, 368–370)
1828/10/25	(Legendre→Abel, Paris, 1828/10/25. In N. H. Abel, 1902a, 74–75)
1829/01/16	(Legendre→Abel, Paris, 1829/01/16. In ibid., 87–89)
1832/04/11	(Löwenhielm→Hansteen, Paris, 1832/04/11. In N. H. Abel, 1902b, 101–102)
1824/08/02	(Schumacher→Hansteen, Altona, 1824/08/02. In ibid., 97)
1829/04/07	(Smith→Holmboe, Froland, 1829/04/07. In ibid., 97)
1881/12/09	(Weierstrass→Lie, Berlin, 1881/12/09. In ibid., 103)
1882/04/10	(Weierstrass→Lie, Berlin, 1882/04/10. In ibid., 103–104)
1873	(Weierstrass→du Bois-Reymond, 1873. In K. Weierstrass, 1923, 199–201)

Appendix B

ABEL's manuscripts

Manuscripts and drafts constitute an important source for historical inquiry, especially when the historian aims at discussing the genesis of certain ideas. In the case of ABEL, the extent sources not published within or immediately after his lifetime fall into two categories:

1. Manuscripts — to various degrees of completion — of papers not published in ABEL's lifetime.
2. Drafts and notebooks documenting the working mathematician but not intended for publication.

Items in the first category was to some extent considered and included in the compilations for both editions of ABEL's collected works (N. H. Abel, 1839; N. H. Abel, 1881). In the second volume of the second edition (1881) collected works, SYLOW also included a general presentation of the known items from the second category[1].

The present appendix has as its aim to document the whereabouts of archival material concerning ABEL. This aim is achieved by reproducing registrants of the archives at the Manuscript Collection, University of Oslo (see table B.1) and the Mittag-Leffler Institute, Djursholm, Sweden (table B.5).

1 (N. H. Abel, 1881, vol. 2, 283–289)

2 The author gratefully acknowledges the participation of KLAUS FROVIN JØRGENSEN in obtaining the information presented in table B.5.

MS:592	Manuscripts and letters (table B.2)
MS:434	Fragments from manuscripts (table B.3)
MS:435	Note accompaning the Abel manuscripts (fragmentary, 6 pages)
MS:969-4	Nogle Bemærkninger om Vinkelfunktionerne (fragmentary, 3 pages)
MS:589	Belonging to the Théorie de la résolution algébrique des équations (fragmentary, 2 pages)
MS:592	Précis de la théorie des fonctions elliptiques (168 pages)
MS:920-4	Niels Abel Berlin-Paris 1825–182? (11 pages)
MS:188-8	Note über die Function ... (2 pages)
MS:969-4	Paa Froland og ved Abels grav 4. og 5. august (4 pages)
Multiple	ABEL's notebooks (table B.4)

Table B.1: Abel manuscript collections in the Manuscript Collection, University Library, Oslo.

1.	Mémoire sur une classe particulière d'équations résolubles algébriquement (64 pages)
2.	Note sur quelques formules elliptiques (18 pages)
3.	Théorèmes sur les fonctions elliptiques (11 pages)
4.	Démonstration d'une propriété générale d'une certaine classe de fonctions transcendantes (4 pages)
5.	Matematiske uddrag fra N. H. Abel's breve (13 pages)

Table B.2: Abel manuscripts in the Manuscript Collection, University Library, Oslo, MS:592.

1.	Précis d'une théorie des fonctions elliptiques (fragmentary, 8 pages)
2.	Om transformationer af elliptiske funktioner, hvorved de to perioder divideres med hvert sit tal (fragmentary, 9 pages)
3.	Divisionsligningers opløsning (fragmentary, 2 pages)
4.	*Unidentified* (fragmentary, 1 page)
5.	Belonging to the Précis or the Recherches (fragmentary, 2 pages)
6.	Belonging to the Paris mémoire (fragmentary, 2 pages)
7.	Belonging to the Précis (fragmentary, 7 pages)
8.	Belonging to the Recherches second part (fragmentary, 2 pages)
9.	Table of contents for a work on elliptic functions (fragmentary, 4 pages)
10.	Integration ved hjælp af algebraiske, logaritmiske og eksponentielle funktioner (fragmentary, 2 pages)
11.	Belonging to the Abelian Theorem for elliptic functions (fragmentary, 2 pages)

Table B.3: Abel manuscripts in the Manuscript Collection, University Library, Oslo, MS:434.

MS:351:A	Notebook A	Mémoires de Mathématiques par N. H. Abel. Paris le 9 Août 1826 (202 pages fol.)
MS:436	Notebook B	*Without title* (178 pages fol.)
MS:351:C	Notebook C	*Without title* (215 pages fol.)
MS:696	Notebook D	Remarques sur divers points de l'analyse par N. H. Abel, 1er Cahier le 3 Sept. 1827 (136 pages 4:o)
MS:829	Notebook E	Mathematiske Udarbeidelser af Niels Henrik Abel (192 pages 4:o)
MS:749		Matematiske Afhandlinger (170 pages 4:o)

Table B.4: Abel's mathematical notebooks in the Manuscript Collection, University Library, Oslo.

1.	Manuskript af Abel (V.Terquem, Bulletin, T. I, p. 56)
2.	Manuskript i 4:o: Note sur quelques formules elliptiques
3.	Manuskript i 8:o: Théories sur les fonctions elliptiques
4.	P.M. af Phragmén
5.	Acta korrektur af Recherches sur les fonctions elliptiques
6.	Tryckt: Mémoire sur une propriété générale d'une classe très-étendue de fonctions transcendantes. 1841. 4:o.
7.	Mémoire sur les équations algébriques etc. Chr:ia 1824. 4:o.
8.	Oplösning af et Par Opgaver ved Hjelp af bestemte Integraler. 8:o
9.	Almindelig methode til at finde Funktioner af een variable Störrelse. . . 8:o.

Table B.5: Abel manuscripts in the Mittag-Leffler Institute, Djursholm, Sweden.[2]

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