An imaginary interview with Niels Henrik Abel

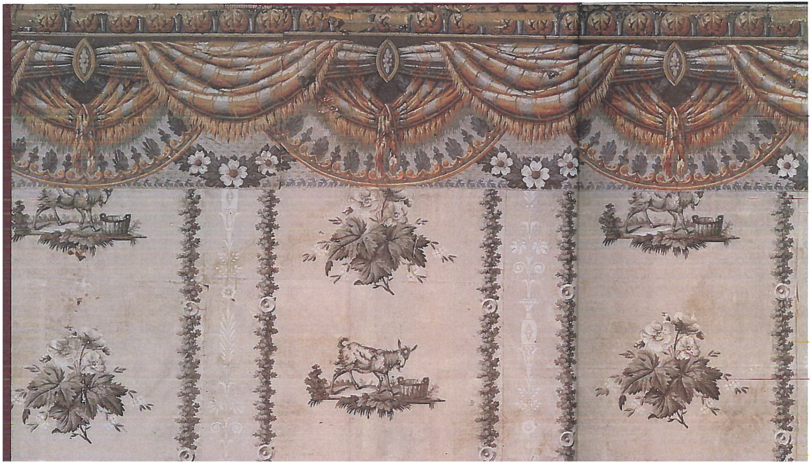
Held in “Bukkerommet” at Froland Verk on February 6, 1829

Christian F. Skau

Department of Mathematical Sciences, Norwegian University of Science and Technology

September 26, 2014

Main building at Froland Verk (located 200 kilometers south-west of Oslo) where Abel stayed from December 19, 1828 to his death on April 6, 1829.



Abel stayed in the “Buck room” (“Bukkerommet” in Norwegian), named after the eponymous goat on the wallpaper

Born Aug. 5 — 1802

1815

1816

1817

1818 — New math teacher (Bernt Michael Holmboe)

1819

1820 — Death of Abel's father

1821 — "Proof" of solvability (sic!) of the quintic

Examen Artium

Pupil at the Cathedral School in Christiania (later Oslo)

1822

Entrance exam to the university

1823 — Visits professor Degen in Copenhagen

Preparation for the travel to Göttingen and Paris – in isolation in Christiania

1824

"Anni mirabiles" (The miraculous years)

Discoveries:
1) Abel's integral equation
2) Unsolvability of the quintic
3) Elliptic functions
4) The addition theorem

1825

Abel's foreign trip to Berlin and Paris

1826 — Berlin and Crelle: "Journal für die reine und Angewandte Mathematik".

Paris and the disappearance of the Paris Memoir.

1827 — Avoids travelling to Göttingen and Gauss.

Hectic work period in Christiania

1828

Development of the theory of elliptic functions.
(Abel-Jacobi "competition".)
Theory of equations.

1829 — January 6: Last manuscript – proof of the addition theorem in its most general form.

Last $3^1/_2$ months at Froland Verk

Interview February 6

†April 6

Q.

Tell us about your first encounter with mathematics.

A.

My father sent me and my older brother Hans Mathias to Christiania in 1815 when I was 13 years old to enroll as pupils at the Cathedral School. This was the best secondary school in Norway at the time. That was the first time I was exposed to mathematics beyond elementary arithmetical computations. I liked mathematics and was pretty good at it. However, we had a math teacher named Hans Peter Bader who I, as well as my co-pupils, hated. I would describe him as a bully bordering on a sadist. Even though Bader was a competently trained mathematics teacher his behaviour dampened my interest in mathematics.

Q.

Could you be more specific?

A.

Bader physically thrashed and beat up pupils in his class if he was not satisfied with their answers or solutions – sometimes even for no apparent reason. One day – this happened in November 1817 – he beat up a pupil named Henrik Stoltenberg who fell to the floor while Bader continued to kick him. Henrik was bedridden and he died a week later. Whether that was directly related to Bader's mistreatment or not I do not know, but the result was that Bader was fired, which made us pupils very happy. I should mention that Henrik Stoltenberg's father, Carl Peter Stoltenberg, was a member of our parliament (Storting), and was therefore an influential person.

Q.

What happened then?

A.

For me it was a watershed – a before and after – in my life. We got a new math teacher in early 1818, Bernt Michael Holmboe. He was only 23 years old and he exhibited an enthusiasm for mathematics that was contagious. More importantly, he opened, figuratively speaking, the door for me into mathematics at a deeper level by introducing me to the great masters of mathematics, especially Euler, Lagrange and Gauss. In the beginning Holmboe gave me private tutoring, but after a while I studied the works of these masters by myself. You could say that I devoured the books of these giants of mathematics. In the beginning I borrowed books from Holmboe's rather impressive book collection, and later from the university library.

Q.

Is it fair to say that Euler, Lagrange and Gauss became your teachers of mathematics, so to say?

A.

That is absolutely true. My life was changed when I studied these masters. I hold the firm conviction that if one wants to make progress in mathematics, one should study the masters, not their pupils. I decided that I would devote my life to mathematics. During all this time Holmboe encouraged me and goaded me on, selflessly admitting that he could not keep up with me.

Q.

This may be an improper question to ask, but your father was a public figure very much in the public spotlight, so it would be unnatural to skirt this matter: Your father was elected member of our parliament (Storting) in 1818, but he made a scandal that almost resulted in an impeachment and dismissal from parliament. Anyway, he was heavily criticized and viciously ridiculed in the newspapers at the time. How did this affect you living in Christiania and experiencing this close up?

A.

It is painful for me to talk about this. I felt that my father was unfairly attacked. He returned to his vicarage at Gjerstad in Aust-Agder as a broken man, his reputation irreparably damaged. He died two years later, in 1820, the early death certainly being hastened by the condemnation he had been exposed to. Initially, at the Cathedral School there were a few pupils that teased me about my father, but fortunately both Holmboe and Rosted, the rector, protected me and shielded me from being taunted. However, my main consolation during this painful period was mathematics. When I got immersed in a mathematical problem or a mathematical theory I was completely lost to the world and the surrounding noise and din.

Q.

This leads naturally to the next question. Tell us about your first mathematical discovery.

A.

Let me start by saying that I early on was fascinated by the theory of equations, and in particular solvability problems. The first result of some importance that I obtained occurred during my last year at the Cathedral School when I was 18 years old. I proved, or so I thought, that the general quintic equation $x^5 + a_4x^4 + a_3x^3 + a_2x^2 + a_1x + a_0 = 0$ can be solved by radicals, thereby settling a problem that mathematicians had struggled with for more than 300 years. However, the proof was wrong – I found the mistake myself – which of course was a let-down, but it had the beneficial effect that I became convinced that it was impossible to solve the general equation of degree $n \geq 5$ by radicals. Two and a half years later, in late autumn 1823, I succeeded in proving the unsolvability.

Q.

Your answer brings to mind what the French mathematician Jean Étienne Montucla (1725-1799) metaphorically says in his widely read "Histoire des Mathématiques" – you may have perhaps read it yourself? – describing the allure of solving the quintic:

"The ramparts are raised all around but, enclosed in its last redoubt, the problem defends itself desperately. Who will be the fortunate genius who will lead the assault upon it or force it to capitulate?"

So you were the fortunate genius! Is it possible to give an indication how you proved your impossibility result, so that non-experts get a flavour of what is involved?

A.

I can try. First I will say that I am indebted to Lagrange for his analysis of the solutions of the general cubic and quartic equations that were obtained by the Italians in the early 1500's. Lagrange gave a methodical explanation of why solutions by radicals were possible in these cases. When he applied the same analysis to the general quintic, the ensuing so-called resolvent equation became of degree six, so higher than five. This was in contrast to the cubic and quartic case, where the resolvent equations were of lower degree; two and three, respectively. All this indicated that something dramatically new happened when the degree of the general equation was higher than four.

A.

Now let us look at the general equation of degree two, whose solution was known in antiquity,

$$x^2 + a_1 x + a_0 = 0$$

$$x_1 = -\frac{a_1}{2} + \frac{1}{2}\sqrt{a_1^2 - 4a_0}, \quad x_2 = -\frac{a_1}{2} - \frac{1}{2}\sqrt{a_1^2 - 4a_0}$$

$$\sqrt{a_1^2 - 4a_0} = x_1 - x_2$$

What we should note are two things:

(i) The radical that occur in the solution is a square root.

(ii) The radical can be expressed as a polynomial in the roots.

A.

The general cubic equation,

$$x^3 + a_2x^2 + a_1x + a_0 = 0$$

which we by a simple substitution can write in the form

$$x^3 + px + q = 0$$

has roots given by the so-called Cardano's formulas:

A.

$$x_1 = \sqrt[3]{-\frac{q}{2} + \sqrt{\frac{q^2}{4} + \frac{p^3}{27}}} + \sqrt[3]{-\frac{q}{2} - \sqrt{\frac{q^2}{4} + \frac{p^3}{27}}}$$

$$x_2 = \omega\sqrt[3]{-\frac{q}{2} + \sqrt{\frac{q^2}{4} + \frac{p^3}{27}}} + \omega\sqrt[3]{-\frac{q}{2} - \sqrt{\frac{q^2}{4} + \frac{p^3}{27}}}$$

$$x_3 = \omega^2\sqrt[3]{-\frac{q}{2} + \sqrt{\frac{q^2}{4} + \frac{p^3}{27}}} + \omega^2\sqrt[3]{-\frac{q}{2} - \sqrt{\frac{q^2}{4} + \frac{p^3}{27}}},$$

where $\omega = e^{\frac{2\pi i}{3}}$, and where

A.

$$\sqrt{\frac{q^2}{4}+\frac{p^3}{27}} = \frac{1}{9}(\omega+\frac{1}{2})(x_1-x_2)(x_1-x_3)(x_2-x_3)$$
$$\sqrt[3]{-\frac{q}{2}+\sqrt{\frac{q^2}{4}+\frac{p^3}{27}}} = \frac{1}{3}(x_1+\omega x_2+\omega^2 x_3)$$
$$\sqrt[3]{-\frac{q}{2}-\sqrt{\frac{q^2}{4}+\frac{p^3}{27}}} = \frac{1}{3}(x_1+\omega^2 x_2+\omega x_3).$$

What we should note is that the first ("inner") radical is a square root, while the next is a cubic root. Furthermore, all the radicals that occur in the solution can be expressed as polynomials in the roots x_1, x_2 and x_3 (as well as the roots of unity – in this case a cubic root of unity).

A.

A similar result is true for the general quartic equation. I realized that the key to prove the unsolvability by radicals of the quintic and higher degrees was firstly to prove that the radicals occurring in a putative solution could be expressed as polynomials in the roots (with certain roots of unity appearing in the coefficients). Then, secondly, by permuting the roots one would get a number of different values, which would not be compatible with the restricted number of values that the radicals can obtain.

A.

By modifying slighty the second part of the proof, it can be summarized as follows: In a putative solution by radicals the first ("inner") radical would have to be a square root. If the degree n of the general equation is larger or equal to three, then the second radical have to be a cubic root. However, if $n \geq 5$, then it followed from my analysis that the second radical have to be a quintic radical. Hence we get a contradiction to the assumption that there exists a radical solution if $n \geq 5$.

A.

There is a peculiar antisymmetry, so to say, between the two parts that the proof consists of: In the first part one starts with the last ("outermost") radical and works successively inwards to the first ("innermost") radical; in the second part one starts with the innermost radical and works outwards.

I should mention that when I came to Paris I learned that there was an Italian – his name was Paolo Ruffini – that had written several papers claiming to have proved the unsolvability result. However, his writing is so obscure that that it is difficult to judge the validity of his proof.

Q.

You wrote up your proof of the unsolvability of the general quintic and higher degrees equations as a pamphlet in 1824, and later published a more detailed proof in the first volume of "Journal für die Reine und angewandte Mathematik", the journal founded by Crelle in Berlin 1826. However, you made some important other discoveries before you had nailed the proof of this spectacular result. Could you tell us about some of these?

A.

I would mention two results, of which the first concerns indefinite integrals (or anti-derivatives). It is well known that the derivative of an elementary function is again an elementary function. By an elementary function I mean a function which comprises

(i) rational functions

(ii) algebraic functions $y = \phi(x)$, i.e. y is implicitly defined by $y^n + a_{n-1}(x)y^{n-1} + \cdots + a_1(x)y + a_0(x) = 0$, where the $a_i(x)$'s are rational functions.

(iii) the exponential function e^x and its inverse, $\log x$

(iv) the trigonometric functions and their inverse arc-functions

(v) all functions which can be defined by means of any finite combination of these preceding classes of functions.

A.

For example, let $f(x)$ be the elementary function

$$f(x) = \frac{x}{2}\sqrt{x^2-1} + \frac{1}{2}\log(x+\sqrt{x^2-1})$$

Then the derivative is:

$$f'(x) = \frac{x^2}{\sqrt{x^2-1}}$$

so again an elementary function.

A.

Is the converse true, namely, is the (indefinite) integral of an elementary function again an elementary function? This question had been investigated by many mathematicians, among them Laplace, but no one had been able to prove what was the conventional conjecture, namely that this is not true in general. I developed a theory around this problem and gave some criteria when the integral of an elementary function is again elementary.

A.

I also proved that

$$\int \frac{\log x}{x+a} dx$$

is <u>not</u> an elementary function if $a \neq 0$. (If $a = 0$ we get $\int \frac{\log x}{x} dx = \frac{1}{2}\log^2 x + C$). This discovery of mine, which I did during the winter 1822-23, I deemed so important that I wrote it up in French – the lingua franca of mathematics. However, the manuscript disappeared when it was sent between various departments as an appendix to my application for a travel grant.

Q.

But haven't you written up this result again? It seems to us that the result is of considerable importance?

A.

No, I have not done that, partly because I was so busy making even more important discoveries. However, I have not forgotten it. In the memoir I worked on this last autumn, "Précis d'une théorie des fonctions elliptiques", of which half is finished, I announce in a footnote that I am in possession of a comprehensive theory on integration of elementary functions, and in particular algebraic functions, and I will get back to it later. This is one of my many projects that I will pursue when I get well and am able to work again.

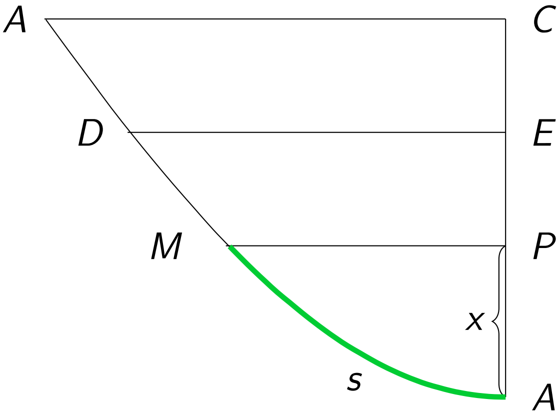
Q.

We interrupted you with our last question. So let's get back to the other discovery you mentioned which you found during the period we are talking about.

A.

In the first year of publication of the Norwegian language scientific journal “Magazin for Naturvidenskapen” that Hansteen, professor in applied mathematics at the university in Christiania, founded in 1823, there appeared one article of mine titled: “Opløsning af et Par Opgaver ved Hjelp af bestemte Integraler” (“The solution of some problems by means of definite integrals”). One of the problems I solved was a mechanical problem that involved an integral equation. As far as I know no one had looked at similar equations before. Let me describe the mechanical problem:

A.



A.

A monotonely increasing function $\psi(x)$ is given. Find the shape of the curve $AMDB$ such that a mass released at rest at M will reach A at the time $\psi(x)$ if it glides (without friction) along the curve under the influence of gravity. The relevant equation turns out to be

$$\int_0^x \frac{g(t)}{\sqrt{x-t}}dt = \psi(x) \qquad (\star)$$

The problem is to find the function $g(x)$, where $ds = g(x)dx$.

A.

I solved this by introducing fractional derivation. Then equation ($\star$) can be written

$$\psi(x) = \sqrt{\pi}\frac{d^{1/2}s}{dx^{1/2}}$$

where s is the arc length between A and M, The solution then becomes

$$s = \frac{1}{\sqrt{\pi}}\frac{d^{-1/2}\psi(x)}{dx^{-1/2}} = \frac{1}{\pi}\int_0^x \frac{\psi(t)}{\sqrt{x-t}}dt$$

and then

$$g(x) = \frac{ds}{dx}$$

A.

One gets to fractional derivation this way, starting with the Taylor series of a function $F(x)$:

$$F(x) = \sum_{n=0}^{\infty} a_n x^n$$

The k'th derivative is

$$\frac{d^k F(x)}{dx^k} = F^{(k)}(x) = \sum_{n=k}^{\infty} a_n \frac{n!}{(n-k)!} x^{n-k} = \sum_{n=0}^{\infty} a_n \frac{\Gamma(n+1)}{\Gamma(n+1-k)} x^{n-k}$$

where Γ denotes the gamma function. (Recall that $\Gamma(m) = \infty$ if $m = 0, -1, -2, \cdots$). Now substitute k with any α.

A.

I define the α-derivative $F^{(\alpha)}(x)$ of $F(x)$ by

$$\frac{d^{\alpha}F(x)}{dx^{\alpha}} = F^{(\alpha)}(x) = \sum_{n=0}^{\infty} a_n \frac{\Gamma(n+1)}{\Gamma(n+1-\alpha)} x^{n-\alpha}$$

One can show that $F^{(\alpha+\beta)} = F^{\alpha} \circ F^{\beta}$, $F^{(0)} = F$ and $F^{(-1)}(x) = \int_0^x F(t)dt$.

A.

By using the equality

$$\frac{\Gamma(x)\Gamma(y)}{\Gamma(x+y)} = \int_0^1 t^{x-1}(1-t)^{y-1}dt$$

which is due to Euler, one gets by interchanging summation and integration, the following identity:

$$F^{(\alpha)}(x) = \frac{1}{\Gamma(-\alpha)} \int_0^x F(t)(x-t)^{-\alpha-1}dt$$

Here α should be less than one.

A.

Setting $\alpha = -\frac{1}{2}$ one gets

$$\frac{d^{-1/2}F(x)}{dx^{-1/2}} = F^{(-\frac{1}{2})}(x) = \frac{1}{\Gamma\left(\frac{1}{2}\right)} \int_0^x \frac{F(t)}{\sqrt{x-t}}dt = \frac{1}{\sqrt{\pi}} \int_0^x \frac{F(t)}{\sqrt{x-t}}dt$$

which is exactly the form we want. So

$$s(x) = \frac{1}{\sqrt{\pi}} \frac{d^{-1/2}\psi(x)}{dx^{-1/2}} = \frac{1}{\pi} \int_0^x \frac{\psi(t)}{\sqrt{x-t}}dt$$

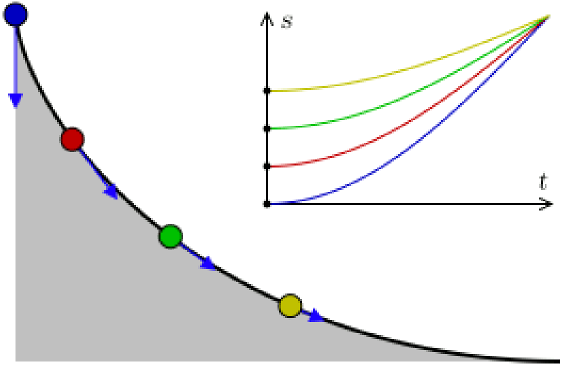
A.

In particular, if $\psi(x) =$ constant, that is if the time of descent of the mass is the same regardless from what height it starts, we get the solution

$$s = k\sqrt{x}$$

where k is a constant. This is a well known equation for the cycloid, and so we get as a very special case of my solution that the cycloid is the isochrone curve, as was shown by Christian Huygens in 1673.

A.



Q.

You got a small travel grant, generously given to you by Søren Rasmussen, professor of pure mathematics at the university, so that you could travel to Copenhagen in the summer of 1823 to visit professor Ferdinand Degen at Copenhagen University. Degen was arguably the strongest mathematician in Scandinavia at that time. Can you tell us what was the most important thing that came out of this visit for you?

A.

Well, first on a personal note: During my stay in Copenhagen I met Christine Kemp, who later become my fiancée, and who is governess here at Froland Verk. She is tirelessly looking after me while I am recuperating.

A.

Mathematically, what was most important for me was that Degen encouraged me to study Legendre's books treating elliptic integrals, which I did right after I returned to Christiania. This led in turn to what I will characterize as a major discovery, namely that by inverting the elliptic integral (of the first kind), which in Legendre's normal form is

$$\int_0^x \frac{dx}{\sqrt{(1-x^2)(1-k^2x^2)}},$$

(note that $k = i$ gives $\int_0^x \frac{dx}{\sqrt{1-x^4}}$) one gets a function with two periods. I had actually met a roadblock before this discovery which held me up for some time.

Q.

This sounds interesting! Is this roadblock the same as what you refer to in the letter that Holmboe told us he received from you during your sojourn in Copenhagen? We quote from the letter:

"...You remember the little paper which treated the inverse functions of the elliptic transcendentals; I asked Degen to read it, but he could not discover any erroneous conclusions or where the mistake may be hidden. God knows how I can pull out of it! ..."

A.

You are right. Let me explain: Reading Gauss' "Disquisitiones Arithmeticae", it was especially Chapter VII which intrigued me. There Gauss studies cyclotomic equations (or, circle division equations). He proves that dividing a circle in n equal parts lead to a cyclotomic equation that can be solved by radicals. He also proves that a regular n-gon can be constructed by ruler and compass if and only if n is a product of distinct Fermat primes (of course multiplied by 2^m for some m).

A.

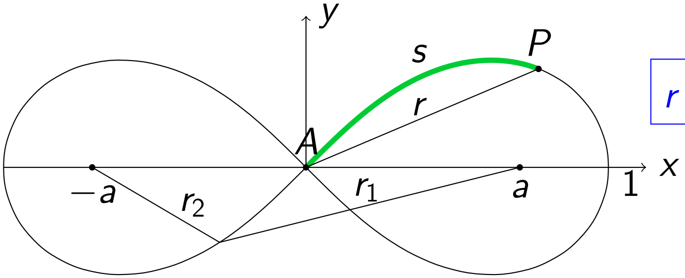
Then he says that his theory can also be applied not only to circular functions (i.e. sine/cosine functions) but also to other transcendental functions, for example those that depend on the integral

$$\int \frac{dx}{\sqrt{1-x^4}}.$$

This points to the lemniscate.

A.

$$s = \operatorname{arc}(AP) = \int_0^r \frac{\mathrm{d}r}{\sqrt{1-r^4}} \qquad r = \phi(s)$$



$$\boxed{r = \sqrt{\cos 2\theta}}$$

$$r_1 \cdot r_2 = a^2 \quad (2a^2 = 1)$$

$$\boxed{(x^2+y^2)^2 = x^2 - y^2}$$

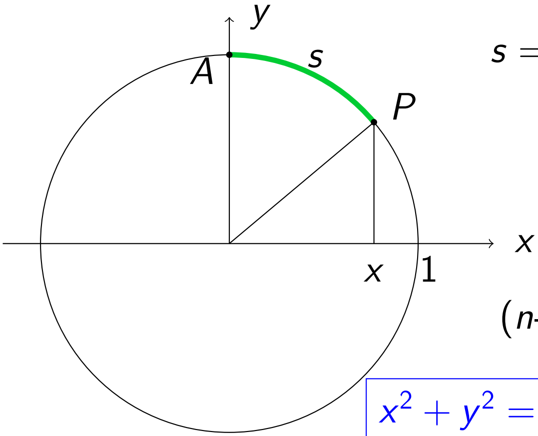
A.

Let us first consider the circle: the equation to find the n points that divide the unit circle in n equal parts is of course $x^n - 1 = 0$, or, removing the trivial root $x = 1$,

$$x^{n-1} + x^{n-2} + \cdots + x + 1 = 0.$$

One can also express this in terms of an integral.

A.



$$s = \mathrm{arc}(AP) = \int_0^x \frac{\mathrm{d}x}{\sqrt{1-x^2}} = \arcsin x$$

$$\frac{2\pi}{n} = \int_0^x \frac{dx}{\sqrt{1-x^2}}, \; x = \sin\left(\frac{2\pi}{n}\right)$$

(n-division of the circumference.)

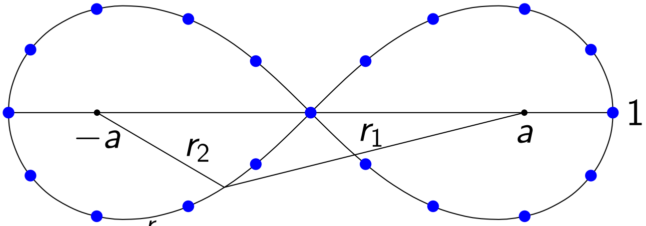
$$\boxed{x^2 + y^2 = 1}$$

A.

I found the equation for the n points dividing the lemniscate in n equal parts, and the equation is of degree n^2, having n^2 distinct roots! But there are only n division points, so how should one interpret the other $n^2 - n$ roots? This was what prompted my question to Degen.

A.

$$\underline{r_1 \cdot r_2 = a^2 \quad (2a^2 = 1)}$$



$$\frac{\omega}{4} = \int_0^1 \frac{\mathrm{d}r}{\sqrt{1-r^4}}$$

$$s = \int_0^r \frac{\mathrm{d}r}{\sqrt{1-r^4}}$$

$$\omega = \frac{2\pi}{M(1,\sqrt{2})} = 5,2411\ldots \ \text{(Gauss)}$$

$$r = \phi(s)$$

n-division of the circumference:

$$\phi(s) = \phi(s+\omega) = \phi(s+i\omega)$$

$$\frac{\omega}{n} = \int_0^r \frac{\mathrm{d}r}{\sqrt{1-r^4}} \qquad r = \phi\left(\frac{\omega}{n}\right)$$

Q.

Could you explain to us why the existence of two independent periods of the inverse function $x = \phi(s)$ of the lemniscate integral,

$$s = \int_0^x \frac{dx}{\sqrt{1 - x^4}}$$

leads to an equation of degree n^2 – and not of degree n – for the division of the circumference in n equal parts?

A.

This is easy to explain. In fact, the equation for dividing the circumference in n equal pieces boils down to find the equation that has the roots $\phi(\frac{k\omega+mi\omega}{n})$, where $0 \leq k, m \leq n-1$, and ω and $i\omega$ are the two periods of ϕ, ϕ being the inverse lemniscate function. So the equation in question must be of degree n^2 since the number of roots are n^2.

In the circle case the inverse function is the sine function which has only one period, namely 2π. So in this case the division equation is of degree n.

Q.

So you discovered the double periodicity of the inverse function of an elliptic integral (of the first kind) in 1824, naming this inverse an elliptic function. You where in Christiania at the time preparing for your travel abroad to Göttingen and Paris to visit Gauss and the leading French mathematicians. However, your first publication on elliptic functions, “Recherches sur les fonctions elliptiques”, which was published in two instalments in Crelle’s journal, were not written up before you returned to Christiania after your travel abroad. Why this delay?

A.

During my stay both in Berlin and Paris, especially in Paris, I worked on and off on elliptic functions uncovering their amazing properties. The "Recherches" – memoir gives a comprehensive theory of these. I thought I was the only mathematician in possession of this theory, but there I was mistaken. If I had known that Jacobi was on my heels on this I would not have postponed writing up my discoveries.

Q.

Holmboe has told us about a letter you sent to him from Paris in December 1826, which is related to what you just said. In this letter you write among other things:
"...I have lifted the mystery which rested over Gauss' theory of division of the circle. ...I am preparing a memoir on elliptic functions in which there are many queer things which I flatter myself will startle someone; among other things it is about the division of arcs of the lemniscate. ...All I have described about the lemniscate is the fruit of my efforts in the theory of equations, my favorite topic. You will not believe how many delightful theorems I have discovered."
In the same letter you said you would visit Göttingen and Gauss on your forthcoming journey back to Berlin. Why did you change your mind?

A.

The reason I did not make a detour to Göttingen on my way back to Berlin from Paris, which, by the way, I left on December 29, was simply that I was almost broke! I could not afford to visit Göttingen. I hoped that when I got to Berlin money would have arrived for me from Norway, which also happened, though not immediately. Unfortunately, there never arose an occasion later for me to meet Gauss. I admired Gauss enormously, but I wished his mathematical writing had been more expansive and explanatory; more like Euler and Lagrange you may say. Gauss was like a fox, who erases its tracks with its tail. In fact, Gauss usually declined to present the intuition behind his very elegant proofs and erased all traces of how he discovered them.

Q.

Maybe it was a good thing you did not meet Gauss! In fact, we have been in contact with Bessel, a confidant of Gauss. After the first part of your "Recherches"-memoir was published in Crelle's journal in September 1827, Bessel wrote to Gauss urging him to publish his results on elliptic functions. Here is a quotation from Gauss' reply letter to Bessel:

Q.

"I shall most likely not soon prepare my investigation on the transcendental functions which I have had for many years – since 1798 – because I have many other matters which must be cleared up. Herr Abel has now, as I see, anticipated me and relieved me of the burden in regard to about one third of these matters, particularly since he has executed all developments with great stringency and elegance. He has followed exactly the same road which I traveled in 1798; it is no wonder that our results are so similar. To my surprise this extended also to the form and even, in part, to the choice of notations, so several of his formulas appeared as if they were copied from mine. But to avoid every misunderstanding, I must observe that I cannot recall ever having communicated any of these investigations to others."

A.

Well, maybe it was a good thing after all that I did not meet Gauss. If we had met I would certainly have told him about my discoveries on elliptic functions, only to be told, I am sure, that he had made the same discoveries almost 30 years earlier. That certainly would have been a disappointment for me. On the other hand, I feel proud and very honoured to hear Gauss' nice words and praise of the first part of my "Recherches"-memoir. Especially since Gauss is not known to lavish praise on the works of other mathematicians. He would probably also have been pleased with the second part of "Recherches", which treats transformation theory, including examples of complex multiplication.

Q.

Finally, we come to your greatest discovery: the addition theorem. The genesis of that discovery, we suspect, is alluded to in a letter dated March 2, 1824, that you sent from Christiania to Degen in Copenhagen. As you understand we have been in contact with Degen about this. Here is a quotation from your letter to him:

Q.

"…I have come across a remarkable discovery: I can express a property of all transcendental functions of the form $\int \phi(z)dz$, where $\phi(z)$ is an arbitrary algebraic function of z, by an equation of the following form (denoting $\int \phi(z)dz = \psi(z)$):
$\psi(z_1) + \psi(z_2) + \cdots + \psi(z_n) = \psi(\alpha_1) + \psi(\alpha_2) + \cdots + \psi(\alpha_n) + p$,
where $z_1, z_2, \ldots$ are algebraic functions of an arbitrary number of variables (n depends on this number and is in general much larger; $\alpha_1, \alpha_2, \ldots$ are constant entries and p is an algebraic/logarithmic function, which in many cases is zero). This theorem and a memoir based on it I expect to send to the French Institute, for I believe it will throw light over the whole theory of transcendental functions…."
Is this an early version of the addition theorem?

A.

You are absolutely right. I should say, though, that in the letter to Degen the theorem is stated somewhat awkwardly and in a rudimentary form, but it contains the gist of the matter. I consider the addition theorem as my most important discovery. When I arrived in Paris in the summer of 1826 I wrote up a memoir on transcendental functions that is based upon the addition theorem and presented it to l'Académie des Sciences on October 30, 1826. Cauchy and Legendre were appointed as referees, and I was eagerly awaiting their judgement.

Q.

And what was their judgement?

A.

I have not heard a word, and since more than two years have elapsed I fear that my Paris memoir is lost.

Q.

Why have you not made enquiries about this? We have been in contact with Legendre, whom you wrote a long letter last November telling him about your mathematical discoveries. But you do not mention anything about the Paris memoir, even though Legendre is one of the referees. It seems to us very strange that you did not bring this up in your correspondence with him.

A.

I guess I did not want to embarrass him by reminding him about my memoir. I did publish a short article, which appeared in Crelle's journal last December, where I treated hyperelliptic integrals using a special case of the addition theorem. In a footnote I said that I had presented a memoir to l'Académie des Sciences in 1826 treating a much more general situation. Finally, exactly a month ago, on January 6, in this very room I wrote a four-page letter to Crelle stating and proving the addition theorem in its most general form. This is the fundamental theorem upon which the Paris memoir is based. I asked Crelle to publish it, which I am sure he will. So at least the main theorem of the memoir will be saved for posterity.

Q.

We have some news for you about all this. In a correspondence with Jacobi, he tells us that after he had read your footnote in your hyperelliptic paper, he sent a letter to Legendre expressing his astonishment that the memoir you submitted to the academy in Paris has not been published. Jacobi informs us that Crelle has shown him your four-page letter, and upon reading it and putting it in context with your hyperelliptic paper, he writes:

"...die grösste mathematische Entdeckung unserer Zeit, obgleich erst eine künftige grosse Arbeit ihre ganze Bedeutung aufweise könne..." ["...the greatest mathematical discovery of our time, even though only a great work in the future will reveal its full significance..."]

A.

These are very good news! Thank you for conveying them. Jacobi's praise makes me feel humble.

Q.

Could you tell us in broad terms what the addition theorem is all about?

A.

Instead of giving you the general statement, let me state it for the very special case of hyperelliptic integrals, on which I did publish a paper in Crelle's journal which I mentioned earlier. Let n be any natural number and let $x_1, x_2, \ldots, x_n$ be any complex numbers. Then

$$\int_0^{x_1} \frac{dx}{\sqrt{f(x)}} + \int_0^{x_2} \frac{dx}{\sqrt{f(x)}} + \cdots + \int_0^{x_n} \frac{dx}{\sqrt{f(x)}} = \sum_{k=1}^{g} \int_0^{z_k} \frac{dx}{\sqrt{f(x)}}$$

where $f(x)$ is a polynomial of degree $2g+2$ or $2g+1$ with no multiple roots, and $z_1, z_2, \ldots, z_g$ are algebraic functions of $x_1, x_2, \ldots, x_n$.

A.

This is generalization of Euler's addition theorem for elliptic integrals:

$$\int_0^{x_1} \frac{dx}{\sqrt{(1-x^2)(1-k^2x^2)}} + \int_0^{x_2} \frac{dx}{\sqrt{(1-x^2)(1-k^2x^2)}} = \int_0^{z} \frac{dx}{\sqrt{(1-x^2)(1-k^2x^2)}}$$

where

$$z = \frac{x_1\sqrt{(1-x_2^2)(1-k^2x_2^2)} + x_2\sqrt{(1-x_1^2)(1-k^2x_1^2)}}{1-k^2x_1^2x_2^2}$$

Q.

There are several other areas of mathematics which we have not touched upon, where you have made fundamental contributions. Your work on infinite series and convergence criteria is one area that comes to mind. But time is running out for our interview. However, we can not end the interview without some words about the theory of equations, which you said was your favorite topic. Let us start with the aforementioned letter you sent to Legendre in November. You ended the letter with these words:

Q.

“I have had the good fortune in finding a definite rule with whose help one can recognize whether any given equation can be solved by radicals or not. A corollary of my theory is that it is impossible to solve the general equation of degree greater than four.”

Could you elaborate?

A.

A week ago I received a reply letter from Legendre, which ends with these words:

"You announce to me, Sir, a very beautiful work on algebraic equations that has as object to resolve for each given numerical equation whether it can be solved by radicals, and to declare unsolvable those that do not satisfy the required conditions. A necessary consequence of this theory is that the general equation of degree higher than four can not be solved. I urge you to let this new theory appear in print as quickly as you are able. It will be of great honour to you, and will universally be considered the greatest discovery which remains to be made in mathematics."

A.

You can just imagine with which eagerness I want to send him the memoir I am writing up – of which a rough sketch exists – presenting my theory. But I do not have the strength to work now, so I have to be patient till I regain my strength.

A.

Besides my memoir on the unsolvability of the general equation of degree $n \geq 5$, I have only published one other paper entirely devoted to the theory of equations, titled "Mémoire sur une classe particulière d'équations résolubles algébriquement". The transformation theory of elliptic functions, of which the division of the lemniscate arc in equal parts is a very special case, gives an abundance of examples of algebraic equations that satisfy the conditions of the memoir, and thus can be solved by radicals. In fact, my various papers on elliptic functions are interspersed with examples of equations that can be solved by radicals. As I said above I am working on a memoir which will attack the general problem of the algebraic solution (i.e. by radicals) of numerical equations.

Q.

Crelle told us about the letter you sent him last year dated September 25. Here is a quote from the letter:

"...It pleases me greatly that you will print my "Précis d'une théorie des fontions elliptiques". I shall exert myself to make it as clear and good as possible, and hope I shall succeed. But do you not think that it would be better to commence with this paper instead of the one on the equations? I ask you urgently."

Q.

“Firstly, I believe that the elliptic functions will be of greater interest; secondly, my health will hardly permit me occupy myself with the equations for a while. I have been ill for a considerable period of time, and compelled to stay in bed. Even if I am now recovered, the physician has warned me that any strong exertion can be very harmful.”

Q.

"Now the situation is this: the equations will require a disproportionately greater effort on my part than the elliptic functions. Therefore, I should prefer, if you do not absolutely insist on the article on equations – in that case you shall have it – to begin with the elliptic functions. The equations will follow soon afterward..."

Your comments?

A.

Well, the letter is self-explanatory, I think. There was an agreement between Crelle and myself that I should write a memoir on the theory of equations, settling once and for all the question whether an arbitrary numerical equation can be solved by radicals or not. This is a problem I have been thinking about for a long time, and during my sojourn in Paris I made an important dicovery that will be crucial for the solution. As I told you I did start writing the memoir in question, but it was interrupted by my sickness in September, so only a rough sketch exists. Crelle wrote me back admonishing me to follow my doctor's advice. Therefore I have applied all my strengths last autumn to write up my "Précis"-memoir – of which half is finished – which treats the theory of elliptic functions from a purely algebraic point of view.

Q.

So you have a lot of things on your mathematical agenda when you get well and are able to work again?

A.

Absolutely! First order of business is to complete the "Précis"-memoir. Then I will write up the memoir on equations. Another project is the theory of integrating elementary functions, especially algebraic functions, which I mentioned earlier. But perhaps the most interesting project is to use the addition theorem to invert integrals of algebraic functions, similarly to what was done with elliptic integrals. This last problem seems to me to be of the utmost importance, whose solution will throw new light on the higher transcendental functions. In fact, I do not think professor Degen was exaggerating when he in a letter to professor Hansteen already in 1821 poetically predicted that, quote:

"...The serious investigator ...would discover a Strait of Magellan leading into wide expanses of a tremendous analytic ocean."

Q.

You received a travel grant in early 1824 from the university in Christiania to travel abroad for the duration of two years to visit Göttingen and meet Gauss and to visit Paris, “the focus of all my mathematical desires”, as you yourself described it. However, there was a catch: the Collegium at the university decided that before you started your travel you should stay in Christiania for a period of two years to study, among other things, foreign languages, primarily French and German, in order to be more prepared for the journey abroad. They presumably thought you were too young to start your travel right away. Anyway, it took about one and a half year before you embarked on your journey. What was your reaction to all this?

A.

I was very unhappy that I could not start my travel right away. I was so eager to meet my peers abroad. However, in hindsight I deem it a "blessing in disguise" that I was held back. It was during the "waiting period" of one and half year before my journey that I, in total mathematical isolation, so to say, in Christiania, made my most important discoveries. During my foreign travel I would spend most of my time to work out and develop the ideas that came to me then. The only really new things I learned after I left Norway was when I came across Cauchy's "Cours d'Analyse" while in Berlin. This spurred me to rigorously examine infinite series and convergence questions related to such. I published a memoir in the first volume of Crelle's journal (in 1826) treating this.

Q.

What you are telling us does remind us about what happened to Isaac Newton when he, at almost the same age as you, was forced to spend the years 1665-67 in total scientific isolation at his birthplace Woolsthorpe in Lincolnshire because of the plague that befell Cambridge. It was during these two years that he made his most fundamental discoveries.

A.

I don't like to be compared with Newton. He is in a class all by himself. However, I see the analogy with his isolation and mine.

Q.

Can you say something about your work style? For instance, have you had moments of epiphany, where all of a sudden you see the light and the solution of a problem you have struggled with appears to you completely transparent? In this connection we mention that your friend Christian Boeck, who was one of the Norwegians that you shared an apartment with in Berlin, tells us that you habitually woke up during the night, lighted a candle and wrote down ideas that you woke up to.

A.

I have experienced many times that the solution to a problem I have been working on appears to me in a sudden flash of insight. This only happens after having worked on the problem intensely over a long period, though.

Q.

Could we ask you a somewhat vague question? Do you have what we may call, for lack of a more precise term, a "philosophy of mathematics"? To put it more down to earth: what is your thinking about mathematics, how it ought to be pursued with respect to formulating problems and how to go about solving these problems?

A.

Let me answer your question this way:

One should give a problem such a form that it is always possible to solve it, something that one can always do with any problem. In presenting a problem in this manner, the mere wording of it contains the germ to its solution and shows the route one should take. The reason why this method has been so little used in mathematics is the extreme complication to which it appears to be subject to in the plurality of problems, especially if these are of certain general nature; but in many of these cases the complication is only seemingly and vanishes at first sight.

A.

I have treated several topics in analysis and algebra in this manner, and although I have often posed myself problems that surpass my powers, I have nevertheless attained a great number of general results that have shed a broad light on the nature of these quantities, the knowledge of which is the object of mathematics.

Q.

We can not end this interview without hearing your opinion on the various mathematicians you met during your sojourn in Berlin and Paris. Holmboe showed us the letter you sent him from Paris in October 1826, where you gave some rather biting and harsh characterization of some of the French mathematicians.

A.

First of all, August Leopold Crelle, who I met and befriended in Berlin, is the most kind and honourable man one can imagine. Crelle, though not a mathematician himself but a construction engineer, founded "Journal für die reine und angewandte Mathematik" in 1826, and I have published almost all my papers there. Crelle has, and still is, working indefatigably to secure a professorship for me in Berlin.

A.

Now Paris and French mathematicians: I made only a fleeting and superficial contact with them during my sojourn in Paris. The only one I found amiable was Legendre. The French are extremely reserved towards strangers. Everybody works for himself without concern for others. All want to instruct, and nobody wants to learn. The most absolute egotism reigns everywhere. Let me exemplify by mentioning two episodes where I met Cauchy and Laplace, respectively.

A.

I showed Cauchy, who is a first class mathematician – no question about that – my memoir on transcendental functions, which I dare say without boasting is good. He would hardly cast a glance at it. The memoir was presented to l'Académie des Sciences and Cauchy was appointed as one of the referees. More than two years have elapsed, and I have not heard a word. He has presumably lost it.

Laplace, who is a small, lively man, suffers from the "disease" that he interferes with the speech of others. I have tried to talk with him, only to be abruptly interrupted, so I was not able to communicate with him.

Q.

We end this interview by asking about your interest aside from mathematics, like music, literature, art, etc.?

A.

I love the theatre, and this is by far my greatest interest aside from mathematics. Everywhere I have travelled I have gone to the theatre to see a play. From Paris I remember with fondness a play by Molière where the eminent actress Mademoiselle Mars played the leading role.

As for music I am simply tone-deaf. Crelle used to have musical soirées which I attended, I listened politely, but the music did not move me at all.

I should mention one other interest of mine: playing cards with my friends. The main thing is the social aspect of card-playing. I love being among friends, and if I am alone for a long time I get depressed.

Q.

Thank you for this most interesting interview!