Julius Petersen 1839–1910
A biography

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1. The person Julius Petersen

The emergence of Danish mathematics on the international scene towards the end of the nineteenth century, and the dominant role played by geometry in Danish mathematical research at that period is closely linked to the work of two mathematicians, Hieronymus Georg Zeuthen and Julius Petersen.

As the two professors of mathematics at Copenhagen University they set the tone of Danish mathematics for almost three decades from the 1870’s onward. Zeuthen was mainly an algebraic geometer (he also made important contributions
to the history of mathematics), and his work has stood the test of time. Petersen is not so easily categorised. His interests were manifold and the quality of his work variable. In fact, he outlived all the contributions he had made to the mathematics of his day, though several contained ideas of great originality. His claim to fame rests on a single paper, *Die Theorie der regulären graphs* [Petersen 1891a], which at the time it was written was a contribution to the mathematics of the future. It represents nothing less than the birth of a new mathematical discipline: graph theory.

By inclination Petersen was a geometer, but he was at ease in a wide range of topics: function theory, number theory, mathematical physics, mathematical economics, cryptography and—in the end—graph theory. He had an unfailing eye for seeing geometry in unexpected places, and the way he used it is often ingenious. In cryptography and mathematical economics he made contributions which today are seen as pioneering.

Throughout his work he strove for intuitive clarity and utmost transparency. His papers, and especially his books, were renowned for their succinctness and masterful exposition. To him, Beauty was Truth, and it happened more than once that he lost sight of rigour in his enthusiasm over the elegance of an idea, sometimes irretrievably so. Embarrassing as this might be, it was not his most serious failing. To preserve, as he claimed, the independence of his way of thinking, he made it a habit to read as little as possible of other people's mathematics, a habit that undoubtedly came to him quite naturally—as it does to many others, but he pushed it to extremes. The price to pay for this independence was high: he spent a nonnegligible part of his time rediscovering known results. How many times this may have occurred, there is no way of telling; we only know of those cases where a referee points to his startling ignorance of the literature, or where an already existing result had to be removed from a submitted paper, or where a paper did not get published at all.

Petersen was a born problem-solver, and like many of their kind, moved frequently from one field to another without leaving a lasting mark on any. The unique exception to this statement is graph theory, where he ended up laying the foundations of a genuine new theory rather than solving a problem. Finding himself in virgin territory, he was for once free from the danger of discovering what others had discovered before, and the absence of this concern (little though he would admit to it in public, and perhaps even to himself) may well have given his mind the extra degree of freedom it needed to do truly timeless work. Petersen was 50 at the time and approaching the end of his career in research. The other pioneering work—in economics and cryptography—he had done in much younger years. Both the great paper of his maturity and the brilliant early pieces suffered the standard fate of works that are ahead of their time: they went unnoticed or met with outright rejection. One can imagine Petersen's feelings;

1 For a biography of Zeuthen (see Kleiman [37]). His approach to the history of mathematics is discussed by Lützen and Purkert [45], and his mathematical research by Noether [52].
but this is not to imply that his life was one of disappointment—far from it. He started from very modest beginnings, and by hard work, some luck and some good connections, moved steadily upward to a station of considerable importance. In Denmark his name was known to many educated people (because of his influence on the teaching of mathematics in the high-schools), and his work received royal recognition through the award of the Order of the Dannebrog (1891). Among mathematicians he enjoyed an international reputation. At his death—which was front page news in Copenhagen—the socialist newspaper *Social-Demokraten* correctly sensed the popular appeal of his story: here was a kind of Hans Christian Andersen of science, a child of the people who had made good in the intellectual world.\(^2\)

About Petersen the man we know almost nothing. The outward details of his life are quite easy to follow, but we have been unable to trace any personal correspondence, and it is doubtful whether any has survived. What little we know comes mostly from obituaries. While these tend to give a one-sided picture, they agree in a number of characteristic details: Petersen’s vigour, frankness, wit, and lack of rancour. An obituary in the weekly magazine *Illustreret Tidende* [August 14, 1910] describes him as being straightforward and kindhearted, unperturbably diligent in his work, and filled with a keen interest in the life around him—far more in life itself than in its reflection in art. In the mathematical journal, *Nyt Tidsskrift for Matematik* [A21 (1918) 73–77], the editors C. Juel and V. Trier wrote:

> A bright man, an original thinker, a master of exposition, always in good mood, never smallminded in his judgement, ready to break a lance with anybody in unfailing trust in the soundness of his own arguments, quick-witted and bubbling in debate, not given to hard feelings or bitterness against his opponents, a personality on a large scale, unsnobish, not academic in his behaviour, sometimes rather rough in his manners—this was the impression one had of the recently deceased Julius Petersen.

(Juel and Trier 1910, transl. from Danish)

In a newspaper obituary, the later professor at the Polytechnical School, J. Mollerup, wrote:

> Among the Danish mathematicians he was the embodiment of the best sense of humour and the most vigorous joy in life. Bursting with good health, he filled his place in life, both in work and festivity. Frank and good-natured was his fun. It was a pleasure when his handsome figure with the beautiful clever head showed up. . . . Many are the anecdotes told about his merry doings.

(*Berlingske Tidende*, August 5, 1910, transl. from Danish)

\(^2\) Prof. Petersen død, *Social-Demokraten*, Aug. 6, 1910.
Only few such anecdotes have survived. One, repeatedly told, is that he was sometimes himself baffled in his lectures and could not see what his own books claimed ‘is easy to see’.

2. Childhood and youth (1839–1871)

Peter Christian Julius Petersen was born on the 16th of June 1839 in Sorø on Zealand. His parents were Jens Petersen (1803–1873), a dyer by profession, and Anna Cathrine Petersen (1813–1896), born Wiuff. After preparation in a private school, he was admitted in 1849 into second grade at the Sorø Academy School, a prestigious boarding school, founded by king Frederik II in 1586. From 1822 it also admitted local boys living outside the school. After second grade there were two lines of study, one leading to the ‘student’-examination after grade 7, the other leading to the ‘real’-examination after grade 5. The student-examination was a prerequisite for admission to the university. Main subjects in the ‘student’-classes were Latin, Greek and mathematics. Other compulsory languages were French, German and Hebrew. In the ‘real’-classes there was no Latin, Greek or Hebrew, but French, German and English, and the emphasis was on mathematics and natural science.3 It was this latter programme that was followed by Petersen.

In his ‘doctorvita’,4 written for Copenhagen University when he obtained the Dr. phil. degree in 1871, Petersen wrote:

Mathematics had, from the time I started to learn it, taken my complete interest, and most of my work consisted in solving problems of my own and my friends, and in seeking the trisection of the angle, a problem that has had a great influence on my whole development.

(transl. from Danish)

One of his friends, living four houses further down the street,5 was Hieronymus Georg Zeuthen (1839–1920), who was also admitted to Sorø Academy in 1849, and who later became the leading professor of mathematics at Copenhagen University and secretary of the Royal Danish Academy of Sciences and Letters. Petersen and Zeuthen always remained close friends.

Petersen was taken out of school after his confirmation in 1854, because his parents could not afford to keep him there, and he worked as an apprentice for

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3 Efterretninger om Sorø Akademis Skole og Opdragelsesanstalt i Skoleåret 1855–1856.
4 Indbydelsesskrift til Kjøbenhavns Universitets Aarsfest til Erindring om Kirkens Reformation, Kjøbenhavn 1871, 143–144. Every candidate for the doctorate was required to write a brief autobiographical sketch.
5 The main street of Sorø, then Realgade, now Storgade. The houses of the families Petersen (Nr. 16) and Zeuthen (Nr. 6) can still be seen. Both have undergone alterations, especially the Petersen house.
almost a year in an uncle's grocery in Kolding, Jutland. The uncle died, however, and left Petersen a sum of money that enabled him to return to Sorø, pass the real-examination in 1856 with distinction, and begin his studies at the Polytechnical School in Copenhagen.

Petersen had two younger brothers, Hans Christian Rudolf Petersen (1844–1868) and Carl Sophus Valdemar Petersen (1846–1935), both of whom were also admitted to Sorø Academy. The elder died early from tuberculosis; the younger passed the real-examination in 1864 and later became a successful business school leader in Odense on Funen, where a foundation still bears his name. Moreover there were two sisters, Nielsine Cathrine Marie Petersen (1837–?) and Sophie Caroline Petersen (1842–?).

In Copenhagen, Julius Petersen passed the first part of the civil engineering examination in 1860 and the same year answered the university prize question in mathematics on the history and properties of the cycloid. There was only one answer, submitted anonymously under the motto 'Lysten driver Værket' (Pleasure Drives the Work). The judges did not think that the answer merited the gold medal but rated it nevertheless as satisfactory ('accessit'). When the envelope containing Petersen's name was opened, it was discovered that the author was not even entitled to participate in the competition since he had not passed the student-examination.

Already in 1858 Petersen had published his first book, an elementary text on logarithms [Petersen 1858]. Moreover he contributed to *Mathematisk Tidsskrift*, from the start of the journal in 1859. By 1860 he had decided to study mathematics at the university, rather than to continue with the more practical second part of the engineering education. However, his inheritance was used up and he now had to teach to make a living. From 1859 to 1871 he taught at one of Copenhagen's most prestigious private high-schools, *Det von Westenske Institut*, also called Bohr's School after its principal (a grandfather of Niels and Harald Bohr), with occasional part-time teaching jobs at other private schools. In 1862 he passed the student-examination, and could now enter the university. That same summer he married Laura Kirstine Bertelsen (1837–1901) and seven months later the couple had their first son Aage Wiuuf-Petersen (1863–1927). Later the family increased with another son, Thor Ejnar Petersen (1867–1946), and a daughter, Agnete Helga Kathrine Petersen (1872–1941).

The 1860's must have been a rather difficult period. From 1863 Petersen was supported by *Det Smithske Stipendium*, but his teaching load was very heavy (6–7 hours a day, 6 days a week), and he had only little time left for his own studies. Several autobiographies by former pupils tell that as a teacher Petersen was very clever, sharp-witted and concerned for good students, but also that he was

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6 Census 1855, Rigsarkivet, Copenhagen.
7 *Indhydelsesskrift til Kjøbenhavns Universitets Aarsfest i Anledning af Hans Majestæt Kongens Fødselsdag*, København 1860. In this account the identity of the ineligible author is not revealed. Petersen does so himself in the autobiography mentioned in footnote 4.
completely unable to keep discipline in class. One pupil wrote:

This genius worked himself to pieces to support wife and children, at the same time studying for the student-examination, later for the magister and doctoral degrees, without any other economic means than the very poor teacher's salary. He did not get the sleep he needed, and seemed sometimes to be living in another world. Perhaps he sat pondering over problems far too advanced for us.

(Henriques [25, p. 64, transl. from Danish])

Another recalled:

He was only interested in pupils who already understood everything. His quick brain could not occupy itself with fools. But he was funny, full of jokes and stories and they came plentifully. It we did not understand the mathematics immediately, we were left on our own.

(Jacobsen [29, p. 44, transl. from Danish])

In 1866 Julius Petersen obtained the degree of magister in mathematics at the University, and the following year he received a gold medal for a treatise on the equilibrium of floating bodies, summarized in [Petersen 1869e]. Again, Petersen's was the only submission. This time he was eligible, and his answer was found to be highly satisfactory and original. However, in their evaluation, the two judges, the professor of astronomy d'Arrest and the professor of mathematics Adolph Steen, expressed their great surprise at “the author's complete ignorance of Dupin's important Mémoire sur la stabilité des corps flottants, presented to the Academy of Sciences in Paris in 1814.”8 This is the first of many instances where Petersen's lack of knowledge of the literature caused raised eyebrows.

During his years as a high-school teacher, Petersen had come to realize the importance of geometric reasoning in mathematics and the essential role played by geometrical constructions in mathematical education. He had also discovered his talent as a writer of textbooks. His mathematical tastes had become firmly set. It is surely not just a consequence of the demands of the marketplace that the five textbooks he wrote during the 1860's were all on geometry. Yet he still did not have a doctorate, and he was 30 when he finally started to work seriously on his dissertation [Petersen 1871a].

3. Geometric constructions (1866–1879)

Many of Petersen's early contributions to Mathematisk Tidsskrift were problems or solutions in the problem section. From 1863 the problems were primarily concerned with geometric constructions. In many cases Petersen widened the

scope of his answers and wrote small systematic discussions of particular geometric methods. Remarkable in this respect are three papers from 1863, 1865 and 1867 on 'rotations', that is, in Petersen’s terminology, rotations followed by dilatations from the centre of rotation. He included many of these ideas in his textbook *Plane and Spherical Trigonometry* [Petersen 1863a] and in particular in his classic *Methods and Theories for the Solution of Problems of Geometrical Construction* [Petersen 1866a]. In its second edition (1879), Petersen added several new methods, in particular ‘inversion in a circle’ which he had discussed in *Tidsskrift for Mathematik* in the meantime [Petersen 1875d].

The first edition of *Methods and Theories* appeared only in Danish (except for a plagiarized Norwegian version and five problems published in *Nouvelles Annales de Mathématiques* 1866), but the 1879 edition was immediately translated into German, English and French, and later into Italian, Spanish, Russian, Polish and Dutch. It went through many editions—the eighth Danish edition appeared in 1926 and was reprinted until 1959, the last English reprint in 1960, and the most recent French reprint as late as 1990. Thus, it probably enjoys the distinction of being the most widely published work by any Danish mathematician.

This small book contributed more than any of Petersen's other works to earn an international reputation for its author. In his first letter to Sylvester, Petersen revealed his own special affection for it:


*(Letter: Petersen to Sylvester, February 2, 1879, St. John's College, Cambridge)*

Thus Petersen's aim was to systematize geometric problem solving. In the introduction he admitted that analytical geometry offered a uniform approach to such problems but he also pointed out that the analytic solutions are often much more complicated than the purely constructive ones. The usual textbooks presented geometric problem solving as a kind of guessing game in which to develop the necessary skill should be left to those with an innate ability for it. Indeed, the schools placed far too little emphasis on problems of geometrical construction, depriving themselves, in Petersen’s view, of one of the most efficient tools for reaching the goal of mathematical education: to sharpen the
ability to observe and recognize relationships, and to develop clear and logical thinking. Systematization of the known methods would place this tool within the reach of the average gymnasium (high-school) student.

Generally a geometric problem asks for the construction of a geometric figure satisfying certain conditions. To this end Petersen formulated the following simple rule.

Image one of the given conditions for the required figure removed, and seek the loci of the points of the figure thus rendered indeterminate.

(Petersen 1866a, English edition 1879c, p. 5)

Often the removed condition stipulates that one of the points of the figure must lie on a given curve (a line or a circle). The intersection between this curve and the corresponding locus determines the position of this point and consequently the required figure. It is obvious that in order to use this method, one must know many loci, and indeed, in the first chapter of the book, Petersen gave many descriptions of plane loci (i.e., loci that are straight lines or circles).

Exercise 48 in Methods and Theories may serve as an elementary example. It asks for the construction of a triangle $ABC$ when the side $a$, the height $h_a$ and the ratio $b : c$ of the other two sides are given. To this end, one first constructs the side $BC$ of length $a$. Then one imagines the condition on the height $h_a$ removed, and seeks the locus of the points $A$ whose distances to $C$ and $B$ are in the given ratio $b : c$. This locus is a circle which can easily be constructed. The removed condition stipulates that $A$ must lie on one of the two lines which are parallel to $BC$ and whose distance from $BC$ is $h_a$. The intersections of these two lines and the circle determine the position of $A$.

If loci cannot be directly applied, Petersen suggested the following general procedure.

From the drawn figure try to form another, in which the relations between the given and sought elements are more convenient.

(Petersen 1866a, English edition 1879c, p. 6)

He explained how this could be achieved by means of various geometric transformations such as translation, rotation, and, in the second edition, inversion in circles. These methods and theories were explained in rather short sections while the bulk of the book consisted of exercises after each section (256 in the first edition and 410 in the second edition). The exercises were designed so that they would teach the active reader to apply the various methods to a long series of problems. They range from easy school exercises to very difficult problems that caused even professional mathematicians trouble—Petersen’s correspondence contains several letters asking for solutions.

The last problem of the 1879 edition was the famous Malfattian problem: In a triangle $ABC$ to inscribe three circles so that each of them touches the two others
and two of the sides of the triangle. In 1826, Steiner had claimed without proof that each of the simultaneous tangents in the point of contact of two of the demanded circles would also touch two of the circles which are inscribed in the three triangles into which $ABC$ is divided by its angle bisectors. This theorem that would lead to a construction, was proved by Schröter in 1877 in Crelle's Journal. Petersen, who does not seem to have been aware of Schröter's work, tried for many years to find a purely geometric solution to the Malfattian problem and as late as the end of 1878, he told H.A. Schwarz that he had still not succeeded. However the following year he found two solutions which were simpler than that of Schröter. He included one of them in the second edition of Methods and Theories, and on the invitation of Schwarz, he published an account of both constructions in Crelle's Journal [Petersen 1880b], where they were admired for their elegance.

In Tidsskrift for Matematik the second edition of Methods and Theories was reviewed very positively (Zeuthen [79]). Zeuthen praised Petersen's exposition and admitted that although several of the problems in the book could be solved by way of the new (algebraic and projective) geometry, the elementary methods used by Petersen could not be said to be included in the general theory. Finally, although these simpler techniques were not new, Zeuthen felt that they were not "won for mathematical science" until Petersen had ordered them methodically.

Zeuthen also wrote a letter to the editors of the Bulletin des Sciences Mathématiques in which he gave examples of the rather sophisticated geometric problems the students were asked to solve at the entrance exam to the Polytechnical School. This high level, Zeuthen concluded, was a result of the wide use of Methods and Theories. The letter was quoted in the Bulletin in an anonymous laudatory review of the French translation (probably Darboux was the reviewer, cf. Section 7).

A problem, posed at the entrance exam to the Polytechnical School in 1874, may serve as an illustration:

Given a sector $ACB$ of a circle, inscribe in it a similar sector $acb$ such that its center is a given point $c$ of the circular arc $AB$ of the first sector. Where should $c$ be on $AB$ for $ca$ to be parallel to $CA$? In this case, what is the ratio between the radius $r$ of the inscribed sector and the radius $R$ of the given sector, and how does this ratio vary when the angle $ACB = v$ varies from 0 to $\pi$?

The difficult part of the problem is the first question; however, when interpreted correctly, its answer follows by a simple application of Petersen's theory of rotation: just rotate the line $AC$ through an angle $v$ around $c$. The intersection of the rotated line with $CB$ is a.12

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11 Schwarz to Petersen, Dec. 2, 1878.

12 The problem and the solution which we have just outlined, is given in Tidsskrift for Matematik (3) 4 (1874), 182. The author is not mentioned but presumably is Petersen. It is clear that for the given solution to work, the points $a$ and $b$ must lie on the segments $CB$ and $AC$, respectively. If this requirement is dropped, there are other solutions when the angle $v$ exceeds $2\pi/3$. 
One of the students at the exam in 1874 was Henrik Pontoppidan (1857–1943), who later became a famous author and Nobel laureate (1917). In his autobiography he relates that one of the examiners had wanted to fail him:

That was Julius Petersen, the youngest of the mathematics professons at the School, who originally had been a chap in a little grocer’s shop, and only became a student when he was 23. But at the same time as he passed his student-exam he made a big name for himself among the mathematicians—a world name—with a thin little book, ‘Methods and Theories’, that was like a revelation. The most involved geometric construction problems changed by his method into trivialities as if by magic. Now one of the four problems at the written exam required knowledge of these new methods, but my teacher had not taken the trouble to tell me about this miraculous book that created so much noise, so I went to the exam without knowing anything about it. Nevertheless I succeeded in solving the problem, but true enough with much more trouble and labour than really necessary. The old professor Steen argued against Julius Petersen that by solving the problem in my own way, I had shown ability for independent thinking and should not be failed. But Julius Petersen never forgave me and always referred to me as “the bloke who had the impudence to come to the exam without knowing my Methods and Theories”.

(Pontoppidan [57, pp. 27–28, transl. from Danish])

Strange words from Petersen, great reader that he was himself!

4. The doctoral dissertation (1871)

Since Antiquity mathematicians had been occupied with the problem: Which geometric problems can be solved by ruler and compass? From the time of Descartes it had gradually been realized that this problem could be translated into the algebraic problem of solving equations by square roots. Following Gauss’ breakthrough in his Disquisitiones Arithmeticae (1801) (Gauss [21]), Wantzel [72] had shown in 1837 that the duplication of the cube and the trisection of the angle could not be solved by these means. It is unlikely that Petersen knew of the impossibility when, as a schoolboy, he tried to trisect angles (see Section 2), but already in his book on trigonometry in 1863 he reduced this problem to a cubic equation and remarked in a footnote:

Since \( z \) is a root of a cubic equation of general form it cannot be expressed by square roots, from which follows the impossibility of the trisection of the angle by ruler and compass ⋅⋅⋅

(Petersen 1863a, p. 16, transl. from Danish)
In his 1866 edition of *Methods and Theories* he emphasized that he only considered constructions by ruler and compass, and that under this restriction many seemingly simple problems could not be solved. In addition to the angle trisection he particularly mentioned the quadrature of the circle. He probably just referred to a general belief among mathematicians, for the impossibility of this latter problem was not established until Lindemann in 1882 proved the transcendence of π.

In addition to this four line discussion of the possibility of ruler and compass constructions, the 1866 edition of *Methods and Theories* only contained a remark to the effect that the intersection of two independent conic sections cannot be constructed. (From now on, construction means construction by ruler and compass.) In the 1879 edition, however, Petersen added an appendix where this problem was discussed, based on the insight he had gained in his doctoral dissertation *On equations solvable by square roots; with applications to the solution of problems by ruler and compass*, published in Danish in 1871. By then Petersen was aware of the works of his predecessors:

The impossibility of the trisection of an angle by ruler and compass was first proved by Wantzel; if you add Gauss' division of the circumference of the circle (Gauss [21]) there is hardly any other important work dealing with this question.

(Petersen 1871a, Notes, transl. from Danish)

In fact, in 1827, Abel had also contributed to the question by showing that Gauss' results can be carried over to the division of the lemniscate [1].

Petersen gave the subject a completely new twist. Inspired by his simple rule (formulated in Section 3 above) for geometric problem solving he addressed the question: What characterizes curves (loci) whose intersection with any arbitrary straight line can be determined by ruler and compass? On July 19, 1870, he had carried his investigations so far that he sketched his results to his Norwegian colleague Sylow. This letter, which marked the beginning of an extensive correspondence,\(^{13}\) gives a good impression of Petersen's methods and main results in this field. Moreover, it explains how these questions forced him to take up algebraic research and reveals the first problems he encountered in this area.

In the hope that you still remember me from the time we met in Kristiania\(^{14}\) I take the liberty of asking you a piece of information. In your talk (Sylow [66]) you mentioned a memoir by Galois on the equations of degree \(2^p\) that can be solved by radicals. Now, I would like to know where I can find it; because in this memoir I may find means to complete a proof that has occupied me for many years. If it does not bore you I shall briefly present the matter. I want to prove the following

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\(^{13}\) The first five letters of this correspondence are translated and annotated in (Lützen [44]).

\(^{14}\) They both participated in the 10th Meeting of the Scandinavian Natural Scientists in Kristiania (Oslo) in July 1868.
Theorem:
The conic sections are the only curves whose intersection with an arbitrary straight line can be constructed by ruler and compass.

The proof runs as follows:

1. When a problem can be solved by ruler and compass, the required quantities can always be expressed rationally or irrationally by \( \sqrt{ } \) [i.e., by square roots] in terms of the given quantities. Thus, if \( x_1 \) is the abscissa of the intersection of \( f(x, y) = 0 \) and \( y = ax + q \) we must have \( x_1 = \mathcal{M} \), where \( \mathcal{M} \) contains lots of square root signs.\(^{15}\) Consider \( \mathcal{M} \) as being expressed in terms of the smallest possible number of different \( \sqrt{ } \); they may appear several times. Then none of the appearing \( \sqrt{ } \) can be expressed rationally in terms of the others. \( \mathcal{M} \) is written as a single fraction with rational denominator; thereby there will not appear new \( \sqrt{ } \) if the multiplication of two such is not performed.\(^{16}\)

2. When \( x_1 \) is a root of the equation \( f(x, ax + q) \) [which determines the intersection of the curve and the line] all the quantities resulting from \( x_1 \) by changing the signs of \( \sqrt{ } \) are also roots [cf. Petersen 1871a, pp. 2-3].

3. If there are other roots, the curve must be compound [reducible] and we only consider the branch with these roots [cf. Petersen 1871a, p. 3].

4. If there are \( n \sqrt{ } \), there are \( 2^n \) combinations [of signs]. If some of these coincide they must combine to \( 2^{n-1} \) or \( 2^{n-2} \) or etc. [different values] and the degree of the equation must be one of these numbers [cf. Petersen 1871a, pp. 4-5].

5. If the equation of degree \( 2^n \) can be solved by \( \sqrt{ } \), one of the roots can be expressed by \( n \) such different \( \sqrt{ } \), where each can appear several times (this is the point I have not yet proved completely).

6. In one of the \( \sqrt{ } \), in which there is no other \( \sqrt{ } \), appear either (a) both \( \alpha \) and \( q \) or (b) \( \alpha \) alone or (c) \( q \) alone.
   a) To each value of \( \alpha \) corresponds a value of \( q \) that gives the \( \sqrt{ } \) the value 0; then the \( 2^n \) intersections coincide to \( 2^{n-1} \), so that infinitely many tangents can be drawn to the curve, which is impossible; therefore \( n = 1 \). (b) and (c) in a similar way. Thus there can only be one \( \sqrt{ } \) or 2 intersections, implying that the curve is a conic section [cf. Petersen 1871a, pp. 24-26].

\(^{15}\)Here we see what Petersen means by construction of the intersection of the curve whose polynomial equation is \( f(x, y) = 0 \), and the line \( y = ax + q \). He means that the intersection must be constructed from \( \alpha, q \) and the coefficients of \( f \).

\(^{16}\)This and many of the following laconic arguments are treated in more detail in the Dissertation [Petersen 1871a, see pp. 2 and 23].
Sylow answered that Galois had never written a paper especially devoted to solution by square roots, but he assured Petersen that his point 5 was correct. In fact, Sylow had deduced it from his famous theorems which were not published for another two years (Sylow [67], cf. Scharlau [61], Kargemo [41]). Fearing that such a general derivation would not satisfy Petersen, Sylow had tried to find a simple proof, but without success. Therefore he was astonished when six weeks later, Petersen sent him an elementary proof based on a general study of the factorization of polynomials over $\mathbb{Q}$ [cf. Petersen 1871a, pp. 9–18].

Sylow continued to suggest extensions of Petersen’s theorem, for example to the case where one can construct the intersections between the curve and all the lines of a pencil of lines, as long as the apex of the pencil does not lie on the curve or on a double tangent [cf. Petersen 1871a, p. 34]. Moreover, he indicated how duality would lead to the following theorem: The conic sections are the only curves to which one can construct tangents from an arbitrary point [cf. Petersen 1871a, p. 27].

From the beginning, Sylow had referred Petersen to the standard literature on group theory, in particular Betti’s papers and Serret’s and Jordan’s books, and on December 25, 1870, Petersen concluded “I shall hardly get any further until I know the theory of substitutions [sic]”. However, before he embarked on a study of group theory, he composed his doctoral dissertation, in which he organized the material mentioned above in a more systematic way, beginning with the theory of solutions to equations by square roots and ending with the application of this algebraic theory to geometric constructions. The geometric part contained many theorems characterizing curves whose intersections with a given family of curves are constructible. In addition to the theorem discussed in the Sylow correspondence, let us also mention its counterpart:

There are no other curves than the circle and the straight line whose intersections with an arbitrary circle can be determined by ruler and compass.

(Petersen 1871a, p. 31, transl. from Danish)

In 1874 Petersen continued to study curves whose intersections with a pencil of lines through a point outside the curve are constructible. For a general algebraic curve he found a necessary condition and for fourth degree curves he solved the question completely [Petersen 1874b].

Let us return to the doctoral dissertation, where Petersen also showed how to use his impossibility theorems constructively. For example, he had, by an ingenious use of the theory of rotations, shown how ‘in a given triangle $ABC$, to inscribe another triangle congruent to a given one’. This is exercise 243 in the 1866 edition and exercise 377 in the 1879 edition of *Methods and Theories* [cf. also Petersen 1871a, p. 39]. (See Fig. 1.)

To solve this problem by Petersen’s simple rule (cf. Section 3), we should

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17 Sylow to Petersen, Sep. 13, 1870.
18 Petersen presented these results in a talk in the *Matematisk Forening*, November 12, 1874.
imagine one of the conditions of the figure removed, say the condition that $a$ falls on $BC$. Then $a$ will describe a curve, and the true position of $a$ is found as the point of intersection between this curve and $BC$. This point is constructible, according to the exercise above, and since $BC$ can be any line (within certain limits), Petersen concluded from the general theorem that the curve must be a conic section. Thus:

When a triangle, congruent to a given one, slides with its two vertices on two given straight lines, then the last vertex will describe a conic section.

(Petersen 1871a, p. 41, transl. from Danish)

This, and the many other locus properties of conic sections, proved by Petersen from his general theorem, can of course be deduced in other ways, but Petersen's method was new and elegant.

Let us conclude this summary of Petersen's dissertation by quoting his beautiful application of a generalization of the preceding theorem to a proof that it is impossible with ruler and compass to construct a triangle congruent to a given one with its vertices on three given circles.

If this problem were solvable by ruler and compass, the locus of one vertex of the given triangle, moving with the two other vertices on given circles, would be a straight line or a circle. However, that is not the case because in the special case where the circles degenerate into straight lines, the locus is a general conic.

(Petersen 1871a, p. 40, transl. from Danish)

Sylow, whom Petersen did not even mention in the dissertation, received the work with some reservation. About the first part he wrote to Petersen:

The question of solving equations by square roots is probably the simplest general problem one can pose in the theory of the solution of equations, and as you will see from Jordan's work *Traité des subst. etc.*
one can, now that Galois theory has become generally known, treat much more extensive problems.

(Letter: Sylow to Petersen, September 19, 1871, transl. from Norwegian)

More specifically, Sylow pointed out that many of the theorems in Petersen's first part were known already by Abel, the only important exception being the problematic point 5 in the letter stating that if an irreducible equation of degree $2^n$ is solvable by square roots, its solution contains $n$ square roots. In his famous paper of 1872, Sylow deduced a more general theorem about solution by $p$th roots, a theorem he had already indicated in his first letter to Petersen. Though Sylow had undoubtedly priority over Petersen, he explicitly referred to Petersen's proof of the special case (Sylow [67, p. 589]), thereby showing greater generosity than Petersen.

In the letter of September 19, 1871, Sylow even pointed out a mistake in the first proof of this theorem [cf. I Section 8 in Petersen 1871a], but it was not crucial. Indeed Petersen had already detected the problem,$^{19}$ and had therefore added another proof [Petersen 1871a, Section 10]. He had, he wrote to Sylow, only left the flawed argument as a 'practical guide'!

Still, Sylow found Petersen's observations "interesting in several respects. I look forward to seeing what Zeuthen will say about it in Mathematisk Tidsskrift".$^{20}$ Zeuthen's review, however, was brief and purely summarizing, and despite a more complete summary by Hoppe in Jahrbuch über die Fortschritte der Mathematik, the dissertation gained little international recognition until some of the central ideas appeared in German in Petersen's algebra book [Petersen 1877b].

5. Social and economic engagement (1871–1877)

At the beginning of the 1870's Copenhagen had a population of about 200,000. In terms of the upper class and the intellectual circles this meant that Copenhagen was still a rather small town. Everybody knew everybody else, and being anonymous was impossible. The growing working class, including a few thousand child workers, were living in very poor conditions. In 1871, Copenhagen society had to cope with two profoundly disquieting events, not related but perhaps felt to be. One was the Paris Commune and the organization also in Denmark of a socialist movement. The second event, considered even more serious, was a young Dr. phil. Georg Brandes, aristocratic and Jewish, giving a series of lectures as private docent at the University on Main Currents in Nineteenth Century Literature. These lectures were an attack on the established view that the role of literature was to create and describe ideals. Instead realism was called for. This was felt as an

$^{19}$ Petersen to Sylow, Sep. 28, 1871.
$^{20}$ Sylow to Petersen Sep. 19, 1871.
attack not only on the general cultural background of society and the values on
which it was based, but also on the authority of the church and the conservative
politicians. Brandes was strongly attacked in all the newspapers; at the same
time, the papers did not give him any opportunity to answer his critics (Knudsen
[39]).

The lectures by Brandes are by far the most famous ones ever held at
Copenhagen University, and they mark what is now called the Modern
Breakthrough in Danish cultural life. Needless to say, they eclipsed all the
fashionable subjects of discussion that had occupied the intellectual scene in
Copenhagen earlier in the autumn of 1871. One of these subjects had been the
question of life after death, and it is in this connection that, somewhat
surprisingly, we encounter Petersen. Earlier in the year he had begun to translate
into Danish (with H.P. Holst) the book *Le lendemain de al mort, ou la vie future
selon la science* by L. Figuier, a prolific popularizer of science. This very
speculative, pseudo-scientific book, claiming the sun to be the resting place of the
souls from Earth and the other planets, was used by the church to show what
absurdities science can give rise to. Petersen himself criticized the main thesis of
the book (Fenger [16, p. 222]). What his motives for the translation were, we do
not know, but it had the effect of making his name known outside the narrow
circle of mathematicians and high-school teachers.

Late in 1871, probably in response to the fact that the socialist movement had
become a reality of everyday life, Petersen published a privately printed
pamphlet (under his initials JP) of socio-economic nature. Entitled *Contribution
to the Solution of the Social Question* [Petersen 1871b] (in Danish), it contains an
investigation of how a redistribution of society's goods can be made in favour of
the workers. His background as a mathematician shines clearly through. In an
almost axiomatic theory he obtains the result that taxation of spending above a
certain basic level is the way to the goal. His style is entertaining and vivid, still
very readable today. In January 1872 the pamphlet was brought to public
attention through a review in *Nyt Dansk Maanedsskrift* (New Danish Monthly), a
progressive cultural magazine that had been founded some months earlier and
was adopted by a group of young contributors sympathetic to Brandes. In spite of
its reputation of being 'red' (it was anything but), *Maanedsskrift* berated
Petersen's pamphlet as being 'socialist and communist in the extreme' (Møller
[48]), but this is difficult to see with modern eyes.21 Petersen's heroes were rich
entrepreneurs wanting to create new enterprises and see their fortunes grow, but
themselves living ascetic lives.

In *Contribution to the Solution of the Social Question*, Petersen investigated
many aspects of social policy, among them the problem of old age pension. The
prevailing point of view of the time restricted the role of the state to that of
organizing a minimal structure to facilitate and encourage self-help. In contrast,

21 In the same spirit, the copy of [Petersen 1871b] in the Library of the Danish Parliament is
classified under: *National Economics-Socialism.*
Petersen argued for a 'pay as you go'—principle, where old age pension should be financed by taxation on spending. He wrote:

It seems to me to taste of irony when the workers are told: Your economic position is bad, you cannot get by with what you have; here is some good advice: Each year, just save so and so much of what you cannot get by with, then your condition will improve sometime in the future.

(Petersen 1871b, transl. from Danish)

Petersen's arguments as well as his conclusions make him an early predecessor of the modern social policy doctrine called the Mackenroth thesis (cf. J.H. Petersen [54]).

In the winter of 1872 the attacks on Brandes became furious. A society, with the neutral name Literaturselskab (Society for Literature), was created to back him. Its aim was to support free research as the final judge of what is true or false. Among the six members of the governing body we find Brandes himself, Jens Peter Jacobsen, Holger Drachmann (both among the most famous Danish writers and poets) and Julius Petersen (Brandes [7, p. 79]). This society was considered by the general public to be very extreme and terrible! It held a few meetings (to which it admitted women), published a few small books, but then dissolved itself. More long-lived and of the usual professional kind were the National Economic Society, formed in the fall of 1872, also with Petersen participating actively from the start, and The Danish Mathematical Society, founded in 1873 by among others Petersen, Zeuthen and T.N. Thiele, then director of the insurance company Hafnia, but from 1875 professor of astronomy. These two societies still exist.

In June 1872, Petersen proposed to Brandes what was to be the most ambitious publishing project of the Literaturselskab: a translation/adaptation to Danish conditions of Manual of Political Economy, a popular account of the basic laws of national economics, by the British parliamentarian and economist Henry

22 The creation of Literaturselskabet is the most colourful episode of the Modern Breakthrough. Of the numerous works dealing with the latter, the following mention Petersen: Borup [6], Brandes [7], Fenger [16], Knudsen [39].

23 Brandes speaks of Petersen as 'a mathematics professor with an excellent brain'. One may wonder how it came about that a mathematician found himself in such illustrious literary company. Most likely, Petersen was introduced into literary circles already in the early 1860's through some of his fellow high-school teachers who were active as writers, translators or historians of literature. One such literary colleague—who may also have had considerable influence on Petersen's political and social thinking—was Carl Michelsen, known as 'the Red Michelsen', who together with Petersen and Frederik Bing (see footnote 30) was employed at Maribo's School in 1862/63. Although not a socialist himself, he entertained close relations with the Socialist Party (Michelsen [46]). Drachmann, Michelsen and Petersen were known as the socialist sympathizers among the people who gathered around Brandes, with Michelsen farthest on the left (Fenger [16, p. 236]). It seems likely that Drachmann (apart from his stature as a literary figure) and Petersen were included in the governing committee of Literaturselskabet as a gesture of openness towards the left that would not make the society vulnerable to accusations of being in league with the socialists.
Fawcett. The translation was done jointly by Petersen and Frederik Bing, mathematical director of the State Life Insurance Company (Bing and Petersen (1874)). In grappling with Fawcett, Bing and Petersen developed their own ideas on economics, and these found their expression in a paper (in Danish) entitled *The Determination of the Rational Wage Rate* (Bing and Petersen (1873)). In 1982 the American economist John K. Whitaker wrote about it:

The authors presented a theory of striking power and novelty. Focusing on the marginal choice between mechanized and handicraft production of various consumer goods, they outlined a comprehensive neoclassical theory of wages, interest, and prices for a multicommodity capitalist economy. Their analysis was marginal, but based on activity analysis rather than calculus. Unfortunately, their ideas seem not to have been grasped by the economic theorists of their day, and the article had little or no impact. It still remains virtually unknown, despite the appearance of an English translation (1962), yet it is one of the preeminent contributions to distribution theory of the latter half of the nineteenth century, ranking with the work of von Thünen and Walras in originality and generality of conception.

(Whitaker [74, p. 333])

The paper was discussed at a meeting of the National Economic Society in September 1873, but the established economists did not understand Bing and Petersen's axiomatic mathematical development. The meeting ended with Petersen challenging his opponents to investigate any question; he would then attempt to prove their conclusions false. The economists reacted to such arrogance by turning a cold shoulder on Bing and Petersen's paper. The editors of *Nationalekonomisk Tidsskrift* (J. of National Economics), who had originally been of the opinion that it represented an interesting and unusual point of view, and had envisaged publishing a reply to it, now shrugged it off as not being serious, declaring that the debate had clearly shown the new method of 'Messrs. B & P' to be inapplicable. Petersen took no offence, but did not give up; he continued to take an active part in several debates of the National Economic Society, and published some more papers and remarks on economics. His aim was to develop economics as a true exact science, by making its assumptions clear, and using mathematical rigour in its deductions. But after 1877 there seem

24 Letter: Petersen to Brandes, June 5, 1872 (Georg Brandes Arkivet, Royal Library, Copenhagen).
25 The book was actually published in several instalments the first of which appeared in December 1872, the last early in 1874.
26 *Nationalekon.* Tidskr. 2 (1873), 248–256.
27 *Nationalekon.* Tidskr. 1 (1873), 167. Petersen's first public attack on the foundations of economics took place at the meeting of the National Economic Society on December 5, 1872. The chairman ruled him out of order.
28 *Nationalekon.* Tidskr. 4 (1874), 260. See also (Vind [71]).
to be no further social or economic papers by Petersen, and he quit the National Economic Society in 1882. However, he played an active role in practical economic life as member of the governing bodies of the insurance company *Hafnia* (from 1885), *Arbejderbanken* (The Workers’ Bank) and *De Forenede Rygnings Snedkerier* (United Carpentry Workshops), among others. Also, from 1875 to 1878 he served as advisor to a government commission on the condition of the workers, and from 1896 to 1909 on the governing board of the relief organization *Centralkomiteen*.

Whitaker comments the neglect of Bing and Petersen’s economic theory, and the termination of their economic writings, in the following way.

The final broad reason for the neglect was Bing and Petersen’s apparent failure to press their cause. They were content to cast their bread upon the waters and turn to other things. Their ambitions lay elsewhere, but science is a social process and, unfortunately, merit does not automatically produce recognition or disciples.

(Whitaker [74, p. 351])

From the 1850’s, when they studied together at the Polytechnical School, Bing and Petersen were close friends (J.P. Jacobsen speaks of them as Castor and Pollux [Jacobsen [30]]). Apparently Bing followed Petersen’s work closely; for example, he contributed to *Methods and Theories* and made an improvement in Petersen’s graph factorization theory [Petersen 1891a, p. 200]. Bing's father was a wealthy businessman with many interests—he owned the most fashionable store in Copenhagen, participated in founding *Bing & Grøndahl* (a porcelain factory, today internationally well known) and in establishing a printing company, *Bing & Ferslew* (from which he soon withdrew, however). Frederik Bing’s brother, Herman Bing, was one of the three founders of *Politiken*, today one of the largest Danish newspapers. Perhaps it was some of these connections

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30 Frederik Moritz Bing (1839–1912) finished his studies at the Polytechnical School in 1862, served briefly as an officer’s cadet in the war of 1864, and after some years as a businessman went to Paris to study mathematics. In 1871 he received an appointment as mathematical director of the State Life Insurance Company which the Danish government was in the process of setting up. He remained in this post until his retirement.

Throughout his life he maintained an active interest in mathematics beyond his professional work as an actuary (see Nielsen [51] for a list of publications). In *Dansk Biografisk Haandleksikon* (vol. 1, p. 146) it is stated that although he published only a handful of papers, his significance for Danish mathematics was nevertheless great, inasmuch as a number of the ideas contained in Petersen’s papers originated with him, and that he willingly left them to his friend to publish. How highly Petersen valued Bing’s judgement can be seen from a remark he often made: “When I find something, I never hold it for certain until Bing has seen it; but if he says that it is right, then there is no longer any doubt” [Obituary for F. Bing, *Illustreret Tidende*, April 7, 1912].

31 Bing’s contribution is acknowledged in the preface to all editions of *Methods and Theories*

32 The other two were Edvard Brandes (brother of Georg B.) and Viggo Hørup. The paper started publication in 1884.
Petersen had in mind when he wrote to the Swedish mathematician Mittag-Leffler, the founder of *Acta Mathematica*:

Should there be attempts to make an attack [on Acta], and I am told in time, I think I can prevent it; I have rather much influence on our main newspapers, both to the right and the left.

(Letter: Petersen to Mittag-Leffler, February 20, 1888, transl. from Danish)

6. Cryptography (1875)

In 1875 Petersen brought out one more privately printed pamphlet, this time in French: *Système Cryptographique* [Petersen 1875a]. Up till then he wrote all his works in Danish, and continued to do so for several years thereafter. It is not known why in this case he switched to French (his other papers in that language did not come until more than 25 years later) or why he chose to publish privately rather than in a journal. From 1881 till 1887 he taught at the Officers’ School of the Army, but there is no evidence that he ever made any allusion to his invention of a new secret code in his teaching there. The archives of the Danish Army also do not contain any trace of this paper.

The pamphlet was rediscovered by Ole Immanuel Franksen, who gave a detailed account of it in his book *Mr Babbage's Secret* (Franksen [18]). Petersen first describes how he has been able to decode all the most well-known ciphers. He then lists the basic requirements which a good cipher should meet—including some of no little insight, for example that it should be error-correcting—and goes on to present his new system, a so-called fractionating cipher.

It uses a two dimensional coordinate representation of the letters, randomized by permuting the row and column indices according to some numerical double key, which is based on a key-word or key-sentence ('Le jour et la nuit' in Petersen’s example). Thus each letter is represented by two coordinates, and the message to be encoded is first replaced by the sequence of the coordinates of the letters. Conceptually, this fractionating of the indices, and then destroying the basic difference between the 1st and the 2nd coordinate, is the main idea of the cipher. The obtained sequence of numbers is rearranged diagonally in a 2-dimensional array. To destroy the diagonal pattern, the columns are permuted according to a further numerical key, also obtained from the same key-sentence. In the resulting permuted array, the columns are read two and two, from top to bottom, giving the coordinates of the letters of the enciphered message. To decode the message the operations should be performed in inverse order, and Petersen says that for 100 letters this can be done in 12 minutes, or 8 minutes.

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33 We only know three original copies: one in the Royal Library, Copenhagen, one in the Library of Harvard University (originally in the possession of Charles Sanders Peirce) and one in the Schleswig-Holsteinische Landesbibliothek, Kiel (probably from the possession of Petersen's German translator, R.v. Fischer-Benzon). According to *Dansk Bogfortegnelse for Aarene* 1869–80 it was available in the booktrade.

34 Franksen [18, p. 107].
Julius Petersen 1839–1910

using a special apparatus with movable slips, or 5 minutes for trained army personnel.

Like Petersen’s economic theory his cipher made no impact at the time (as far as we can tell). The first to gain recognition for a fractionating cipher was Felix Delastelle who, 27 years later in 1902, published the so-called bifid, a cipher of considerable importance in the development of cryptography (cf. Franksen [18]). As far as we know, Petersen never returned to cryptography; this seems to be another instance of a problem that must have occupied him intensively for a period, until he found a satisfactory solution and moved on to something else.

7. The theory of algebraic equations (1877)

In line with the activities of Literaturselskabet of generally promoting science, Petersen agreed to write a book in Danish on The Theory of Algebraic Equations for the book series Universalbibliothek, published by Høst & Søn. The Universalbibliothek consisted of books on a level between that of scientific journals and popular accounts. However, when Petersen’s book appeared in 1877, it was outside the series, probably because it was too advanced.

Petersen had revealed his first interest in algebra in an elementary note [Petersen 1865b] on elimination procedures. His next paper [Petersen 1874a] on this subject, which he presented to the Scandinavian Meeting of Natural Scientists in Copenhagen in 1873 came about in a way characteristic for him:

I began to read it in Serret’s book, but it was 8 pages long; so I would rather do it myself.

(Quotation in: Crone [12, p. 8, transl. from Danish])

Petersen’s more elegant proof was included in The Theory of Algebraic Equations, together with other earlier results, such as another simplification of Serret’s methods [Petersen 1876b], an elegant derivation, using determinants, of the equation whose roots are the nth power of the roots of a given equation [Petersen 1867d], and certain tricks in connection with Sturm’s theorem [Petersen 1869d].

Petersen’s book was modelled after J.A. Serret’s Cours d’algèbre supérieure (Serret [62]), but with his concise style Petersen succeeded in covering almost the same material in one fourth of the space. In addition to the algebraic methods of solution, the book covered numerical procedures. His initial plan did not include group theory, and he treated as many subjects as possible without Galois theory, e.g. the impossibility of the solution of the general equation of degree higher than four and the so-called Abelian Equations. Moreover, Petersen included his own theory of solution of equations by square roots.

35 A description of the scope of the Universalbibliothek as well as an announcement of Petersen’s book appears on the back of one of the published books of the series (Herbert Spencer, Om Opdragelse, 1876). The publishing house Høst & Søn still exists but no records from that period have been preserved.

36 According to a letter from Petersen to Sylow of March 6, 1877.
Having thus completed the first three sections of the book with methods similar to those of Abel, he decided to follow Sylow's advice from 1870–71 and study group theory and Galois theory. Here he ran into great difficulties which one can follow in his resumed correspondence with Sylow. It began as follows.

København, July 15, 76.

Hr. Overlærer Sylow.

When you see this letter, you will no doubt immediately conclude that I need your help and I must admit that usually trouble and duress is needed in order to make me fetch paper and pen... I have thus been forced to tackle the theory of substitutions but since it is so new to me, I feel somewhat uneasy when I engage in making changes; I therefore hope that you will spend a couple of hours in examining this part of my manuscript... If you do not have the time or the inclination to read it, there is a theorem which I have not found in Serret or in Jordan (who both have a special case of it) and where I have a feeling that I may have fallen into many pitfalls. I shall here tell you the outline.

A transitive group that contains the alternating (or complete) group on p letters, but no alternating group of more letters must (1) be generated from mp letters $a_1, a_2, \ldots, a_p, b_1, b_2, \ldots, b_p, c_1, \ldots$, (2) contain the alternating groups on all the m sets of letters, (3) be 1-ply transitive and not primitive and (4) be of order $q(p!)^m/2^a$ where a is at least 0 and at most m, and q is the order of a group of m letters.$^{37}$

(Letter: Petersen to Sylow, July 15, 1876, transl. from Danish)

Sylow answered by pointing out certain pitfalls which Petersen had indeed fallen into, and during the subsequent correspondence, Petersen gradually reformulated his theorem into the one on 'transitive groups that contain transitive subgroups' which he included on p. 289 of the book. All through the correspondence Sylow corrected Petersen's mistakes, suggested generalizations and referred Petersen to the published literature, in particular to Jordan's papers which Petersen only knew imperfectly. Petersen, for his part, admitted that "in the theory of substitutions I still feel as if I were walking on a tightrope".$^{38}$

Even when Sylow referred Petersen to Jordan's papers, Petersen did not always read them. For example Sylow quoted a theorem by Jordan concerning degrees of transitivity, but Petersen gave up "since I cannot find a simple proof".$^{39}$ Although Sylow hastened to send Petersen a summary of Jordan's proof on May 30, it came too late to be incorporated in the book, so the theorem appeared without proof in Section 174. In fact Petersen's formulation of the theorem is incorrect (one must assume that the group is not the symmetric or the alternating group).

$^{37}$ Petersen wrote $[p]$ for $p!$
$^{38}$ Petersen to Sylow, April 25, 1877.
$^{39}$ Petersen to Sylow, May 27, 1877.
In his letter of May 27, Petersen further asked Sylow to confirm that Jordan had made an error stating that the alternating group on $k$ letters is $(k - 1)$-ply transitive (it is in fact $(k - 2)$-ply transitive). Sylow answered by referring to Jordan's own correction in the *Bulletin de la Société Mathématique* vol. 1, and added that Jordan also corrected another mistake in that paper. When Petersen sent the printed book to Sylow on August 21, 1877, he admitted that his last remark had been of great help:

> In fact I had included the theorem by Jordan that you mentioned to be incorrect. I gave a different proof (No. 175), but since I trusted the theorem, I had overlooked the exception until your letter made me look over my proof once more.

(Letter: Petersen to Sylow, August 21, 1877, transl. from Danish)

The theorem in question appeared as follows in the book.

> If an $n$-ply transitive group $G$ contains a group $H$ which permutes with the substitutions of $G$ [i.e., $H$ is a normal subgroup of $G$], then $H$ is at least $(n - 1)$-ply transitive. (There is one exception.)

(Petersen 1877b, Section 175, transl. from Danish)

The problem is really greater than Petersen admits with his hastily added parenthesis. As the proof goes by induction on $n$, and the exception occurs in each step of the induction, it is quite unclear what the proof proves, if anything.

During their correspondence, Petersen had mentioned in November 1876 that it had caused him a lot of trouble to try to prove Sylow's famous theorems in a way that could be presented in his book. Sylow helped him in his next letter, and Petersen included Sylow's theorems in the book (Section 215). In the entire book, Petersen only referred to Sylow in this connection.

Petersen concluded the book with a chapter on Galois theory. He used Galois's definition of the Galois group by means of rational functions of the roots, and he applied the general theory to the Abelian equations he had discussed previously and to equations of degree $p^n$, where $p$ is a prime. The latter was a generalization of results in his doctoral dissertation.

The *Theory of Algebraic Equations* became a success. Next to *Methods and Theories* it was Petersen's most widely read and quoted work. It appeared in 1878 in a German translation and in 1897 in a French edition, revised by Petersen's friend Hermann Laurent. The Danish edition was reviewed very favorably by J.P. Gram in *Tidsskrift for Matematik*. He correctly remarked that although Petersen had borrowed much from Serret, both in the overall structure and in details,

> there is almost always something new in Dr. Petersen's proofs, be it a new and original twist of the argument or a remark he finds occasion to

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40 For his "remarkable translation of the works [sic] of Petersen", Laurent was awarded the Order of the Dannebrog [Bull. Trimestriel Inst. Actuaires Français 19 (1908), p. 6].

41 Jørgen Pedersen Gram, well known through the Gram-Schmidt Theorem.
make. Compared to Serret, the proofs by Jul. Petersen generally appear with greater clarity.

(Gram [23, p. 27, transl. from Danish])

In particular Gram mentioned the following novelties: A generalization of Descartes' sign rule, a simplified proof of the fundamental theorem of algebra, a new proof of Waring's formula for symmetric functions, and, finally, an improved proof of Bezout's theorem. The last proof, however, did not deserve Gram's praise. Petersen discovered that the argument was flawed and corrected it the following year in *Tidsskrift for Mathematik* [Petersen 1879a]. In connection with his translation, Laurent raised a much deeper criticism of Petersen's proof:

\[ J'ai \text{ le regret de vous faire observer que la démonstration que vous donnez (Section 35 et Section 36) du Théorème de Bezout, de même que celle qu'en a donné Serret et peut-être Bezout lui-même est absolument illusoire, elle ne prouve rien du tout. Elle repose sur une hypothèse. C'est que les équations du premier degré auxquelles vous ramenez l'Élimination des puissances de } x, y, z \text{ (considéré comme des inconnues au premier degré) sont bien déterminées, ont si vous voulez leurs déterminants différents de zéro. Or ces équations ne sont pas des équations générales, elles sont des fonctions compliquées des coefficients des équations proposées. Il est très probable que vous rencontrez des impossibilités dans vos calculs. Au moins faudrait-il prouver que vous n'en rencontrez pas.} \]

(Letter: Laurent to Petersen, June 8, 1895)

In his translation Laurent replaced the proof with a better one. He also pointed out that Petersen's two proofs of the fundamental theorem of algebra were not entirely rigorous:

\[ \text{La première suppose la continuité des courbes algébriques, qu'il faudrait établir, la seconde, celle de Cauchy, suppose l'existence d'un minimum qui a été démontrée par O. Bonnet et Darboux.} \]

(Letter: Laurent to Petersen, February 21, 1895)

Gram to whose review we shall now return, also had certain misgivings, in particular about the group theory and the Galois theory (for example he considered the problematic induction proof in Section 175 'almost incomprehensible'). He found these sections 'much too condensed', and he judged it to be one-sided that Petersen (like Serret) went so far in this direction without mentioning the theory of Invariants and Covariants at all:

But it [Galois theory] says nothing about specific given equations when we do not know anything about their roots in advance, and for that reason it is not very applicable in practice. Therefore it necessarily requires a supplement and this is naturally found in the theory of
transformations, the theory of invariants and covariants and transfor-
mations of higher order whose main object is precisely to classify equations
by way of the relations between the coefficients, to seek the correspond-
ing relations between the roots and to examine which changes the
equations undergo when subject to transformations.

(Gram [23, p. 30, transl. from Danish])

Gram published a similar review in the *Jahrbuch über die Fortschritte der
Mathematik*. In France the book (more precisely, its German translation, 1878)
was received less enthusiastically in a brief review by Darboux in his own
*Bulletin*. After emphasizing Petersen's debt to Serret, Darboux continued:

*L'Ouvrage de M. Petersen contient, condensées dans un espace rela-
tivement étroit beaucoup de théories importantes d'Algèbre
supérieure, et l'on doit savoir gré à l'auteur d'avoir fourni aux étudiants
une nouvelle occasion de s'instruire et de se préparer à la lecture des
Traités où les mêmes questions sont reprises avec tout le développement
qu'elles comportent.*

(Darboux [13, p. 275])

Petersen, no doubt, considered his book an improvement of Serret's work and
not an introduction to it. Moreover, since Darboux did not mention the novelties
in Petersen's book, except for one, but criticized Petersen's proof of the
unsolvability of the quintic, Petersen was dissatisfied with the review. Apparently
he told Darboux about his feelings so when the French edition of *Methods and
Theories* appeared in 1880, Darboux wrote to Zeuthen:

*Notre Collègue, M. Petersen, a fait un recueil de problèmes de
géométrie qui me paraît sortir de l'ordinaire, comme tout ce qu'on lui
doit···Pourriez-vous nous donner une analyse de ce travail? Je vous en
serais bien reconnaissant. Je lui avais fait un compte rendu de son
algèbre dont il n'a pas été tout à fait content. Je désirerais effacer cette
mauvaise impression.*

(Letter: Darboux to Zeuthen, May 1, 1880)

Zeuthen, however, felt that a French reviewer would be more suitable and
wrote to Petersen:

Darboux himself would be the best to do it and it is a pity that you have
made him unhappy with your remarks against his criticism.

(Letter: Zeuthen to Petersen, added on the letter from Darboux to
Zeuthen, May 1, 1880, transl. from Danish)

As we mentioned above in Section 3, the laudatory review in the 1881 volume
of the *Bulletin des Sciences Mathématiques* was anonymous, but it was probably
written by Darboux.
Other French mathematicians were more enthusiastic about Petersen's algebra, in particular Laurent, who in 1895, when asking Petersen for permission to translate it, payed him the following compliment:

Quand j'ai lu votre livre (en allemand), j'ai tout de suite dit, l'auteur n'est pas un allemand, son style est trop clair pour cela: je ne m'étais pas trompé.

(Letter: Laurent to Petersen, February 14, 1895)

In the French edition (1897) Petersen seems to heed Gram's criticism of almost 20 years earlier: he added a chapter dealing with invariaints and covariants of binary forms. However, this chapter is little more than an almost textual translation of three of his papers which had originally been published in Danish [Petersen 1880a, 1881a, 1889b]. Apparently he was convinced that the royal road to invariant theory passed by the so-called semi-invariants, and the three papers had been written to give a systematic account of this idea. About the relationship between invariants and algebraic equations they say nothing, and their inclusion in the book is, at best, surprising.


The professor of mathematics at the Polytechnical School, Fr. A.V. Kolling (1833–1871), became suddenly ill in 1870 and died the next summer. To fill the vacancy, and at the same time to introduce a reform of the teaching, a competition was held. Each applicant had to give two lectures on topics announced 24 hours in advance, one in analytical geometry and the other in rational mechanics. Petersen was the only competitor. He held the lectures satisfactorily, and in September 1871 was appointed as docent of mathematics (cf. Steen [64]).

The Polytechnical School, founded in 1829 by the discoverer of electromagnetism H.C. Ørsted (1777–1851), was still rather small. Henrik Pontoppidan, who entered the school in 1874 at the age of 17, gives his impression of it in an autobiographical novel:

He [Lykke Per, the hero of the novel] had imagined it as a kind of temple, a solemn workshop for thoughts, where the future happiness and welfare of liberated mankind was being forged under the lighting and thunder of spirit. He found an ugly and unimpressive building in the shadow of an old bishop's residence; and inside, some dark and depressing rooms, reeking of tobacco and food, where some young men stood bent over small tables, others sat with long pipes and read their notes, or played cards on the sly. He had imagined his future teachers as

\[^{42}\text{For basic invariant-theoretic definitions see footnote } 70.\]
fiery preachers of the holy gospel of Science, but he met in the lecture-rooms some old, dried-out schoolmaster types...  
(Pontoppidan [56, p. 44, transl. from Danish])

There were three main lines of study: mechanics (mathematics), applied science (chemical engineering) and civil engineering. During the 45 years 1829–1873 a total of 713 students were admitted, and of these 303 graduated. However, the number of students was increasing—between 1869 and 1878 a total of 351 students were admitted (cf. Steen [64]).

The mathematics programme—which was also followed by the mathematics students from the University—lasted four semesters:

1. Analytic geometry, theory of functions and principles of differentiation.
3. The application of differential and integral calculus to geometry and integration of differential equations.
4. Rational mechanics.

It had been the practice to start this cycle of courses every second year (in odd years), but in view of the increasing number of students this had become problematic. With Petersen’s appointment a new cycle was started also in even years, for the first time in 1874. With minor modifications in 1884, Petersen taught the 4 semester cycle beginning with the analytic geometry in the fall of even years. The students who entered in odd years were taught the mathematics cycle by Professor Steen (cf. Lektionskatalog of Copenhagen University). During the period 1881–87 Petersen was, as we have already mentioned, also employed as professor of the Officers’ School of the Danish Army. The courses he taught there were similar to those at the Polytechnical School: analytic geometry in 2 and 3 dimensions, introduction to infinitesimal calculus, and algebraic equations, all for the second year students.

Petersen wrote four textbooks for his courses at the Polytechnical School, the first of these in the spring of 1877, while the production of his *Theory of Algebraic Equations* was temporarily stopped because the printer had gone bankrupt. It owes its title *Analytic Plane Geometry II* to the first semester of the above curriculum (volume I was an elementary schoolbook [Petersen 1873a]), but *Introduction to Projective Geometry* would have been a more appropriate title. It was reviewed very favorably by the Danish master of modern geometry, Zeuthen:

> The book shares with other works by the same author the valuable merit of a textbook that it burdens the reader with little and still gives him much.

(Zeuthen [77, transl. from Danish])

According to Petersen, Zeuthen even contemplated to use it as an introduction to his own advanced lectures at the university.

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43 Petersen to Sylow, March 6, 1877.
44 Petersen to Sylow, April 23, 1877.
Petersen’s remaining textbooks for the Polytechnical School covered the curriculum of the last semester. This trilogy on statics [Petersen 1881b], kinematics [Petersen 1884a] and dynamics [Petersen 1887b] was translated into German, and the first volume into Hungarian as well. The three books give a concise introduction to the subject, beginning with the parallelogram of forces and ending with the Hamilton formalism. Although they contain many pseudo-practical problems, they are very mathematical in nature, and a substantial part of the kinematics is in fact devoted to one of Petersen’s favorite subjects: geometric loci. For example, he used the theory of instantaneous rotations to show that if a right angle turns in such a way that its two sides constantly touch an ellipse, the vertex will describe a circle.

Like Petersen’s other textbooks, his mechanics book contained his own personal points of view. In particular we shall mention two ideas which he had published in two previous papers.

In his first paper on mechanics [Petersen 1869b], after his prize-winning essay [Petersen 1867b], Petersen showed how the principle of virtual velocities which is normally limited to mechanical systems with holonomic constraints, could also be used on systems with friction: the virtual displacement should simply be given a direction that makes an angle $\theta$ with the tangent to the surface (determined by the constraints) such that $\tan \theta = \text{coefficient of friction}$. This paper was also published in an Italian translation two years later, and thus became Petersen’s first paper in a foreign journal. The translation was made by Beltrami on the initiative of Cremona.45

Second, Petersen showed in a paper from 1884 that if the shape of a string under the action of given forces is known, then one can immediately determine the trajectory of a point mass under the same forces. This principle, which he used in his book on dynamics, was in fact not new. Möbius had published it in his *Lehrbuch der Statik* (1837) but it is characteristic that Petersen rediscovered it without being aware of the work of his predecessor.

In his textbook on statics, and in particular in a paper from the same year [Petersen 1881d–1882a], Petersen gave an account of graphical statics which was more elegant than the classical works of Culmann and his successors. His criterion for the existence of a stress diagram of a given system of linked rods was of particular importance.

Through these and his later writings on mechanics he became an authority in this field to such an extent that in the fall of 1899 Klein asked him to write the section on elementary dynamics for the *Encyclopädie der Mathematischen Wissenschaften*. Petersen agreed to do so in letters of November 11 and 17, 1899,46 but for unknown reasons nothing came of this project. As an old man Petersen declared that if he could live his life again, he would devote it to mathematical physics (Juel and Trier, *Nyt Tidsskrift for Matematik* A21 (1910) 73–77).

45 Cremona to Petersen, Dec. 3, 1869.
46 Nachlass Klein, Univ. Bibliothek Göttingen.
9. Style of exposition and method of research

In all reviews of Petersen’s works, research papers as well as textbooks, his style was praised for its elegance. In a biography of Petersen, C. Juel wrote:

The requirement, to unite a scientific exposition with a beautiful and natural language, has in this country begun with Julius Petersen.

(Juel [32, transl. from Danish])

Conciseness was another characteristic feature of Petersen’s writing. He always sought the shortest and clearest arguments and left out everything that he considered superfluous. In many instances he ignored special cases and even exceptions to rules, and since he left out so many details, his mathematics was often hard to read.

His aim was not primarily rigour, but visuality or ‘Anschaulichkeit’ (we shall use this German word for lack of an English equivalent). As an example of such an ‘anschaulich’ argument we shall quote his simple derivation of Wilson’s theorem:

Let $p$ be a prime and let a circle be divided into $p$ equal parts. Denote the dividing points by 1, 2, 3, . . . , $p$ and let $12\cdots p$ denote the convex or nonconvex polygon we get by joining 1 with 2, 2 with 3 . . . and finally $p$ with 1. By permutation of the numbers we get new polygons. The number of permutations is $p!$, but since each polygon can be denoted in $2p$ ways, there are only $(p - 1)!/2$ figures. $(p - 1)/2$ of these are regular, that is, they cover themselves when they are turned an arbitrary number of divisions forward. The rest are congruent in groups of $p$, since by rotation every polygon can occupy $p$ different positions. Thus $p$ divides $(p - 1)!/2 - (p - 1)/2$ or $(p - 2)! - 1$ from which we obtain Wilson’s theorem by multiplication by $p - 1$.

(Petersen 1872b, transl. from Danish)

Petersen’s elegant ‘anschaulich’ style can easily seduce a reader of his works to overlook serious problems and, as pointed out by Juel, Petersen was sometimes seduced himself:

Julius Petersen was a man of ideas; he was pleased with a good idea; his face could shine when he said: ‘Now I have got it!’ However it also happened that, overjoyed by a good idea, he would neglect something which hindered the use of the idea.

(Juel [32, p. 87, transl. from Danish])

We have seen some examples of this in Petersen’s correspondence with Sylow. Juel gives another example, namely Petersen’s deduction of the maximal value of the positive roots in a real polynomial; it is elegant, but basically flawed [Petersen 1877b, Section 100].
According to (Zeuthen [80]), Petersen could very quickly make himself acquainted with new areas of mathematics. He would be completely absorbed by a problem for weeks, trying to find an elegant solution, but he rarely stuck to the same subject long enough to make lasting innovations. He was a sharp problem-solver rather than a penetrating researcher.

As Petersen admitted in a letter to Sylow: 'I read very little',\textsuperscript{47} and when he read, he only read so far that he understood the problem; then he tried to find his own solution. He preferred to ask his friends about what was known in a special area, and as we have seen in the case of Sylow and shall see in the case of Schwarz, he astonished them with his unfamiliarity even with the central works. According to Juel [32], Petersen thought that he would loose some of his independence by reading what other people wrote. Certainly this often lead him to more elegant proofs than those of his predecessors but it also meant that many of the results which he believed were new, had in fact been discovered earlier. In one case this even led to a charge of plagiarism (cf. Section 10). For this reason most of his work had only little influence on the development of mathematics.

Thus one might say that Petersen wasted his exceptional mathematical powers but, as pointed out by Zeuthen [81], it is impossible to guess what a person could have accomplished had he behaved differently.

\section*{10. Miscellaneous papers (1870–1890)}

Petersen published all his papers before 1887 in Danish. This should not be regarded as a sign of provincial mediocrity, but rather as a manifestation of a nationalist and Scandinavian revival. Hans Christian Ørsted in particular had successfully argued that Danish could, and should, replace Latin and German as the languages of Danish science, and the German wars of 1848 and 1864 had encouraged this development. Thus it is symptomatic that in mathematics, Zeuthen's and Petersen's doctoral dissertations were the first ones to be written in Danish. The earlier dissertations had been in Latin.

By publishing in Danish, Petersen did not only address a Danish audience, but a Scandinavian one; he actually participated actively in several of the Scandinavian Meetings of Natural Scientists. Moreover, the \textit{Tidsskrift for Matematik}, in which Petersen published the large majority of his papers was, as the only Scandinavian journal of mathematics (until the creation of \textit{Acta Mathematica} in 1882), widely used and read in the sister nations. It even enjoyed an international reputation, in particular during the years 1871–1889, when Zeuthen edited it (from 1883 together with Gram). \textit{Zeuthen's Journal}, as it was often called, was regularly reviewed in the \textit{Jahrbuch über die Fortschritte der Mathematik} and once a year Zeuthen wrote a report on its contents in Darboux's \textit{Bulletin des Sciences Mathématiques}.

\textsuperscript{47} Petersen to Sylow, Oct. 24, 1870.
In fact, around 1880 Danish mathematics gained an international reputation it had not enjoyed since the time of Erasmus Bartholin (1625–1698) and Georg Mohr (1640–1697). (Caspar Wessel’s (1745–1818) contribution to the geometry of the complex numbers was not internationally known until the 1890’s). Of course this was primarily due to Zeuthen, who published half of his 150 greatly admired works on algebraic geometry and the history of mathematics in German or French. However, T.N. Thiele (1838–1910) and Petersen, and later J.P. Gram (1850–1916) and J.L.W.V. Jensen (1859–1925) also gained an international reputation above those of their predecessors (Andersen and Bang [2]).

Many of the results which Petersen published in Tidsskrift for Matematik became known to a wider circle because they were included in his widely circulated text books or because they appeared in translated versions in foreign journals. His papers range over a wide spectrum of subjects. In addition to elementary geometry, algebra and mechanics which have been mentioned above, Petersen contributed to higher geometry [Petersen 1869c, 1872c, 1878c, 1878d, 1881c, and 1883a], differential equations [Petersen 1859, 1862b 1872~ and 1879d (which was summarized in Mathesis vol. 1 (1881))] and number theory [Petersen 1871e, 1872b, 1879c, 1882c].

We have already illustrated Petersen’s contribution to the latter field with his proof of Wilson’s theorem. Another example is provided by the simplified proof of the law of quadratic reciprocity which he published in Tidsskrift for Matematik [1879b] and in the American Journal of Mathematics [1879c]. When Petersen sent the paper to H.A. Schwarz, the latter pointed out several unfortunate mistakes and stylistic unclarities. However, he excused Petersen with the words:

Dieser Beweis [Petersen’s] ist unter den denkbar ungünstigsten Umständen publiciert. Sie schreiben in einer Sprache [German], die nicht Ihre Muttersprache ist, an einen fremden Gelehrten und machen ihm eine Mitteilung, die vielleicht gar nicht für den Druck bestimmt ist; dieser übersetzt Ihre Mitteilung ins Englische und befördert dieselbe zum Druck, vielleicht ohne dass Sie Auch nur Gelegenheit erhalten, auf die für die Veröffentlichung bestimmte Formulierung des Textes einen Einfluss auszuüben.

(Letter: Schwarz to Petersen, February 22, 1880)

Here, however, Schwarz was much too kind to Petersen. In fact, Petersen had sent the paper to Sylvester with the following remark:

Wünschen Sie eine kleine Mitteilung für Ihre Zeitschrift, werden Sie eine solche auf der nächsten Seite in meinem besten Englisch finden.

(Letter: Petersen to Sylvester, April 20, 1879, St. John’s College, Cambridge)
Moreover, Schwarz showed the paper to Schering:

\begin{quote}
Dieser hat mir geschrieben, dass Ihr Beweis mit Hilfe einer geringen
Abänderung dem Zellerschen Beweise (Monatsberichte der Berliner
Akademie vom Jahre 1872) nachgebildet sei.
\end{quote}

(Letter: Schwarz to Petersen, February 22, 1880)

Thus, Petersen's lack of knowledge of the literature this time led to a charge of plagiarism.

The correspondence between Hermann Amandus Schwarz and Petersen bears witness to their personal friendship. It began in 1877 when, during a visit to the Scandinavian countries, Schwarz met Petersen and Bing. Later the same year, Petersen sent Schwarz a paper on integration in closed form. This paper [Petersen 1876a] which had been published in *Tidsskrift for Mathematik*, contained a generalization of Liouville's results which had been discussed by various Danish mathematicians. In the 1830's Liouville had studied evaluation of integrals in finite form (i.e., expressed in terms of algebraic, exponential and logarithmic functions) and the integration of certain linear differential equations in finite form and by quadrature. He had also tried to treat the general first order differential equation $M(x, y) \, dx + N(x, y) \, dy = 0$ but without success (cf. Lützen [43]). Petersen succeeded in treating the latter problem even for several variables. Moreover, he extended the concept of 'being given in closed form'. He said that $y(x_1, x_2, \ldots, x_n)$ is in closed form if it can be obtained by successive applications of algebraic functions and solution of a given first order differential equation $dw + N_1(1) \, dv_1 + N_2(2) \, dv_2 + \cdots + N_n(2) \, dv_n = 0$, where $N_1, N_2, \ldots, N_n$ are algebraic functions of $v_1, v_2, \ldots, v_n, w$ such that there exists an algebraic integrating factor. In this way he could allow e.g. elliptic integrals as functions in closed form. Indeed his main theorem in this case had been stated without proof by Abel. Petersen had the paper translated to German and sent it to Schwarz in Göttingen.

In the subsequent correspondence Schwarz pointed out imprecisions in Petersen's paper and suggested various changes. In particular, he insisted that Petersen write an introduction relating his results to those of earlier authors, especially Abel. Therefore Petersen turned to Sylow, who by then was preparing a new edition of Abel's works, asking him if he had found a proof of Abel's theorem in Abel's unpublished papers.48 Sylow confirmed that Abel had proved the theorem.49 On the basis of Schwarz's and Sylow's letters, Petersen then composed an improved German version in which the references to Liouville were replaced by references to Abel. It was presented by Schwarz to the Königliche Gesellschaft der Wissenschaften zu Göttingen on February 2, 1878, and published in *Göttinger Nachrichten* [Petersen 1878e].

48 Petersen to Sylow, Nov. 15, 1877.
49 Sylow to Petersen, Dec. 15, 1877.
It is characteristic that Petersen modelled his methods after those of Liouville without being aware of the more recent works of Fuchs and Frobenius. Still, Petersen's theorem was undoubtedly new and interesting. However, simultaneously with Petersen's German version, there appeared a paper by Koenigsberger, entitled *Ueber algebraische Beziehungen zwischen Integralen verschiedener Differentialgleichungen*, which covered the same ground. This paper is dated June 1877, so there is no doubt about the independence of the two. On June 2, 1878, Koenigsberger wrote to Petersen:

Hochgeehrter Herr.


(Letter: Koenigsberger to Petersen, June 2, 1878)

Thus Petersen had again been overtaken and soon Koenigsberger who, contrary to Petersen, continued to work in this field, carried it much further.

Finally we shall mention two papers by Petersen on the foundations of mathematics. They were a contribution to a philosophical dispute in *Tidsskrift for Matematik* on the nature of mathematical axioms. One participant in this dispute had argued that axioms are simply based on experience, another defended Kant's view that they are synthetic *a priori* statements. Zeuthen tried to clarify the matter but none of these authors came as close to the modern conception as Petersen who declared that

*Mathematics chooses its assumptions in an arbitrary way and deduces from them what can be deduced in a logical way. It has little scientific importance that the assumptions are chosen for practical reasons with a view to what appears in nature*

... 

When I have said that Mathematics can choose its assumptions arbitrarily, it should perhaps be added that the assumptions must not contradict each other.

(Petersen 1883a, pp. 3–4, transl. from Danish)
This almost sounds like the formalist programme Hilbert formulated 15 years later. However Petersen was not in agreement with Hilbert when it came to the meaning of mathematical objects. For Hilbert the mathematical objects were undefined, as long as the axioms were fulfilled. Petersen, on the other hand, wrote:

It is important at each point to relate the concept which the ‘Anschauung’ has given us and the concept which is determined by our definitions, in order to see if the first which we intended to treat, agrees with the latter, which in fact we treat.

(Petersen 1883a, p. 5, transl. from Danish)

The distinction is modern, but Petersen’s idea that mathematicians intend to treat the ‘anschauliche’ concepts, rather than the formally defined ones is very different from Hilbert’s formalist conception. Moreover, according to Petersen the formal mathematical concepts are determined by definitions, they are not undefined or given through the axioms. He illustrated what he meant by the distinction between the ‘mathematical definition’ and ‘anschauliche’ concepts, by discussing the meaning of point, line and plane. It is an important property of the ‘anschauliche’ point that it has no size.

Mathematically speaking, we might say that a point is something that is defined by three parameters which can vary from \(-\infty\) to \(+\infty\), such that different systems of values correspond to different points. The set [Indbegrebet] of all possible points is the space. The set of the points satisfying a certain condition is a surface etc.

(Petersen 1883a, p. 6, transl. from Danish)

With these ‘mathematical’ definitions Petersen could produce what we would call a model of a non-euclidean space: A ‘point’ is going to be a straight line:

As an example I shall call a straight line [through a given point] a point. The set [Indbegrebet] of the \(\infty^2\) straight lines through the given point is a ‘Plane’. The set of the \(\infty\) lines through the plane and in the same plane is a ‘straight line’. One can see that the usual definition applies equally well to these points and straight lines as to those we actually see [anschauen]. A straight line is determined by two points; two straight lines intersect in one point; three straight lines form a triangle (a corner with three angles), the plane can be moved so that it continues to cover itself; a straight line can be divided into congruent parts; in short, as long as we only use the definitions and not the ‘Anschauung’, the usual deductions of Geometry also apply here. In the new triangles, however, the sum of the angles is not \(2\pi\) [if angles are defined as the solid angles between the planes—Petersen did not define ‘angle’]. Thus it is clear that one cannot deduce the theorem about the sum of the angles from the formulated assumptions.

(Petersen 1883a, p. 6, transl. from Danish)
According to Petersen the reason for this surprising conclusion lies in the fact that “we treat a more extensive domain than the one we really have before our eyes”. The cure is by suitable axioms to limit the domain further so that the mathematically defined concepts of point and line are in a better agreement with the ‘anschauliche’ point and line. Petersen gave one such axiom that implies that triangles have angle sum equal to $2R$:

A plane has the property with respect to successive translations in itself that if one of its points returns to its original position, then so does the entire plane.

(Petersen 1883a, transl. from Danish)

Moreover, he showed that the sphere with ‘straight lines’ equal to circles through a fixed point satisfies this axiom but fails to satisfy the congruence theorems.

It is possible that Petersen had discovered these models without being aware of the work of Beltrami, Klein and Poincaré. When he published his paper, however, he added Poincaré’s model of the hyperbolic plane which had appeared in *Acta Mathematica* the previous year (Poincaré [55]). It has the advantage over Petersen’s model that lines are infinitely long. This is not the case in Petersen’s model and explains how he could get an angle sum larger than $2R$, although Legendre (under the implicit assumption of infinitely long lines) had proved that it must be at most $2R$.

Petersen’s paper was translated into German by Fischer-Benzon$^{50}$ who sent it to Klein for publication in *Mathematische Annalen* [Petersen 1887c]. Klein, who in 1871 had published a much more wide ranging analysis of the ‘sogenannte’ non-euclidean geometry ([38]), made the following remarks in Fischer-Benzon’s letter:


(Letter: Fischer-Benzon to Klein, November 6, 1886)

Fischer-Benzon subsequently declared that Petersen apparently did not know Klein’s paper.

$^{50}$Rudolph von Fischer-Benzon (1839–1911) was the translator of all nine books by Petersen that appeared in a German edition, and also of two books by Zeuthen. Over the years, the collaboration between Fischer-Benzon and Petersen on the translations evolved into a genuine friendship.

A native of Schleswig-Holstein of Danish ancestry, Fischer-Benzon studied at the University of Kiel, where he became a Privatdozent in mineralogy in 1865. Shortly afterwards he gave up his academic career, and for 25 years taught mathematics and natural science at high-schools (Gymnasium) in various towns in his native province. His activities as a translator earned him the Order of the Dannebrog in 1893. In 1895 he became the founding director of the Schleswig-Holsteinische Landesbibliothek in Kiel, a post he held till his death. A list of his publications (mostly botanical, but also some on elementary geometry) can be found in *Zeitschr. d. Gesellsh. f. Schleswig-Holst. Geschichte* 41 (1911), x–xii.
Ich habe ihm vor Jahren schon Vorwürfe gemacht, weil er sich so wenig um die ein-schlägige Litteratur kümmere, und er wusste nicht recht etwas darauf zu erwidern; aber gebessert hat er sich seitdem in dieser Beziehung nicht.

(Letter: Fischer-Benzon to Klein, March 3, 1887)

In the end, Klein accepted Petersen’s paper with added references to the relevant literature which made it clear that Petersen had again been outdistanced before he even wrote his paper.

Petersen’s 1883-paper also contained some remarks about the basic concepts of algebra. In particular he argued that with a suitable definition of ‘=’ there are no axioms in algebra. He returned to this subject in 1885 in a talk at the Royal Danish Academy of Sciences and Letters, printed in Tidsskrift for Mathematik [Petersen 1885a]. Now he defined algebra as that ‘theory of sign language’ which has the following characteristics.

1) The language has signs designating the objects in one or more sets (Petersen uses the word ‘group’ in the sense ‘set’ or ‘algebraic structure’). “The signs in algebra characterize different things but the differences are only specified when the theory is applied”. Moreover, the language should contain (2) signs for operations, (3) an = sign, and (4) basic equations which, as in geometry, can be chosen arbitrarily. Petersen further described homomorphic algebraic structures [ensgeldende grupper] and the introduction of ideal elements.

With this characterization of algebra, Petersen was very much up to date. Indeed, the first abstract algebraic structure, the abstract group, had only just been introduced, and the structural movement was barely visible. Petersen, however, does not seem to have had such abstract structures in mind. He did not refer to the concept of group but chose to illustrate the basic concepts of algebra with a more geometric intuitive subject: the so-called n-dimensional complex numbers. It is well known that Hamilton after many years of vain efforts had given up his plan of finding a three-dimensional analogue of the complex numbers, but had in 1843 constructed the four-dimensional quaternions whose product, however, was not commutative. Later, Cayley (1845) and Clifford (1873) had constructed noncommutative and non-associative eight-dimensional numbers ([8], [11]), and Peirce (both Benjamin and Charles Sanders), Frobenius, Weierstrass, Dedekind and others had studied n-dimensional complex numbers, i.e., numbers of the kind \( \xi_1 e_1 + \xi_2 e_2 + \cdots + \xi_n e_n \) where \( \xi_i \in \mathbb{R} \) and \( e_1, \ldots, e_n \) are so-called units. Addition is defined as in an n-dimensional vector space and multiplication is assumed to be distributive.

In the paper in Matematisk Tidsskrift, Petersen discussed 2, 3 and 4-dimensional complex numbers. The two-dimensional numbers were the usual complex numbers, though Petersen suggested an unusual geometric interpretation. In four dimensions he succeeded in finding a commutative and associative multiplication. However this multiplication does not obey the
zero-divisor law which states that the product of two nonzero elements is nonzero. This means that a unique division cannot be defined.

In fact, Weierstrass [73] had proved in 1861 that the only \( n \)-dimensional complex numbers with an associative and commutative multiplication satisfying the zero-divisor law are the real and complex numbers (\( n = 1, 2 \)). However, Petersen does not seem to have been aware of Weierstrass' work, nor of the work of his other predecessors, except Hamilton.

Fischer-Benzon also sent a translation of this paper to Klein in 1886, but Klein refused to publish it in *Mathematische Annalen*.\(^{51}\) Instead, Petersen, who had now become aware of Weierstrass' work, generalized his results to \( n \) dimensions and sent the paper to Schwarz in May 1887. The law of multiplication of \( n \)-dimensional numbers is clearly determined when the products of the unit elements \( e_i e_j \) have been fixed. Petersen showed how it was possible to characterize the product through an \( n \times n \) matrix, and how one could read off whether it satisfied the usual associative and commutative laws. Schwarz presented the paper to the Königliche Gesellschaft der Wissenschaften zu Göttingen on May 7, 1887. However, while preparing the paper for publication in *Göttinger Nachrichten*, he discovered


*(Letter: Schwarz to Petersen, May 13, 1887)*

Dedekind himself also informed Petersen of his approach to this field in a letter of May 1887. Thus again Petersen had been anticipated by another mathematician. However, this discovery only led to minor changes of Petersen's paper. Naturally he added the references to Dedekind and admitted that the main

\(^{51}\) Fischer-Benzon to Klein, Jan. 31, 1887.
theorem had already appeared there. As for the relation between his and Dedekind’s paper, he simply remarked:

Der von mir gefundene Beweis scheint mir etwas einfacher zu sein, als derjenige, welchen Herr Dedekind veröfentlicht hat.

(Petersen 1887d, p. 490)

However the problems did not end with that. On September 9, 1887, Schwarz sent two letters to Petersen. In the first one, he showed that a passage where Petersen had criticized Weierstrass’ analysis, was in fact based on a misunderstanding of Weierstrass’ paper.

Meine Meinung geht nun dahin, dass entweder Ihre Bemerkung über die Ungenauchkeit ganz wegfallen oder dass dieselbe ganz anders gefasst werden muss.

Petersen deleted the remark. In the second letter, Schwarz simplified an example which Petersen had learned from Bing, who had heard it from Schwarz. With these alterations “ich [Schwarz] erblicke in Ihrer Arbeit einen wirklichen Fortschritt.”

On October 3, Schwarz sent the proofs to Petersen but two weeks later he informed Petersen that Hölder, who had also gone over the proofs, had found an error which would be difficult to correct. Despite these problems the paper was finally published in the November 16, 1887, issue of Göttinger Nachrichten [Petersen 1887d].

Later in his life, Petersen seems to have changed his ideas about the basic structure of mathematics. After Hilbert had published his formalist programme in geometry, and algebraic structures had become well known, Petersen turned in the opposite direction, away from formalism and towards the ‘Anschauliche’:

The latest developments in algebra have resulted in a purely formal construction of the system in the air. I have declared war upon this development by claiming that we can only reach certainty when algebra rests on a concrete basis. Therefore I examine operations with ‘anschauliche’ things and try to form a system of signs for the things, so that I can operate with the signs instead of with the things. My foundation of algebra is a couple of geometrical constructions.

(Petersen 1899b, transl. from Danish)

Already in 1883 Petersen had claimed that it would be impossible to prove independence and consistency of formal axioms (Gödel proved him to be right). Only anschauliche models could help. In 1899 he took this to its logical conclusion and claimed that one must build on the ‘Anschauliche’, not on formal definitions. This, undoubtedly, was much closer to the way he had always worked in mathematics.
Also in his school books this attitude became very clear, in particular in the 1897 edition of *Arithmetic and Algebra* [Petersen 1877a]. A positive real number \( r \) corresponds to a line segment of length \( r \). To define the sum \( a + b \) and the product \( ab \) of two positive real numbers \( a \) and \( b \), Petersen just exhibited some simple geometric constructions with line segments. Thus the basic laws of algebra are derived as simple consequences of elementary plane geometry. About this method Petersen wrote:

> We have a choice between two things: We can write text books whose foundation rests on difficult philosophical considerations, which are understood neither by the pupils, nor the teachers, nor by the authors; or we can write a text book, free of all philosophical considerations because it is based on a concrete foundation, and which is scientifically completely sound \ldots. In the main, I think I have reached the goal I sought for many years: a correct and easily understandable foundation for algebra.

(Petersen 1899b, transl. from Danish)

**11. Models and instruments (1887–1895)**

Several letters in Petersen's Nachlass bear witness to his activity as a designer of mathematical models and instruments, but one can only guess about their exact nature, since we have only found a precise description of one of them.

The first of these letters was written in 1888 by "Verlagsbuchhändler L. Brill", the brother of the famous mathematician. Through Felix Klein, Brill had heard that Petersen had constructed 'eine Serie von kinematischen Modellen' which he now asked for permission to produce and sell. Petersen answered by sending him sketches of the models, but being unable to produce the models after the sketches, Brill asked Petersen for a completed model. If Petersen could not have it made, Brill would ask his brother to find a student to construct it.

The fate of Brill's plan is unclear, as is the nature of these kinematic models. It could be a series of linkages producing straight lines and one linkage doubling (or halving) an angle, which still exist at the Department of Mathematics at the University of Copenhagen. Indeed, in 1877, *Tidsskrift for Mathematik* published a translation of a survey lecture which had been given in the London Mathematical Society by its outgoing president, the Oxford mathematician H.J.S. Smith, which among other things dealt with linkages.\(^{52}\) This aroused some interest among Danish mathematicians in the simple linkages invented by Peaucellier (1864) and Lipkin (1871) which could draw straight lines, and in Kempe's recently discovered theorem (Kempe [35]) which states that any algebraic curve can be drawn (locally) by a linkage. In 1877, Zeuthen published a

paper on linkages in his Journal (Zeuthen [78]), and Petersen also wrote a popular paper on the subject [Petersen 18??], entitled *Om at tegne en ret Linie* [How to draw a straight line]. This title is exactly the title of (Kempe [36]), but Petersen does not mention Kempe in his paper. Zeuthen's paper was classified under Kinematics in *Tidsskrift for Matematik*, so it is possible that the 'kinematic models' Brill asked for were in fact these beautiful linkages of brass rods.

The next letter dealing with models and instruments was written in 1891 in connection with the meeting of the Deutsche Mathematiker-Vereinigung in Nürnberg in 1892. Walther Dyck was in charge of an exhibition and asked Petersen in two letters if he could contribute with such objects 'sei es zur Geometrie oder Mechanik oder Math. Physik'. Whether Dyck had anything specific in mind is not clear. At any rate, no models of Petersen's were exhibited.\(^5\)

In 1887 Petersen had constructed a planimeter which he presented to the Royal Danish Academy of Sciences and Letters on April 1.\(^5\) It consisted of an arm, of, whose one end o is fixed to the paper by a lead cylinder with a pin p, and whose other end f is connected to a second arm dc (or df) of length l (see Fig. 2, which is reproduced from Laurent's paper of 1896, *Note sur un nouveau planimètre inventé par M. Petersen* [42]).

When the stylus d is moved around the domain \(\Omega\) once, the area is measured as \(\int \text{dh}\), where dh is the differential displacement of the arm dc orthogonal to itself. This part of the apparatus is identical with Amsler's widely used polar planimeter from 1854. Amsler had mounted a wheel perpendicular to dc, and had measured \(\int \text{dh}\) by the total rotation of this wheel. Petersen, on the other hand, measured \(\int \text{dh}\) by two wheels a and b mounted on an axle ab which can slide

\[ \text{Fig. 2.} \]

\(^5\) According to the catalogue of the exhibition (Dyck [15]). Because of an outbreak of cholera in Nürnberg the meeting was cancelled and the exhibition postponed to the Munich meeting, 1893.

\(^5\) *Oversigt over Det Kgl. Danske Videnskabernes Selskabs Forhandlinger* 1887, p. 43.
perpendicularly to the arm \( cd \) through a bearing \( \Lambda \). In Fig. 2 the bearing is placed at the point of encounter of \( of \) and \( cd \), but according to Willers [76] the bearing was usually placed at another point along \( dc \) (see also (Jacob [28])). The total displacement of this axle relative to the bearing, which is obviously proportional to \( \int dh \), can be read off a scale.

Petersen's measurement of \( \int dh \) by the lateral displacement of wheels that were prevented from sliding sideways on the paper, might have been inspired by the hatchet planimeter which a Danish captain, Holger Prytz, had invented the same year. During the fall of 1895, Petersen sent a small copy of his planimeter to H. Laurent, who had just completed the translation of Petersen's book on algebraic equations. Laurent answered:

> Je viens de recevoir votre planimètre et je vous en remercie. C'est un joli petit joujou dont j'ai besoin d'étudier le fonctionnement et que je ne comprends pas encore très bien, quoique la pratique avec vos explications soit facile. Je vais passer mon après-midi à l'étudier. C'est très amusant.

(Letter: Laurent to Petersen, October 27, 1895)

In a letter of November 11, Laurent mentions that he would present the planimeter to the Institut des Actuaries Français. The presentation took place at the session of November 21, 1895, and created enough interest for Laurent and his colleague Héripon at the Institut Agronomique to produce a bigger and better version of the instrument (to diminish the possibility of slipping, they replaced the wheels by a pair of heavy rollers). Laurent wrote to Petersen:

> J'ai fait aux élèves de l'école polytechnique une conférence sur votre planimètre, qui les a vivement intéressée. Ces pauvres enfants qui ont tout de peine à apprendre à intégrer ont été émerveillés de voir une machine aussi simple intégrer plus rapidement qu'eux.

(Letter: Laurent to Petersen, May 12, 1896)

Though Petersen's planimeter is described by Laurent [42], Jacob [28] and Willers [75–76], it does not seem to have been a commercial success. It probably could not compete with the many planimeters constructed around this time and in particular with Amsler's simple and cheap polar planimeter.

12. Invariant theory and graph theory (1888–1899)

We have no testimony from Petersen as to what he considered his most important work. His contemporaries would perhaps have given that title to his paper in the *Göttinger Nachrichten* on integration of algebraic differential equations in closed form [Petersen 1878e], in spite—or perhaps precisely

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because of its overlap with the work of other mathematicians. It was mainstream mathematics. The significance it had at the time found its expression in personal reactions (see Section 10), and more strikingly in a record 2½-page review in Darboux’s *Bulletin des Sciences Mathématiques* (Sér. 2, vol.2(1) (1878), 272–274). What we, with hindsight, unhesitatingly recognize as Petersen’s masterpiece, drew no such response. By ill luck, it appeared at a time when the part of mathematics to which it superficially belonged, was nearing a dead end. To say that this great paper was almost stillborn, is only a slight exaggeration.

Petersen worked on *Die Theorie der regulären graphs* [Petersen 1891a] during exactly one year, from the beginning of October 1889 till the end of September 1890. Title and content notwithstanding it was, at least in its original intention, a contribution to invariant theory, a field in which Petersen had until then only produced some minor observations [Petersen 1880a, 1881a, 1889b].

It is in keeping with Petersen’s character as a problem-solver that even his most important paper owes its existence to a sequence of coincidences, perhaps more so than any other. Its genesis can be followed to a certain extent through an exchange of letters which took place intermittently during the years 1889–91 and involved Petersen, Sylvester, Klein, Hilbert and Gordan, primarily the first two (Sabidussi [59]). This correspondence (preserved in the Royal Library, Copenhagen, as part of the Petersen Nachlass, and in the University Libraries at Göttingen and Erlangen) is essentially complete, with the unfortunate exception of one of its key elements, namely the letters Petersen wrote to Sylvester. Thus, what Petersen had to say about his progress can for the most part only be guessed from the reactions of his correspondents.

The main feature which emerges from this correspondence is that Petersen’s paper came close to being published jointly with J.J. Sylvester. That Sylvester had also been working on the same problem and that there had been an exchange of ideas between them which, if nothing else, provided the psychological momentum for carrying on with the work, is clearly stated by Petersen in his Introduction:

\[\ldots\text{[Sylvester], der gleichzeitig mit mir die Frage nach den Grundfaktoren in Angriff genommen hat, und mit dem ich vielfach darüber verkehrt habe. Obgleich wir die Beantwortung der Frage auf ganz verschiedenen Wege gesucht haben, habe ich doch seinen Mitteilungen eine Erregung zu verdanken,\textsuperscript{56} ohne die ich vielleicht längst von den grossen Schwierigkeiten, die sich für jeden Schritt darbieten, ermüdet worden wäre.}\]

(Petersen 1891a, p. 194)

As apparently all of Sylvester’s letters to Petersen have survived, it is possible to verify that Petersen’s carefully worded statement—which does not really credit Sylvester with any concrete mathematical contribution—is correct: without

\textsuperscript{56}In (Biggs et al. [3, p. 190]) this part of the sentence is incorrectly translated as ‘I am nevertheless grateful for his encouragement’. Petersen speaks neither of gratitude nor of encouragement but of stimulation or excitement.
Sylvester, the paper would never have been written, but as it stands, its graph theoretical content originates entirely with Petersen (with the exception of one important feature, explicitly attributed to Sylvester, and confirmed by the letters, viz. the discovery of the existence of indecomposable regular graphs of arbitrary odd degree).57

The collaboration between Petersen and Sylvester, if such it can be called, started in October 1889 and lasted till January 1890, altogether a little more than three months. It was a race, and both mathematicians invested their full efforts in it. It culminated in a visit which Petersen paid to Sylvester at the very end of the year, spending about two weeks with him in Oxford and London, working on the problem.58 It was Petersen’s first visit to England. What importance he attached to his association with Sylvester can be seen from the fact that he was in Oxford on New Year’s Eve—indeed, the eve of a new decade—rather than with his family in Copenhagen.

The two collaborators faced each other from very different positions. Petersen must have had a profound respect and great admiration for Sylvester. Undoubtedly he wanted their joint (or rather: simultaneous) efforts to find their expression in a joint publication, although he left Sylvester a free hand in this matter. Sylvester’s attitude towards Petersen was erratic, ranging from nearly insulting, but he recognizes him as ‘a very able man indeed’.59 As a correspondent, Sylvester shows himself a master of the Baroque art of announcing results (true or false), giving no proofs, only hinting vaguely at the methods. In return, Petersen sent proofs.60 Sylvester, in spite of many attempts, proved nothing at all.61 Yet his role must not be underestimated: the numerous

57 For graph theoretical terminology see (Bondy and Murty [5]). With due apologies to graph theoretical readers, here are some basic definitions: A graph G is an incidence structure consisting of vertices and edges, each edge being incident with exactly two vertices (there may be several edges joining the same pair of vertices). The order of G is the number of vertices; the degree of a vertex is the number of its incident edges. G is regular if all vertices have the same degree. A k-factor of G is a regular subgraph of degree k having the same vertices as G. A factorization of a regular graph is a decomposition of G into pairwise edge-disjoint factors; if all factors of the decomposition are of degree k one speaks of a k-factorization. G is indecomposable if it does not have a decomposition into two or more factors. A trail [path] in G is an alternating sequence of incident vertices and edges [without repetition of terms]. G is connected if any two vertices can be joined by a trail. A bridge is an edge whose removal increases the number of connected components of G.

58 “Petersen has been passing a week with me in Oxford and is now in London with me”. [Sylvester to Klein, Jan. 4, 1889 [recte 1890]].

59 Sylvester to Klein, Jan. 19, 1890.

60 Two of Petersen’s proofs are mentioned by Sylvester in sufficient detail to be clearly recognizable: (a) the proof of the factorization of 4-regular graphs by alternating edges in an eulerian trail [Sylvester to Petersen, Nov. 24, 25, 1889, and Dec. 8, 12, 1889]; (b) the proof of what Petersen calls the “theorem of ablation”, i.e., the 2-factor theorem for regular graphs of even degree [Sylvester to Petersen, Nov. 16, 24, 1889, and Dec. 6, 1889].

61 Eloquent evidence for this can be found throughout Sylvester’s letters to Petersen. There is also Petersen’s own very blunt statement: “Sylvester meinte zwanzig Mal, dass er einen Beweis für den geraden Fall gefunden hätte, aber jedesmal war der Beweis unrichtig. . . . Nun schrieb er, dass er sicher ist, dass er nicht allein den geraden sondern auch den weit schwierigeren ungeraden Fall erledigt hat. . . . Nun ist aber die Sache, dass ich zufolge meiner Erfahrung an der Richtigkeit seines Beweises zweifle . . .”. [Petersen to Klein, Jan. 26, 1890].
conjectures which he produced, provided the stimulation which Petersen speaks of in the Introduction to his paper.

At first, Petersen and Sylvester made rapid progress, so rapid, in fact, that already by mid-October Sylvester felt justified in writing to Klein about the possibility of submitting a paper to *Mathematische Annalen*.\textsuperscript{62} Klein was interested. It was however only in December that Sylvester sent him an outline of his and Petersen's work for publication as 'Letter to the Editor', announcing at the same time that a joint paper was to follow.\textsuperscript{63} This was premature optimism. During Petersen's visit to Oxford the two 'collaborators' realized that they had been working along diverging paths, and that for a joint paper there simply was not enough common ground. They decided instead to submit two separate ones—Sylvester first, then Petersen—and informed Klein of their changed intentions.\textsuperscript{64}

Petersen had misgivings about the new plan. It had become clear to him that Sylvester was not in good physical and mental condition (he had serious troubles with his eyes, causing him great anxiety), to the extent that his mathematical abilities might not be at their usual level. After his return to Copenhagen, Petersen wrote to Klein about his concerns, and asked to be given Sylvester's paper to referee should it ever be submitted.\textsuperscript{65} He was spared the painful task. By the end of January, Sylvester had quit the race, never to take up graph theory or invariant theory again. When Petersen sent him some further material the following May, he did not even feel up to the effort of trying to understand it.\textsuperscript{66}

Once Petersen was on his own, he seems to have lost some of his speed. In a letter to Hilbert he speaks of 'months of fruitless efforts'.\textsuperscript{67} Still, by the middle of February 1890 he had enough material to give a talk on 'The decomposability of a graph' in the Royal Danish Academy of Sciences.\textsuperscript{68} What he sent Sylvester in May probably amounted to a first draft of the complete paper, but it took another four months to get it into publishable shape. With Sylvester out of the running, the project of publication in the *Annalen* was dead, and Petersen submitted the paper in early October to *Acta Mathematica* of whose editorial board he was a member.\textsuperscript{69} This probably ensured acceptance without refereeing. Petersen also seems to have counted on rapid publication, but in this he was deceived. It took a year for the paper to appear.

Petersen's paper must be understood within the framework of some of the major developments that were occurring in invariant theory at the time. Central

\textsuperscript{62} Sylvester to Klein, Oct. 15, 1889.
\textsuperscript{63} Sylvester to Klein, Dec. 12, 1889, with two addenda.
\textsuperscript{64} Sylvester to Klein, Jan. 4, 1889 [recte 1890].
\textsuperscript{65} Petersen to Klein, Jan. 26, 1890.
\textsuperscript{66} Sylvester to Petersen, June 15, 1890.
\textsuperscript{67} Petersen to Hilbert, Apr. 11, 1891.
\textsuperscript{68} On February 21, 1890. He gave a second talk in the Academy on November 28, 1890, on 'The decomposability of graphs of odd order'.
\textsuperscript{69} Petersen to Mittag-Leffler, Oct. 4, 1890. Petersen was a member of the 'Comité de rédaction' of *Acta Mathematica* from its inception in 1882 until his death.
to the theory throughout the better part of its history, was the so-called Finite Basis Problem, first considered by Cayley in the 1850's. The invariants of a given form or a given finite set of forms (usually called groundforms) generate a ring and the question was whether this ring has a 'finite basis', i.e., is finitely generated. This was provided in 1868 by Gordan for the case of binary groundforms. Gordan's proof was constructive, providing—at least in principle—an algorithm for explicitly calculating a basis. On the other hand, it was quite involved and shed little light on why it worked, causing a number of mathematicians to look for more transparent arguments. Petersen may have been among them: his two early papers on invariant theory [Petersen 1880a, 1881a; actually a single paper in two parts] are aimed in the direction of a Finite Basis Theorem in a more general setting than Gordan's, dealing with semi-invariants of binary forms rather than invariants. However, this work has remained a fragment; at least one further paper was to follow but never did.

For twenty years the constructivist approach espoused by Gordan dominated all attempts to extend the Finite Basis Theorem to groundforms in an arbitrary number of variables. As is well known, Hilbert put an end to these attempts by giving a purely existential proof in a series of papers beginning in 1888. Like everybody else who had worked on the Finite Basis Theorem, Hilbert began by considering invariants of binary forms. Early in 1889, he published a short and very elegant proof of this case of the theorem in the Göttinger Nachrichten; a slightly modified version appeared later that year in Mathematische Annalen under the title Über die Endlichkeit des Invariantensystems für binäre Grundformen (Hilbert [26]). This paper is the point of departure for Petersen's graph theoretical work.

Hilbert's proof relies on a theorem of Gordan from 1885 to the effect that the

A binary form is a complex homogeneous polynomial in \(2 + (n + 1)\) variables, \(x = \begin{pmatrix} x \\ y \end{pmatrix}\) and 
\[ F(a, x) = a_0 x^n + a_1 x^{n-1} y + \cdots + a_{n-1} x y^{n-1} + a_n y^n, \] 

i.e., \(F\) is a homogeneous polynomial in the variables \(x, y\) in the usual sense, with the coefficients also being considered as variables. The degree of \(F\) in \(x, y\) is referred to as the order of the form. A (binary) invariant is a homogeneous polynomial in the coefficients, \(P(a_0, \ldots, a_n) \in \mathbb{Z}[a_0, \ldots, a_n]\), which is invariant under the action of the special (complex) linear group \(SL(2)\) on \(\mathbb{C}^{n+1}\) induced by its action on the variables \(x, y\). That is, given \(A \in SL(2)\) let \(a' = (a_0', \ldots, a_n')\) be defined by

\[ F(a', x) = F(a, Ax). \]

Then \(P\) is an invariant if \(P(a') = P(a)\) for any \(A \in SL(2)\). If \(P\) is invariant under the induced action of one of the two subgroups of \(SL(2)\) fixing \(x\) or \(y\), then \(P\) is a semi-invariant. A covariant is a polynomial \(Q(a, x)\), homogeneous in \(a\) and \(x\), and satisfying \(Q(a', Ax) = Q(a, x)\) for any \(A \in SL(2)\). Invariants and covariants of \(m\)-ary forms, \(m \geq 3\), are defined similarly, by starting with the general expression for a homogeneous polynomial in \(m\) variables instead of (1).

To judge by their content, Petersen does not explain the purpose of the papers. It is interesting to note that not quite two years earlier, Sylvester also had worked on a proof of the Finite Basis Theorem by means of semi-invariants. "I intend to make differentiants [i.e., semi-invariants] the basis of my memoir—the protoplasm as it were of the whole theory" [Sylvester to Cayley, Dec. 23, 1877; St. John's College, Cambridge].
cone (over the integers) of positive solutions of any system of homogeneous linear diophantine equations is generated by the indecomposable solutions (i.e., those which are not sums of other solutions), and that these are finite in number (Gordan [22, p. 1991]). Hilbert showed that given the binary groundform of order $n$, a finite basis for the ring of its invariants can be obtained from the indecomposable solutions of a system of diophantine equations in $n^2$ unknowns $e_{ij}$ of the following type:

$$
\sum_{k=1}^{n} e_{ik} = \sum_{k=1}^{n} e_{kj}^\prime \quad i, j = 1, \ldots, n. \quad (2)
$$

The system (2) simply is the symmetrized condition for a polynomial of the form

$$
P(x_1, \ldots, x_n) = \prod_{1 \leq i < j \leq n} (x_i - x_j)^{e_{ij}^\prime}
$$

(3)

to have the same degree in each of its variables. The importance of these polynomials lies in the fact that if they satisfy (2), then their symmetrization (or permutation sum)

$$
P^\ast(x_1, \ldots, x_n) := \sum_{\sigma \in S_n} P(x_{\sigma(1)}, \ldots, x_{\sigma(n)}),
$$

(4)

when expressed in terms of the elementary symmetric functions, is an invariant. Conversely, any invariant is the symmetrization of some integral linear combination of such polynomials.

Contrary to his proof of the general Finite Basis Theorem, Hilbert's proof of the binary case is constructive. However, the finiteness of the number of indecomposable solutions of (2) suffices to conclude the existence of a finite generating set for the invariants. This is how far Hilbert went. If one wants the generators explicitly, the indecomposable solutions will also have to be known explicitly. The problem which Petersen attacked, was to determine these solutions. He did not fully succeed, but that matters little. What does is that he recognized the purely combinatorial nature of the problem, and that he made it the nucleus of a whole new mathematical theory. In both respects he owes a considerable debt to Sylvester.

As he was not in the habit of keeping abreast of the literature, Petersen did not know about Hilbert's paper when it appeared. He must, however, have been aware in a general way that invariant theory was in the process of being revolutionized. It was Sylvester who drew Petersen's attention more closely to these developments when, at the middle of September 1889, he paid a short visit to Copenhagen on his way back from Sweden, where he had gone for treatment of his persistent eye troubles. During a walk in the Tivoli gardens Sylvester told

72 Sylvester to Klein, Oct. 15, 1889.
73 Sylvester to Petersen, Oct. 3, 1889.
Petersen about yet another recent proof of the Finite Basis Theorem for the binary case, this one by Cayley, which avoided diophantine equations altogether. What neither Sylvester nor Petersen knew, was that Cayley had submitted this proof to *Mathematische Annalen*, and that it had been found seriously in error by its referees, Hilbert and Gordan. Efforts by Klein, and also by Hilbert, to convince Cayley of his mistake had proved unsuccessful, and Klein had been obliged to publish the paper (Cayley[10]). It appeared in the summer of 1889.⁷⁴

From his conversation with Sylvester, Petersen realized that Cayley’s proof was wrong. Sylvester had some difficulty accepting this extraordinary fact (he himself had found no fault with Cayley’s argument), and left Copenhagen with some doubts about Petersen’s objections. That Cayley was wrong should not have come as such a surprise to Sylvester. It had happened once before, in 1856, also in connection with the Finite Basis Theorem, when Cayley claimed that the theorem was true for invariants of binary forms of order \( \leq 6 \), and false thereafter (Cayley [9]). The proof by Gordan that the theorem was true for all orders created a sensation and established Gordan’s leading position in invariant theory.

By the time Sylvester was back in Oxford he was convinced that Petersen was right, and turned to Hilbert’s paper to see whether it contained the same mistake.⁷⁵ Naturally his attention centered on the diophantine equations, Hilbert’s key tool which Cayley had thought unnecessary. Independently of him, Petersen did the same thing. Both were struck by the possibility of a new method for determining the generators of the invariants, and set themselves the task of solving the equations. Quickly they communicated to each other their first results, thus starting the collaboration which we have outlined above.

Checking through the most recent issues of the *Annalen*—the main journal for invariant theory—in search of Hilbert’s paper (for which Sylvester had been unable to give him a precise reference), Petersen came upon Cayley’s erroneous note. He immediately wrote a letter to Klein, rather sketchily outlining where Cayley had gone wrong.⁷⁶ The letter is dated October 20, 1889, but there are reasons for believing that Petersen was in such a hurry that he misdated it, and that it was actually written on September 20. Klein replied on September 23, explaining some of the background of Cayley’s paper, and invited Petersen to write a formal rejoinder for the *Annalen*, taking into account the earlier work by Hilbert.⁷⁷ Petersen set to work at once, and before September was out, sent Klein the note *Über die Endlichkeit des Formensystems einer binären Grundform* [Petersen 1890a]. Although the note must also have been written in a very short time,⁷⁸ its style of exposition is far superior to that of the letter. However, in an attempt to go one better not only on Cayley but also on Hilbert (by extending his

⁷⁴ Klein and Hilbert exchanged several letters dealing with Cayley’s paper. See Frei [19], letters 40–42.
⁷⁵ Sylvester to Petersen, Sep. 23, 1889, and Oct. 1, 1889.
⁷⁶ Petersen to Klein, Oct. [?]20, 1889.
⁷⁷ Klein to Petersen, Sep. 23, 1889.
⁷⁸ The note bears the remark ‘September 1889’ but no precise date is given.
proof from invariants to semi-invariants), Petersen overlooked a well-known elementary detail, suggesting instead a cumbersome modification of Hilbert's argument. This oversight (semi-invariants are in one-one correspondence with covariants, and the latter may be considered as invariants of an enlarged set of groundforms) was pointed out to him some weeks later by Sylvester, to whom he apparently had sent a copy of the note.\textsuperscript{79} Petersen was by no means ignorant of the relationship between semi-invariants and covariants; on the contrary, it is one of the key points in [Petersen 1880a]. One can only suppose that it had slipped his mind in the (quite unnecessary) hurry with which he had composed the note for Klein. Hilbert also takes Petersen to task for having missed the obvious, first in a letter written in April 1891, and then again in his joint review of (Cayley [10]) and [Petersen 1890a] in *Jahrbuch über die Fortschritte der Mathematik.*\textsuperscript{80}

What traces did Sylvester's involvement leave in *Die Theorie der regulären graphs* beyond creating the climate of competition which Petersen guardedly refers to in his Introduction? In the absence of Petersen's letters it is difficult to make a definite judgement. On the other hand, there is much information in the letters written by Sylvester which allows an answer to the question in some respects, leaving it open in others.

Petersen's paper consists of four major parts:

(i) The transformation of the original algebraic problem into a graph theoretical one (by associating with any polynomial of the form (3) a (multi)graph on the vertices $x_1, \ldots, x_n$, with $e_{ij}$ edges between $x_i$ and $x_j$; and conversely). Under this correspondence, a regular graph of degree $d$ corresponds to a solution of Hilbert's equations (2) for which $\sum_{j=1}^{n} e_{ij} = d$ for all $i = 1, \ldots, n$, and the indecomposable solutions of (2) correspond to indecomposable graphs in the sense of factorization.

(ii) The problem of factorizing regular graphs of even degree. Here Petersen proves his first major result, viz. that any such graph has a 2-factorization (the so-called 2-factor theorem).

(iii) Criteria for the existence of edge-separating factorizations of 4-regular graphs.

(iv) The factorization of regular graphs of odd degree, in particular, the theorem that any bridgeless 3-regular graph can be decomposed into a 1-factor and a 2-factor (Petersen's theorem).

The order of presentation of these four main topics coincides with the chronological order in which they were attacked and solved. Also worth mentioning is a purely technical but fundamental device on which Petersen relies throughout his paper, and which has been used by many later authors:

(v) the concept of alternating paths.

\textsuperscript{79} Sylvester to Petersen, Oct. 11, 1889.

\textsuperscript{80} *Jahrb. Fortschr. Math.* 21 (1889), 104. Hilbert to Petersen, Apr. 25, 1891.
One thing which can be clearly seen from the sketches which Sylvester gives of his methods is that there is no overlap whatsoever in the proof techniques employed by him and by Petersen. He stresses this himself in a letter to Klein:

Our lines of investigation are absolutely different.

(Letter: Sylvester to Klein, January 19, 1890)

For (ii) (factorization of graphs of even degree) this means concretely: the idea of first proving the 2-factor theorem for 4-regular graphs by taking alternate edges in an eularian trail (i.e., a closed trail passing through every edge of the graph exactly once), and then reducing the general case to the 4-regular one by local arguments, is entirely Petersen's. Sylvester all along had his mind set on induction on the number of vertices, and tried various stronger induction hypotheses instead of simple 2-factorability.

The splitting of an eularian trail of a 4-regular graph into two 2-factors is perhaps the most elegant application (besides being the first one) of the general technique of ‘alternating paths’. Petersen’s fundamental idea was to ‘colour’ the edges of a trail or a path alternatingly red and blue, and then to use the edges of one or both colours for the construction of other paths or trails. This idea not only dominates his entire paper but recurs time and again in the later development of factorization theory and its successor, matching theory. The importance of the concept was not lost on Sylvester, although at first he seemed reluctant to come to grips with it (he never used it himself), and his respect for Petersen went up accordingly:

My admiration for the ingenuity and simplicity of this process is unbounded.

(Letter: Sylvester to Petersen, December 12, 1889)

With respect to the statement of the 2-factor theorem (as opposed to its proof) the situation is less clear. Sylvester sent Petersen no fewer than eight conjectural factorization theorems for graphs of even degree, some true, some false, and unless Petersen ignored them all, they may well have helped him in arriving at a correct formulation. In all likelihood he did so before Sylvester who, for his part, moved on to incorrect versions even after Petersen had proved the theorem.

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81 For a more detailed account of this and the topics mentioned under (iii) and (iv) in their graph theoretical and historical context, see Mulder [49].

82 “I have never deviated from my original idea of an Inductive Proof.” (Sylvester to Petersen, Dec. 12, 1889).

83 For example: “...instead of taking for the ground of the induction the supposition that the n point graph (of frequency 2r) can be arranged in r cycles (simpliciter) I add that they are such as that any r bonds can be found singly in each of them.” (Sylvester to Petersen, Nov. 17, 1889).

84 Petersen had found his proof of the 2-factor theorem before November 15 (it is mentioned in Sylvester’s letter to Petersen of November 16). Sylvester refused to read it for several weeks because Petersen had drawn a corollary from his method which is manifestly false (every 2k-regular graph can be decomposed into two k-factors).
As to (iii), the general problem of edge-separating factorizations may be stated as follows: Given a pair of edges \( e, e' \) of a regular graph \( G \), when does there exist a factorization with \( e \) and \( e' \) in different factors? Petersen deals with the special case where \( G \) is 4-regular, devoting almost one-fourth of his paper to this problem. It stands out in two ways: there is no visible invariant theoretical motivation, and the methods employed for its solution have a strong geometrical flavour (Mulder [49]). Its lack of immediate applicability makes it the first instance of a graph theoretical problem being raised and investigated for its own sake. However, separation properties are of little concern in graph theory, and Petersen has remained virtually alone in his attempt to deal with such a problem. Given its relative insignificance, and finding it hard to tackle, why did Petersen bother with it at all? The answer lies in his race with Sylvester. As already mentioned, Sylvester’s plan was to find an inductive proof for the factorability of regular graphs of even degree. Not unreasonably, the first and simplest (but false) induction hypothesis which he tried was that in a 4-regular graph of order \( n \) any two edges can be separated by a 2-factorization. All his subsequent attempts use refinements of this basic hypothesis, claiming separation for various kinds of edges. Petersen was aware that Sylvester’s ‘proofs’ consisted in good part of wishful thinking. Perhaps he also got annoyed at some of the language to which Sylvester treated him—especially in connection with the factorization of graphs of even degree—and decided to set the record straight and make it clear to Sylvester that his separation hypotheses were too simplistic to be of use as the basis of an inductive proof.

For (iv) (factorization of regular graphs of odd degree) we do not have to speculate. It is in the proof of the factorization theorem for bridgeless 3-regular graphs that we find the truly sophisticated applications of alternating paths. This method of proof—and the theorem itself—are so much Petersen’s that Sylvester admits (albeit at a time when he was not feeling well) “…it would be vain for me that I should endeavour to follow your ingenious but complicated theories of blue and red lines”. Sylvester’s principal contribution to this topic—which seems to have caught Petersen by surprise—is the realization that there are indecomposable regular graphs of arbitrary odd degree, and he conjectured correctly and most likely before Petersen (although this is not acknowledged in the paper) that the degree of an indecomposable regular graph of order \( n \) is at most \((n - 1)/3\).

This leaves the fundamental step (i), the transformation of the problem from algebra to graphs. At the time Sylvester and Petersen began their work on

85 [Petersen 1899a, p. 38].
86 Sylvester to Petersen, Nov. 16, 1889.
87 Particularly in his letter of December 6, 1889.
88 Sylvester to Petersen, June 15, 1890.
89 Sylvester first mentioned the bound \((n - 1)/3\) in his letter to Petersen of November 16, 1889, but gives slightly different bounds later on. In his paper, Petersen established the weaker bound \(n/3 + 1\) [Petersen 1891a, p. 208].
Hilbert’s equations, the idea of representing a (multi)graph $G$ with vertices $x_1, \ldots, x_n$ and adjacency matrix $(e_{ij})$ by the polynomial

$$P_G(x_1, \ldots, x_n) := \prod_{1 \leq i < j \leq n} (x_i - x_j)^{e_{ij}}$$

was by no means new. It goes back to a paper by Sylvester on invariants and ‘chemical graphs’, written early in 1878, in which he shows how invariants and covariants of binary forms can be coded by the highly compact device of graphs (Sylvester [68]). Sylvester’s impenetrable style, his repeated and sometimes questionable insistence on the significance of invariants for chemistry, and the impression which he creates that mathematically—as distinct from chemically—graphs are no more than a pictorial device, combined to ensure that the paper has had next to no influence on invariant theory, in spite of the interesting ideas it contains. One of these is the correspondence $G \mapsto P_G$, although it must be added that Sylvester does not introduce it in any formal way and only uses it in the case where $G$ is regular, being interested not so much in the polynomial $P_G$ itself as in the invariant expressed by its symmetrization $P^*_G$. It is only in Petersen’s paper that the correspondence is spelled out explicitly and with no restrictions on the graphs.

We have no clear-cut evidence to show whether it was Sylvester or Petersen who first saw the possibility of applying this correspondence to Hilbert’s equations. What evidence there is, weighs in favour of Sylvester or of an independent and almost simultaneous discovery by both. Petersen’s Introduction is silent on this point, giving the impression that the idea originated with him. Sylvester’s letters would suggest otherwise. Writing to Klein in December 1889 he states: “Petersen has been working at the same subject in the direction and on the lines marked out by me”. Unfortunately this is not altogether unambiguous. In the context in which it is said, it could refer to the graph theoretical programme as a whole, but it could equally well mean that in Sylvester’s view, Petersen was doing no more than trailing behind, proving Sylvester’s conjectures. Much less ambiguous is a letter to Petersen—written the same day as the letter to Klein—in which Sylvester sets down an account of his contributions to the whole question of factorization. He particularly stresses that even previous to hearing from Petersen (i.e., at the beginning of October 1889) he had realized that quadratic expressions of the form $(x_i - x_j)^2$ represent cycles as do cyclic products of three or more factors, such as $(x_i - x_j)(x_j - x_k)(x_k - x_i)$. This leaves no doubt that Sylvester was using the correspondence between polynomials and graphs from the very beginning. What the general framework of his ideas was, he makes unmistakably clear in another letter by referring to one of his results as the “most
important theorem discovered hitherto in the science of Chemical Graphology”.

In spite of all this, the argument in favour of Sylvester as the sole discoverer of the applicability of graphs to Hilbert’s equations is not conclusive. In submitting his paper to Mittag-Leffler (the editor of Acta Mathematica), Petersen speaks of possible applications of graphs:

Most interesting would perhaps be applications in chemistry, a compound molecule is a graph, and the question of its resolvability will thus naturally present itself.

(Letter: Petersen to Mittag-Leffler, October 4, 1890, transl. from Danish)

This is very much in the spirit of Sylvester’s paper of 1878. However, in his letters to Petersen, Sylvester never mentions his theory about invariants and chemistry (except for the allusion to the ‘most important theorem in Chemical Graphology’) nor, more specifically, does he mention the correspondence between polynomials and graphs. It is probably safe to assume that Sylvester who was very fond of his theory, discussed it with Petersen during his visit to Copenhagen. The very fact that he takes it for granted that Petersen knows what Chemical Graphology is all about, points in that direction. Thus chemical graphs would still have been fresh in Petersen’s mind after Sylvester’s departure, making it not altogether unlikely that, without any further prompting from Sylvester, he recognized in them the ideal tool for the solution of Hilbert’s equations. The immediate trigger of the idea could have been the ‘variable-free’ notation which Hilbert employs in setting up his equations, writing \( \prod_{\leq j} (i, j)^a \) for the polynomials of the form (5). Needless to say, this notation is highly suggestive of some underlying combinatorial or geometrical structure, being only one step removed from the modern representation of a graph as a set (or multiset) of ordered pairs of vertices.

In April 1891, after the second issue of vol. 14 of Acta Mathematica had appeared, and Petersen had been disappointed and mildly annoyed to notice that his paper had not been included, he decided to write to Hilbert and give him an outline of his investigations. By then, Hilbert was not just the author of the

93 Sylvester to Petersen, Oct. 18, 1889. Sylvester continues: ‘‘...it seems to me that [the lemma] ought to suggest new ideas in Theoretical Chemistry and that it may possibly lead to a new conception or improve the existing conception of the nature of Valence.” The lemma which was to have this remarkable effect reads: Every graph of minimum degree \( \geq 2 \) has a 2-factor. As Sylvester soon noticed, it is false.

94 In December 1877 Sylvester had been asked to give a talk about his work to the Johns Hopkins Scientific Association. Making invariants intelligible to nonmathematicians was a challenge which “I have overcome in a remarkable manner through the medium of the chemical notion of valence.” [Sylvester to Cayley, Dec., 23, 1877; St. John’s College, Cambridge]. See also the introduction to Sylvester [68].

95 Hilbert [26, p. 224]. To use variable-free notation had been fairly common practice since at least the 1860’s, and it is surprising that its combinatorial nature had gone unnoticed for so long. Although quite different in substance, it may have its origin in the very similar notation which Cayley introduced for his ‘hyperdeterminants’ in 1845.

96 Petersen to Hilbert, Apr. 17, 1891. That Petersen had expected his paper to appear quickly is clear from the opening sentences of the letter.
paper which had been the starting point of Petersen's own, but he had become a key figure in invariant theory. What Petersen did not know was that Hilbert was aware of his collaboration with Sylvester. We have already mentioned that in December 1889, Sylvester had sent Klein a ‘Letter to the Editor’ for the *Annalen*, containing a sketch of the work on graph factorization. This note was so disorganized that Klein asked Hilbert for help.\(^97\) Whether Hilbert was able to make any real sense of it, is doubtful, and not surprisingly his reaction was strongly unfavorable. Replying to Klein, he suggests that Sylvester might profit from Petersen's help in organizing his material, and then goes on to say:

> Was übrigens die Wertschätzung der Sylvesterschen Untersuchungen anbetrifft, so betont zwar Sylvester mit Nachdruck die Wichtigkeit derselben; doch kann ich mich aus dem Briefe nicht davon überzeugen, dass seine Resultate wirklich tief liegen und auch Solchen gefallen, welche sich mit der Freude an rein formalen Entwickelungen nicht begnugen.

*(Letter: Hilbert to Klein, December 29, 1889)*

In spite of the outrageous incoherence of Sylvester's note this is a rather severe judgement, and the remark about 'purely formal developments' completely misses the mark. It is possible that Hilbert saw in graphs no more than a manifestation of the so-called 'symbolical' theory of invariants,\(^98\) which indeed consisted almost entirely of formal developments, and for which he did not have a particularly high esteem (Gordan was one of its leading proponents). Reading Petersen's letter more than a year after this incident, Hilbert may have found reasons for revising his judgement. In his reply he makes no specific comments whatsoever on Petersen's results, but it is clear that he was conscious of the absence of any connection between graphs as considered by Petersen, and the symbolical representation of invariants. Indeed he makes the important suggestion that the graph theoretical approach might be used to obtain a good upper bound for the number of generators for the invariants of a given set of groundforms as a function of the degrees of the forms.\(^99\) Hilbert was under considerable pressure to pay more attention to the constructive aspects of his methods. A significantly improved bound for the number of generators (bad ones

\(^{97}\) Klein to Hilbert, Dec. 25, 1889; (Frei [19]) letter 51.

\(^{98}\) In his negative assessment of graphs Hilbert may have been influenced by M. Noether's review of Sylvester [68] (*Jahrb. Fortschr. Math.* **10** (1878), 90–92) which, albeit unintentionally, gives a one-sided picture of Sylvester's paper. Sylvester actually speaks of two correspondences between invariants and graphs, based, respectively, on the *symbolical* and *actual* resolution of an invariant. The second one is what we have termed the Sylvester/Petersen correspondence. In Sylvester's paper it plays a minor role, the place of prominence being given to the 'symbolical' correspondence, and it is only on the latter that Noether reports in his review. The impression that 'chemical graphs' are nothing but an exercise in symbolical invariant theory (apart from their connections with chemistry) is almost inevitable.

\(^{99}\) Hilbert to Petersen, Apr. 25, 1891.
were known) based on his diophantine equations, would have been a most welcome vindication of his approach.

Petersen did not take up Hilbert’s suggestion. Had he done so, graph theory might have become a respectable mathematical discipline 50 years before it actually did. However, to follow through on Hilbert’s idea would have been a difficult undertaking, requiring much more information about indecomposable graphs than was available to Petersen without a substantial amount of additional work. Also, in his heart of hearts, he may have had doubts about the accuracy of the statement he makes at the end of his paper, and which he repeats to Hilbert, that his methods would work equally well for graphs of odd degree greater than three.

Even before writing to Hilbert (probably even before submitting the paper), Petersen had considered a question which is much more ambitious than the determination of upper bounds. Is it possible, he asks, to construct a generating set for the binary invariants by using only regular graphs of degree 1 or 2 (the indecomposable factors of regular graphs of even degree)? The motivation for this question is not hard to see. It is, in fact, an outgrowth of Sylvester’s discovery of indecomposable graphs of arbitrary odd degree. Petersen’s 2-factor theorem had shown that it is possible to give a complete solution of Hilbert’s equations for 12 odd (in which case the degree is even). Nothing in the form of the equations gives any indication that there might be a fundamental difference in the nature of their indecomposable solutions depending on the parity of \(n\), but this is precisely what the existence of indecomposable graphs of odd degree \(\geq 3\) implies. There is no general procedure for determining these graphs, and consequently no general solution for Hilbert’s equations when \(n\) is even. It follows that Petersen’s and Sylvester’s programme of effective calculation of a generating set for the binary invariants via Hilbert’s equations cannot really be carried out. By proposing the new problem, Petersen implicitly admits as much. He gives no reasons why his new idea might work, but he mentions it in his letter to Hilbert, adding that he expects the difficulties to be great. Hilbert preferred to talk about Hilbertian mathematics and does not comment. We do not know whether Petersen made any progress beyond the statement of the problem.

In this connection Petersen produced another graph theoretical ‘first’: automorphisms. Recall that the Sylvester/Petersen correspondence associates to a (multi)graph \(G\) the polynomial \(P_G(x_1, \ldots, x_n)\) given by (5). As pointed out earlier, the symmetrization \(P_G^*\) is a binary invariant if and only if \(G\) is regular. It can happen, however, that \(P_G^*\) is identically zero. A graph for which this occurs is irrelevant for the purposes of invariant theory. This had already been noticed by Sylvester in his paper on ‘chemical graphs’ (Sylvester [68]). He remarks that there should be an intrinsic graph theoretical criterion for characterizing such graphs but that he has been unable to make out what it is.\(^\text{100}\) Petersen also attaches considerable importance to this problem:

\(^{100}\)Sylvester [68, pp. 77, 83, 89].
Going a step beyond Sylvester he proposes the following elegant criterion. For any automorphism $\alpha$ of $G$ either $P_G(\alpha(x_1), \ldots, \alpha(x_n)) = P_G(x_1, \ldots, x_n)$ or $-P_G(x_1, \ldots, x_n)$. Call $\alpha$ positive or negative accordingly. Clearly either all or exactly half of the automorphisms of $G$ are positive. Petersen notes that if $G$ has a negative automorphism, then half the terms in (4) cancel out against the other half, and $P_G^a$ vanishes. In his own words:

Dieses geschieht offenbar, wenn der graph (ausgedrückt durch die Wurzeldifferenzen) durch eine gewisse Permutation der Wurzeln nur

\[ \sum (x_1 - x_2)(x_3 - x_4) = 0, \]

sein Vorzeichen ändert; so ist z.B. indem das Glied oben durch Vertauschung von $x_1$ und $x_2$ das Zeichen wechselt, so dass in $\Sigma$ die Glieder sich zu zweien aufheben.\(^{101}\) Aber ist dieser Fall der einzige? Das weiss ich noch nicht \ldots

(Letter: Petersen to Gordan, November 15, 1891)

In 1894, Petersen sent this question to the *Intermédiaire des Mathématiciens* (vol. 1 (1894), p. 24, Question 66) in an algebraic formulation which mentions neither graphs nor invariants. In fact, no motivation of any kind is given. In this form it was also included in the French edition of Petersen’s *Theory of Algebraic Equations* which appeared in 1897. Although in the book Petersen refers to his *Acta* paper, and presents the problem in an invariant-theoretical context, he gives again no indication of its true significance. The following year it was reprinted in the *Intermédiaire* together with various other problems which had remained unsolved. It was still listed as unsolved in 1926 when the *Intermédiaire* disappeared.

The answer to Question 66 turns out to be negative (Sabidussi [60]). What is more, assuming a certain familiarity with the invariant-theoretical literature, it was easily within Petersen’s reach. He may once again have been the victim of his disinclination to read other people’s mathematics. It seems more likely, however,

\(^{101}\) $\sum$ is the notation for the symmetrization operator.
that the intuitive neatness of the criterion he had in mind convinced him of its correctness.

Whether or not Petersen ever worked seriously on this problem and we have no evidence that he actually did—his conviction that automorphisms of graphs play a role in the formation of invariants made him take a quantum leap and raise the question of determining graphs with a given group of automorphisms. He had arrived at these considerations already in October 1890:

I have thought of various applications and am far enough along to see that there will be great difficulties to overcome. The formation of invariants is the most obvious, but also substitution theory calls for treatment by graphs. Those substitutions that do not change a graph form a group, and conversely one can form a graph that remains unchanged by a given group of substitutions: this can be accomplished in several ways; if one could find one not changed by the group but by all other substitutions, this would be an important step.

(Letter: Petersen to Mittag-Leffler, October 5, 1890, transl. from Danish)

As is well known, the first statement of this problem in printed form occurs in the book by Dénes König [40, p. 5], forty-five years after Petersen. However, König and Petersen are not asking exactly the same question. König’s version is abstract, requiring the graph to have an automorphism group isomorphic to the given group, whereas Petersen’s is permutational: what is given is a group of permutations which is to be equal to the automorphism group of the desired graph. There is also a difference on the level of motivation. König asked the question on purely general grounds (the automorphisms form a group, what groups can occur?). Petersen in contrast, has in mind a specific application, namely to eliminate those graphs which lie in the ‘kernel’ of the Sylvester/Petersen correspondence, i.e., do not give rise to an invariant. Petersen’s question is much the harder of the two. The abstract problem was settled in the affirmative in 1938 only two years after König’s book appeared (Frucht [20]). For permutational isomorphism the answer depends on the group, and no satisfactory general theory exists.

It is rather striking to see Petersen produce all these remarkable ideas in the wake of his paper, and then apparently not pursue them any further. The same can be said of the paper itself. There are many open ends but no sequel. The things left undone are dropped with an undertone of not being worth the investment, even though their significance (for invariants as well as graphs) must have bee clear to Petersen if nobody else. Throughout his career, one can see Petersen take up some mathematical field, write a few usually short and elegant papers, and then move on to something else. With graphs, superficially, he seems to be doing the same.

Yet, Die Theorie der regulären graphs was his supreme effort. Graphs, being objects of ‘Anschaulichkeit’ par excellence, must have held a great appeal for
him. He knew, with or without acclaim from Sylvester, that the ideas contained in his paper were well above the ordinary. He had beaten Sylvester all along in their race. His mathematical inconstancy alone seems insufficient to explain why he let go of graph theory.

During the two years that Petersen worked on his paper and waited for its appearance, he must have become increasingly aware that he was concerned with a problem whose interest to the invariant theoretical public for whom it had been intended, was rapidly on the wane. The prospects opened up by Hilbert’s spectacular existential proof of the Finite Basis Theorem drew the attention of invariant theorists away from computational aspects, even though Hilbert’s critics called for more constructivism. A paper whose avowed purpose it was to deal with a step, no matter how essential, in the explicit calculation of a basis for the binary invariants, already bore the mark of the past. Perhaps it was to counter this that Petersen decided to give the paper a title which, being nearly meaningless to his contemporaries, might attract attention. To what extent the graph theoretical method, or even the concept of a graph, was understood by those who looked at the paper, is also a question of some doubt. In this respect, Petersen had a rather disappointing experience with Gordan (to whom he had even explained his ideas in person),[102] which may well have made him wonder how lesser mathematicians might fare with his paper.

The review of the paper (Jahrb. Fortschr. Math. vol. 23 (1891), 115–117) written by F. Meyer, which appeared in 1894, although favorable, was also not particularly helpful as publicity. Meyer, one of Germany’s leading invariant theorists,[103] stressed the novelty of Petersen’s attempt to shed light on an invariant theoretical question ‘auf rein anschaulichem Wege’, but gave a somewhat forbidding image of Petersen’s methods: “The nature of the methods employed would render any discussion here useless; once again, we refer the reader to the ingenious [geistreich] paper itself”. It is doubtful that, in 1894, many people would take up this suggestion. The same holds for Question 66 in the Intermédiaire which most probably remained unsolved not so much because of its lacklustre presentation but because it was perceived as slightly stale.

Petersen briefly returned to graph theory in two notes which he wrote for the Intermédiaire des Mathématiciens in 1898 and 1899, both dealing with Tait’s theorem concerning the 1-factorability of 3-regular graphs.[104] A debate on this

[102] Gordan and Petersen met at the 1st Annual Meeting of the newly founded Deutsche Mathematiker-Vereinigung in Halle a.d. Saale, September 22–26, 1891. Following this encounter, Petersen sent Gordan a reprint of his paper, and there was a short exchange of letters.

[103] The Deutsche Mathematiker-Vereinigung confined to Meyer the task of writing the first of the mathematical surveys which were published annually in the Jahresbericht (Bericht über den gegenwärtigen Stand der Invariantentheorie, Jahresber. DMV 1 (1890–91), 79–292).

[104] The modern form of Tait’s ‘theorem’ is essentially the same as that given by Petersen [Petersen 1898c, p. 226]: Every bridgeless 3-regular graph has a 3-factorization. For a discussion of the confusion surrounding this statement as well as the (equally confused) original sources, see Biggs et al. [3, chapters 6 and 10].
subject—and on the Four Colour Problem had been started by Delannoy and Goursat in the very first issue of the *Intermédiaire* (1894), and had been going on at intervals ever since. Petersen's contribution was to straighten out some basic misconceptions about Tait's theorem. It is conceivable that he did so at the suggestion of the editors of the *Intermédiaire*, C.-A. Laisant and E. Lemoine, with whom he was personally acquainted and entertained very cordial relations.

The first note [Petersen 1898c], which appeared in October 1898, begins with an account of the theory of factorization as developed by Petersen in his paper in *Acta Mathematica*. He then goes on to show that there exist bridgeless 3-regular graphs which are not 1-factorable, giving first a brief analysis of the case where the graph contains a chordless pentagon, and then using this to arrive quickly at his famous counterexample to Tait's theorem (the Petersen graph).

Petersen's note drew an immediate response from Goursat in which he pointed out that, while ingeniously arguing the incorrectness of Tait's theorem, Petersen had not really addressed the question which lay at the heart of the whole debate: the 1-factorability of planar 3-regular graphs. This objection caused Petersen to take a close look at the Four Colour Problem (we have no evidence that he had done so earlier; there is no hint of it in [Petersen 1898c]). His reply to Goursat [Petersen 1899a] consists of a proof that Tait's theorem for planar graphs is equivalent to the Four Colour Theorem, and some remarks about the difficulty of the latter. He is rather pessimistic:

\[
\cdots \text{je ne sais rien avec certitude, seulement, s'il fallait gager, je tiendrais que le théorème des quatre couleurs n'est pas exact.}
\]

(Petersen 1899a, p. 38)

His reasons for thinking so are in part based on the experience he had gained in working on the *Acta* paper. An inductive proof of the Four Colour Theorem would, in his opinion, have to rely on a criterion for the separability of edges by a 1-factorization. Recalling that he had solved this problem for 2-factorizations of 4-regular graphs, he says:

\[
\text{J'ai trouvé les difficultés très grandes, mais elles seront, sans comparaison, plus grandes pour un graphe du troisième degré.}
\]

(Petersen 1899a, p. 38)

In January 1899, Petersen gave two lectures on the Four Colour Problem, one in the Danish Mathematical Society, the other in the Royal Danish Academy of Contributors, besides Delannoy and Goursat, were de la Vallée-Poussin, Borel, Brocard, and others.

Lemoine informed Petersen of Goursat's comments in a letter dated October 20, 1898. They were also printed in *Intermédiaire, Math. 5* (1898), 211. Goursat's formulation of the problem reads as follows: Etant donné un polyèdre convexe, dont tous les sommets sont des angles triédres, on demande d'attribuer à chacune des arêtes une des trois lettres $a$, $b$, $c$ de telle façon que les trois arêtes issues d'un même sommet du polyèdre soient toujours marquées de lettres différentes (*Intermédiaire, Math. 1* (1894), 213).
Sciences.\textsuperscript{107} In these he undoubtedly elaborated on the ideas he had just barely sketched in the note in the \textit{Intermédiaire}. We can only regret that no record of the contents of these lectures has survived.

\section*{13. Professor at Copenhagen University (1887–1909)}

In 1883, Copenhagen University appointed Zeuthen as extraordinary professor of mathematics, in addition to the ordinary professor Steen. When the latter died three years later, Zeuthen got the ordinary professorship and there was again only one mathematics professor. At the beginning of 1887, Zeuthen became Dean of the Faculty of Natural Sciences, and succeeded in creating a second ordinary professor's chair, which Petersen obtained.\textsuperscript{108}

Until 1890 Zeuthen and Petersen continued to give the elementary mathematics lectures at the Polytechnical School, but then P.C.V. Hansen, who had already replaced one or the other of them in some semesters, took over Petersen's classes. Thereafter Petersen devoted all his lectures to more advanced subjects intended for the university graduates. He usually taught four hours a week, either one four hour course or two courses of two hours. He almost exclusively lectured on six different subjects, the most frequently occurring being \textit{Function Theory}. Algebra figured under two headings in the catalogues of lectures: \textit{Group Theory} and \textit{Theory of Equations}. Moreover, he lectured on \textit{Number Theory}, \textit{Differential Equations} and \textit{Rational Mechanics}. Finally, in 1907 he gave two courses not fitting into this scheme, on \textit{Systems of Linear Forms} and \textit{Linear Equations of Second Order with Two Variables}.\textsuperscript{109}

The most striking feature of this list is the complete lack of courses in geometry. Petersen left this subject entirely to the master Zeuthen, but one may wonder if Petersen could not have accomplished more, had he allowed himself to lecture and work more in this area which seems closer to his heart and to his 'anschauliche' conception of mathematics. Still, he had a great influence on his students:

J.P. was also highly gifted as a university teacher. His lectures were both inspiring and amusing. He was not always equally well prepared. He lectured two hours in a row, and usually used the first one to warm

\footnotesize{\textsuperscript{107} On January 20, 1899, in the Society, and on January 27, 1899, in the Academy.
\textsuperscript{108} Royal confirmation of the appointment was given on April 1, 1887 [\textit{Ministerialtidende} 1887B, p. 337].
\textsuperscript{109} The courses given by Petersen are: \textit{Function Theory} (A88, S89, S + A91, A93, S94, S + A95, S + A97, A98, S99, A1901, S02, A03, S04, A05, S06, A07 (here A88 means autumn 1888 and S04 means spring 1904); \textit{Group Theory} (S88, A92, S93, S1900, S05); \textit{Theory of Equations} (A94, A95, A99, S04, A04, A06); \textit{Number Theory} (A87, A90, S93, A96, A1900, A02, A04); \textit{Differential Equations} (S90, S92, S94, S98, S1901); \textit{Rational Mechanics} (A89, A91, A94, S + A96, A99, S03, S + A06). [\textit{Aarbog f. Kjøbenhavns Universitet}, 1887–1907].}
up to his subject. He was sometimes astonished by his own idea in the textbook and stopped with a: 'I wonder what he means by that'.

(Mollerup, _Berlingske Tidende_, August 5, 1910, transl. from Danish)

In a similar vein, Zeuthen [81] stated that in his teaching, in conversations and in brief remarks, Petersen planted seeds that later bore much fruit.

After his promotion, in September 1887, Petersen moved with his family to Vesterbrogade 84, at the corner of Frederiksberg Allé. His friend, Frederik Bing, followed him there in 1895. Castor and Pollux were united as next-door neighbours on the same floor! Petersen remained at this address almost till the end of his life.¹¹⁰

In earlier years he had held mathematical evenings in his home to which all mathematicians were welcome (Crone [12, p. 8]) and after the Mathematical Society had been founded, Petersen often gave talks there (more than 25 in the period 1873–1904) on all the subjects that occupied him. Moreover he was a witty and inspiring centre during the dinners that followed the talks:

Jul. Petersen was a very stimulating element with his sometimes grotesque ideas, e.g. his plan to bring a projectile to rotate before it was placed into the gun barrel, or his claim that a piece of music would sound equally well if one started at the end and played it backwards to the beginning. Once he proved experimentally that if one gave an egg on a table a rotation, it would stand up if it was hard-boiled but it would continue to lie if it was soft-boiled.

(Crone [12, p. 77])

Similarly lively gatherings took place at Bing’s home—with Petersen in attendance—especially after both Bing and Petersen had moved to Vesterbrogade:

For many years a company of highly talented men met in his [Bing’s] flat every Monday evening · · ·. For an accidental listener such an evening could be a true delight. No subject was too low or too human for these debates, none too high: politics and morals, science and art, everything would be discussed. For a time, the physiologist Chr. Bohr [the father of Niels and Harald B.] was among the regular guests; his sharp and penetrating wit would be matched against Julius Petersen’s lively inventiveness in shaking up the ideas of the assembled company, and little respect could be felt in these tournaments for preconceived notions.

_(Illustreret Tidende, April 7, 1912)_

After being elected member of the Royal Danish Academy on April 4, 1879 (together with Thiele and such foreign celebrities as Charles Darwin and Louis

Pasteur), Petersen entertained this learned society a dozen times with his mathematical findings, but he never published any of these talks in the Academy's journal. He was a member of several judging committees of submitted papers and worked as the Academy's accountant 1888–1908. But unlike Zeuthen, who was the secretary of the Academy for more than 25 years, Petersen was otherwise not very active in this forum.

14. Inspector of the Learned Schools (1887–1900)

Among the ordinary Danes, Petersen became a well-known person, beloved by some, hated by others, for this mathematics schoolbooks. During his lifetime he produced a connected system of textbooks starting from grade 6 (children aged 14) and ending with the Gymnasium (the high-schools leading to the student-examinations). During Petersen's lifetime they almost enjoyed a monopoly in the Danish schools.

It took Petersen all his life to write his system of textbooks; the first one (on logarithms) was written in 1858 and the last one in 1906. Most of them appeared during the two periods from 1867 to 1877 and 1900 to 1906. Their characteristic small thin format reflected their content. Like his higher textbooks, they were generally very well received and for the same reasons: they were carefully thought out, exact, condensed, and were written in a clear and elegant style. In many cases reviewers could point out small inconsistencies, mistakes etc., and Petersen himself sometimes changed his approach when new editions appeared.

The only serious reproach directed against Petersen’s textbooks was their exaggerated briefness, which made them hard to understand for the students. Many reviewers and readers particularly resented Petersen’s use of the phrase ‘it is easily seen’ in the sense ‘it can be seen without principal difficulties but may require a long calculation’. Zeuthen [77] for example thought that such a phrase did not belong in a schoolbook. Petersen, however, maintained his style, with the argument that it was up to the teachers to lead the students through the text. He had great confidence in the teachers and did not want to encumber them with unnecessary details. The success of his system shows that he was right (at least then).

In 1887 Petersen was appointed member of the Commission of Education for the Learned Schools, the so-called Education Inspectorate under the Ministry of Education. The Commission consisted of three university professors, one from mathematics or the natural sciences, and two from the humanities. His new position made Petersen superintendent over all the mathematics and physics teachers of the learned schools in the whole country and the Minister’s prime

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111 This did not necessarily place Petersen in a minority. The divisions in the Commission were mostly political, opposing liberal to conservative, with Petersen and the chairman, M.C. Gertz, a classical philologist, usually following a common line (Skovgaard-Petersen [63, p. 39]).
advisor in matters concerning the teaching of these two subjects. One of the main tasks of the Commision was to set up the examination problems for the country-wide final examinations in the high-schools (studentereksamen), and to supervise the evaluation process of the results, in particular, to choose the external examiners and deal with their reports. To get some first-hand knowledge of the level of teaching, the members of the Commission themselves also served as external examiners. Many teachers considered Petersen a mild examiner and an enjoyable company during his visits at the schools, but of course his powerful position brought him also enemies, especially in the private schools, where the quality of the instruction tended to be less uniform than in the public schools. Here we shall mention one stormy event in Petersen's period as superintendent, namely the student-examination of 1897. Petersen had formulated the following computational exercise:

In a triangle $ABC$, $\angle A = 123^\circ 45' 18''$, the height from $A$ is 56,789 and the bisector of angle $A$ is 72,453. Compute the sides and angles of the triangle.

Two days after the exam the newspaper *Politiken* brought the announcement that the above exercise was 'impossible' because "when computed it turned out that in the given triangle one of the angles would be $-10^\circ$, i.e. it does not exist". The article concluded with the sarcastic criticism:

There is only one thinkable explanation of this phenomenon: The mathematician who has set the exercise has not bothered to solve it himself. He will probably do so next year.

(*Politiken*, June 16, 1897, transl. from Danish)

This created a scandal. Rectors and teachers complained; the chairman of the Inspection was called to the Minister the same day, and immediately sent Petersen an angry letter:

I must say that whoever is to blame for the error, it is an unforgivable mess, and it is a disgrace for the Inspection.

(*Letter: C. Gertz to Petersen, June 16, 1897, transl. from Danish*)

Of course, Petersen could not publicly admit his mistake and stay on the Commission, but his reply in *Politiken* on June 19 is unconvincing and smells of sophistry:

I find the problem a nice little problem, because it gives the pupils the chance to show their greater or lesser maturity by the interpretation of the meaning of the negative solution.

He argued that from grade one, the pupils had learned how to interprete negative solutions in various contexts.
Similarly with this problem: the negative solution shows that the angle \( A \) has to be interpreted as the interior [misprint for exterior] angle in order to get a positive solution.

(\textit{Politiken}, June 19, 1897, transl. from Danish)

This raised a long debate in \textit{Politiken} between the mathematics teachers. Petersen intervened twice to point out that (1) all teachers ought to have taught their pupils to make a drawing before proceeding to the calculations; (2) problems of this kind could be found in his own textbooks which all the pupils used; (3) 90\% of the pupils had been able to solve the exercise, which was a better score than in the remaining exercises.

However the Ministry was not satisfied with Petersen’s arguments. A reexamination was proposed, and on June 21 the Minister issued a decree annulling the marks for the unfortunate problem. This created a lot of confusion because some of the students had already passed the oral examination and received their final mathematics mark. Later, at the beginning of July, the matter was finally settled: only in those cases where, without doubt, the problem had had a negative influence on the answers to the set as a whole, special consideration should be exercised!

Although the whole incident had been his creation in the first place, Petersen now chose to be upset, and accused the Minister of acting behind his back. He even threatened to resign. This led to a trial of strength between Petersen and the Minister, who apparently would have liked to get rid of Petersen. Petersen kept the skin on his nose this time and remained in the inspectorate for a while longer. His credibility, however, had suffered.

In 1900, Petersen was finally overthrown. His inflexible views on a number of matters, and the overly direct manner with which he sometimes defended these views, had brought him into conflict with many people, both in the University and among the high-school teachers. One contentious issue were his ideas on the teaching of physics. He had first put these forward in 1894 when he wrote a review in defence of a textbook on electricity and magnetism by another Julius Petersen [Petersen 1894a]. He was by no means alone in this concern; in 1895 a debate on physics teaching that was to last for several years, started in \textit{Nyt Tidsskrift for Fysik og Kemi} [New Periodical for Physics and Chemistry]. For three years Petersen kept aloof from it; when he broke his silence, he angered the physicists and astonished the mathematicians by his singleminded emphasis of the mathematical side of physics teaching. In his view, the principal aim was to teach the pupils the basic mathematical laws of physics, and their logical connections, and he warned the teachers from ‘wasting time on various useless experiments’. Moreover, he launched a somewhat haughty criticism of all the physics textbooks on the market, including the mechanics book by K. Prytz, editor of the journal, and professor of physics at the Polytechnical School.
Prytz [58] immediately accused the ‘lofty’ Petersen of crippling physics by making it an appendage of mathematics, and thereby started a debate between the two professors on the basic concepts of mechanics [Petersen 1898d, 1899c, 1899d, 1901]. C. Christiansen, professor of physics at the University, joined the debate when, on November 2, 1899, he gave a talk in the Mathematical Society on some newly published physics textbooks. “Jul. Petersen made some rather sharp remarks which insulted Christiansen, so that he left the meeting” (Crone [12, p. 77]). Privately, Christiansen accused Petersen in no uncertain terms of his neglect of the physics education:

I have told you [Petersen] many times both in a friendly manner and when that did not help, ruthlessly, that you were ruining the education of physics, that your perception of physics and physics teaching was as erroneous as it could be.

(Letter: Christiansen to Petersen, March 29, 1900, transl. from Danish)

At the same time, Petersen had managed to offend the high-school teachers, especially those in the private schools. About the science teachers he made public remarks that cast doubt on their competence. 112 The teachers in the humanities, on the other hand, considered him worse than the devil for his general attitude towards classical education, and in particular for having advocated (although unsuccessfully) the abolition of Greek in the learned schools and its replacement by more mathematics and natural science. When, in September 1899, a commission was set up with the purpose of making recommendations for a new teachers’ training programme at the University, the Minister, aware of the hostility of the teachers toward Petersen, appointed Christiansen to the commission, although Petersen as member of the Education Inspectorate would have been the logical choice. The handwriting was on the wall for all to see. An editorial in the Messenger for the Society for Germanic Philology—the journal of an influential teachers’ organization—made little effort to hide its delight. Calling Christiansen a man without paedagogical talents, it went on to say that it was nevertheless

⋯a great gain that he [Christiansen] and not Julius Petersen will represent the natural sciences. Perhaps it has now become clear to the general public that a man, who without any shame displays his disdain for all humanistic education, and his own lack of it, has no right whatsoever to have his say in paedagogical matters.

(Commissionen, Budstikke til Selskabet for Germansk Filologi 2 nr. 1 (Sep. 1899), 2–3, transl. from Danish)

112 In (Kaper [34]) it is claimed that these remarks were made at a ‘big academic meeting’ on a Government project of reform of the learned schools. We have been unable to find out what meeting is being referred to.
Petersen's widespread unpopularity gave the Minister an opportunity to fire him from the Commission of Education in the spring of 1900 and appoint Christiansen in his place. Petersen, however, was convinced that he had been sacrificed as a personal revenge. Having strongly criticized Christiansen for having negotiated with the Minister behind his back, he continued in a draft of a letter:

[The Minister] has hardly mentioned the real reason, a hatred which primarily stems from the fact that I have had to give a son of his in Nykøbing a 'g-' [this is a bad mark] in physics (and a couple of rather bad marks in mathematics) (and I even did my best to help the fellow) and which [i.e. the hatred] has afterwards increased during various clashes. He has hardly explained to you that he wanted to end his career by giving me an ass's kick, and that he could not do this without help. However, I think it is impossible that you could be so unaware of this generally known situation that you did not realize what your help was needed for . . .

I can tell you that many of the men you are later going to collaborate with condemn you severely, even more severely than they condemn the minister, from whom one might expect anything, and whom they are ready to excuse for lack of intelligence. Certainly many are happy; one does not work in a responsible position for 12 years, only with one's duties in mind, without making enemies among people whose interests or vanity one offends. The headmasters of the private schools in Copenhagen will shout with joy.

(Petersen to C. Christiansen, March 29, 1900, Draft. This passage is deleted, transl. from Danish)

As one might expect, joyful shouts also came from the direction of the teachers. Here again is the Messenger for the Society for Germanic Philology:

You won't find a single person employed in a school who does not rejoice that Petersen—after so many stupidities and his attitude which lacks so much as a hint of the humanities—has been removed from his post.

(Kaper [33, transl. from Danish])

Petersen was clearly upset by being sacked, and by the attacks that followed now that he no longer could bite back, but he must have drawn consolation from letters he received from various mathematics teachers who expressed how sorry they were to hear that Petersen had been fired.

113 A particularly vicious one came from Kaper [34], one of the leaders of Privatlærer-Foreningen (Private Teachers' Association).
Fortunately we still have in the schools your good and amusing books, which even the coming regime of the physicists will not be able to displace.

(Letter: Johannes Mollerup to Petersen, July 3, 1900, transl. from Danish)

Petersen answered the physics teachers by writing his own schoolbook of ‘mechanical physics’ [Petersen 1900b]. It was received very positively by Niels Nielsen, who in 1909 succeeded Petersen as Mathematics professor at the University:

I wish one could make the physics teachers read it; they would probably benefit from reading it if they are not too hardened. · · · The state of the teaching here in our schools causes me increasing anxiety, now that we no longer have the Professor as a strong helmsman.

(Letter: Niels Nielsen to Petersen, 1900, transl. from Danish)

In 1903, Nielsen succeeded Christiansen in the Education Commission but instead of rehabilitating Petersen’s views, he now criticized them from a new angle. Nielsen was the first Danish mathematician who adhered to Weierstrass’ programme of arithmetization of mathematics. In 1905 he published a Textbook in Analytic Plane Geometry which began:

The present textbook in Analytic Plane Geometry differs from its immediate Danish predecessor [i.e. Petersen’s textbook], first by making the exposition rigorous wherever possible · · · and second by leaving out synthetic arguments, so that the developments are purely analytic. As a teacher I have always been embarrassed, when in various sections I had to point out inaccuracies, which admittedly allow a shorter and more elegant exposition.

(Nielsen [50, Preface, transl. from Danish])

In a counterattack [Petersen 1905] in Tidsskrift for Matematik, Petersen pointed out many inaccuracies and unnecessary complications in Nielsen’s book, and described its ‘terrible language’. He ended by suggesting that members of the Commission of Education should not be allowed to publish textbooks during their term of office; Petersen had not done so himself.

Petersen always preferred elegance, clarity and ‘Anschaulichkeit’ over formal rigour and this finally decided the fate of his textbooks. After Petersen’s death they were rewritten many times (by two high-school teachers, C. Hansen and later Albert Kristensen), and each time they became more formal. In 1963 a new curriculum based on ‘new math’ was introduced in the Danish high-schools. Like all existing textbooks, Petersen’s system did not meet the new standards. The publisher tried to bring the system up to date, but after two of the new books had appeared and been found unsatisfactory by the Ministry of Education, the attempt was abandoned. Some of the older teachers continued to use the earlier
editions on the sly, but by 1970 they had completely disappeared from the classrooms.

Petersen's interest for mathematics education was also known abroad. On the suggestion of Laisant he became a member of the Comité de Patronage of *l'Enseignement Mathématique*, but he never wrote any papers for the journal. On the other hand, many problems and solutions were contributed by him to the *Intermédiaire des Mathématiciens* including [Petersen 1898c], where the Petersen graph is first exhibited.

15. Function theory, latin squares and number theory (1888–1909)

In 1888, the Royal Danish Academy set the following prize problem:

Given two arbitrary power series with rational coefficients and converging in the entire plane, describe a method by which a third everywhere convergent power series can be found in a finite number of calculations, such that its zeros are the common zeros of the two given series. The calculations should be carried out for one or more examples.

As Petersen explained in a letter to Mittag-Leffler (February 14, 1888), it was he who had suggested the problem. It was motivated by his general interest in the factorization of power series, and in particular by the question: If an entire function with rational coefficients has only one zero, must the zero then be rational? He had found the answer to this question to be negative, and in another letter, written six days later, he described to Mittag-Leffler his construction of an entire function with rational coefficients which has e as its only zero. His main idea was to find a suitable entire function \( g(z) \) such that

\[
1 - \frac{z}{e} \cdot e^{g(z)}
\]

has only rational coefficients in its power series expansion. Exactly the same idea was used by Hurwitz who—apparently without knowing of Petersen's result—published its ultimate generalization three years later in *Acta Mathematica* (Hurwitz [27]). He showed that it is possible to find an entire function with rational coefficients in its expansion, which takes on a given sequence of zeros without accumulation points. Already in his letter to Mittag-Leffler, Petersen had indicated that 'of course one can easily extend the theorem', but he does not seem to have had such a grand extension in mind. Indeed, writing to Hilbert he says:

Grüssen Sie Herrn Hurwitz, dass er mir mit seiner Abhandlung in Acta zuvor gekommen ist. Vor ungefähr drei Jahren habe ich bei meinen Vorlesungen den Satz bewiesen: Man kann eine ganze transcendente Function mit rationalen Coefficienten bilden, welche nur eine und zwar

(Letter: Petersen to Hilbert, April 17, 1891)

In this way Petersen was deprived of the honour of publishing a striking theorem in function theory. However, he continued to work and teach in this field, and in 1895 he published his lectures in book form (supported by the Carlsberg Foundation; a German translation appeared in 1898). In addition to general complex function theory and a second part on specialized topics (like the gamma- and zeta-functions with applications to prime numbers, doubly periodic functions and elliptic functions and integrals), the book also contained a more topological chapter on surfaces and their classification. Already in 1888 and 1891 Petersen had sent two papers on surfaces to Göttingen, where they were presented by Schwarz to the Königliche Gesellschaft der Wissenschaften. However they were never published, perhaps because the classification of surfaces and establishing their normal forms had already been carried out by Möbius in a manuscript, submitted in 1861 to the Académie des Sciences in Paris for its Grand Prix de Mathématiques and published as part of Möbius’ Nachlass in 1886 (Möbius [47]).

The complex function theory of [Petersen 1895] is developed in Petersen’s usual geometric and ‘anschaulich’ style. For example, the proof that absolute convergence implies convergence, is given by a geometric argument, with no use of formulas, just elegant text. It uses, without explicitly mentioning completeness, that nested circles in the plane whose radii tend to zero, converge to a point. The reviewer Jensen [31] found the proof incomplete, but Petersen maintained its correctness. Similarly, at the beginning of the book, Petersen gave an elegant geometric argument for the fundamental theorem of algebra. However, with Weierstrass’ programme of arithmetization, promoted in Denmark by Nielsen, Petersen’s function theory was soon considered out of date.

In the chapter on surfaces, the classification of two-sided surfaces is proved in a geometric intuitive manner (apparently independent of Möbius); moreover, the so-called Neuman axiom is proved (it says that a connected surface with at least one boundary curve can always be cut into a disk).

Petersen used his knowledge of surfaces to give, in [Petersen 1902], a short geometric argument for the non-existence of orthogonal latin squares of order 6 (Euler’s 36 officers problem), previously obtained by Tarry in 1900 by an exhaustive search (Tarry [69]). Petersen found Tarry’s solution unsatisfactory, as it did not explain why the squares do not exist. The idea of Petersen is this: given orthogonal latin squares $A$ and $B$ of order $n$, let the first row in each be $1, 2, 3, \ldots, n$. For each $(i, j)$ with $2 \leq i \leq n$ and $1 \leq j \leq n$ form the triple $(i, a_{ij}, b_{ij})$ and colour the pairs $(i, a_{ij})$, $(a_{ij}, b_{ij})$ and $(b_{ij}, i)$ blue, red and black respectively, thus obtaining a set of $n(n-1)$ edge-coloured triangles. A given pair $(i, j)$ with
of a given colour appears in exactly two of the triangles due to orthogonality. Gluing together triangles along similarly coloured edges, a two-sided triangulated surface (not necessarily connected) is obtained. For \( n = 6 \) a use of Euler's formula then gives a contradiction.

Tarry was informed about Petersen's paper and got a copy from the editors of *Annuaire des Mathématiciens*. He wrote back in April 1902 that Petersen's proof must be wrong because of a contradiction between some of Petersen's observations and his own, and that he was certain about the correctness of the latter. He attributed the mistake to Petersen's use of Euler's formula. Then, later in April, Tarry and Petersen briefly met in Algiers and discussed the 36 officers (some letters in Petersen's Nachlass in the Royal Library in Copenhagen seem to suggest that he travelled to Algiers to buy tobacco and was not really interested in meeting Tarry). Soon after, Petersen realized that his argument was seriously flawed. What he had overlooked was that there can be different vertices with the same label in the same connected component of the triangulation. Thus the triangulation defines a pinched surface or pseudo-surface which at the multiple points fails to be locally euclidean, so that, as Tarry has correctly surmised, Euler's formula no longer applies in its usual form. In a letter to Petersen, Tarry writes:

\[
\text{Comme vous le dites, il faut chercher la cause dans ce fait, qu'il est possible que les sommets se partagent sans que le réseau soit partagé.}
\]

(Letter: Tarry to Petersen, November 5, 1902)

In this letter and also in a letter to the editor of the *Intérim des Mathématiciens*, Tarry suggests that Petersen write an explanatory note setting the record straight, but Petersen did not follow this suggestion. Three years later, in another note on Latin squares, Tarry explains what is wrong in Petersen's argument, and says

\[
\text{M. Petersen a reconnu lui-même ce défaut d'énumération des cas possibles.}
\]

(Tarry [70])

There is little doubt that *Les 36 officiers* was written at the invitation of C.-A. Laisant who edited the *Annuaire*, and with whom Petersen was, as we have already mentioned, on very friendly terms. The *Annuaire des Mathématiciens* had been conceived essentially as a world-wide directory of mathematicians; on the model of an almanach, it also contained a few mathematical papers on subjects intended to interest a large audience. That nobody besides Tarry noticed Petersen's error is perhaps an indication that Laisant overestimated the appeal of a combinatorial subject in spite of its association with the name of Euler.

Petersen's wife died in the fall of 1901, at the age of 64. This came as a shock to him and in the following years he seems to have lost some of his earlier energy. Except for new editions of his textbooks, he did not publish any more until 1907 and 1908, when he turned back to one of his earlier interests: number theory. He

\[114\] Tarry to E. Lemoine, April 4, 1902. Lemoine sent the letter on to Petersen.
wrote an 80 page long survey on the sum and distribution of quadratic residues for prime numbers of the form \(4n + 3\) [Petersen 1907], and a 100 page book on general elementary number theory [Petersen 1909], both in Danish. In these works he included some of his own earlier discoveries, among them his proof of the law of quadratic reciprocity. These became his last mathematical works.

16. Last years (1908–1910)

In the spring of 1908 Petersen suffered a stroke. But even in this condition his optimism and desire to work was not beaten. In a letter to Mittag-Leffler in Stockholm he wrote in his usual handwriting:

I feel in all respects rather well, it is only that I cannot walk and have difficulties in talking. However I hope to get so far this summer that I can resume my lectures in the autumn.

(Letter: Petersen to Mittag-Leffler, April 15, 1908, transl. from Danish)

However, his last two years became a period of physical and mental debility, where, towards the end, he hardly had any memory left of his wide interests and the rich work which had filled his life [Illusteret Tidende, August 14, 1910]. In 1909, the Danish Mathematical Society celebrated the 70th birthdays of Zeuthen, Thiele and Petersen, but Petersen was unable to attend. The same year he retired from his professorship. He died on August 5, 1910, after having been hospitalized for five months, and was buried at Vestre Kirkegaard, where Copenhagen University cared for his grave until 1947.

Only very few of Julius Petersen’s belongings are preserved to this day. Of books we have found only three, including a copy of the 1866 edition of Methods and Theories with numerous annotations in Petersen’s handwriting. They had been given around 1930 by Petersen’s brother, Valdemar Petersen in Odense, to a 14 year old boy, Poul H. Rasmussen, who was interested in mathematics, and who grew up to become an engineer (without losing his interest in mathematics). It is perhaps fitting that of all the books in Petersen’s private library among the few to survive should be the one he loved best.

Until about 1980 a collection of books and manuscripts that had belonged to Petersen remained intact in Aarhus, but then disappeared. Only special items, like his doctoral diploma (1871) and the letter of appointment from the Polytechnical School (1871), signed by the King, were preserved, due to Mrs. Åse Wiuff Borregård-Otzen, Copenhagen. Petersen’s silverware and his early Bing & Grøndahl porcelain of course still exist, but scattered among many people. An interesting item is a silver fork-spoon, given to Petersen as a gift from his

\[\text{115 They are mostly copies of some of the changes and additions that distinguish the second edition of the book from the first.}\]
children at his 70th birthday, bearing the inscription Sorte Peter (Black Peter). This had been his nickname when he was a school teacher in the 1860's.

Our story about Julius Petersen is coming to an end. One thing to learn from it is that an uncomprehending surrounding world shall be met not with bitterness and hard feelings, but with vigour and happiness; and so Julius Petersen was able to overcome serious difficulties. He must often have felt the lack of proper recognition of his achievements, yet he remained an active and happy man. Let us finish with his own words:

When throughout life you have obtained honour and money for enjoying yourself, what more can you ask for!

(Crone [12, p. 8, transl. from Danish])

17. References

Letters

If not otherwise stated, the quoted letters to Petersen are preserved in the Manuscript Collection of the Royal Library in Copenhagen, Ny Kgl.Saml. 3259, 4°, I(A–N) and II(O–Z).

The letters from Petersen to Sylow are preserved in the Manuscript collection at the University Library in Oslo, those to Mittag-Leffler in the Mittag-Leffler Institute in Stockholm.

The letters to Klein and Hilbert are in the Manuscript Division of the Niedersächsische Staats- u. Universitätsbibliothek, Göttingen (Cod.Ms. Klein 9, 11, and Cod.Ms. Hilbert 179, 302).

Books and papers


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Julius Petersen 1839–1910


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