Around Caspar Wessel and the Geometric Representation of Complex Numbers

Proceedings of the Wessel Symposium
at The Royal Danish Academy of Sciences and Letters
Copenhagen, August 11-15 1998

Invited Papers
Edited by Jesper Lützen

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Abstract

On March 10 1797 the Norwegian surveyor Caspar Wessel presented an essay *On the Analytical Representation of Direction* to The Royal Danish Academy of Sciences and Letters in which he described the geometric representation of complex numbers that has since become standard. The paper was printed in the Academy's Journal two years later. In order to celebrate the 200th anniversary of this event the Academy arranged a Wessel Symposium on August 11-15 1998. The contributions to the present volume are based on invited papers presented on that occasion. Their subjects range over a variety of historical themes related to Wessel and his family, to Wessel's work as a surveyor, to the geometric representation of complex numbers, and to the emergence of hyper-complex numbers.

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Preface

This book arose as a result of a five day symposium held from the 11th to the 15th of August 1998 at the Royal Danish Academy of Sciences and Letters in order to celebrate the bicentenary of the publication of Caspar Wessel's essay *Om Directionens analytiske Betegning, et Forsøg anvendt fornemmelig til plane og sphæriske Polygoners Opløsning.* In this essay Wessel presented his famous geometric representation of the complex numbers and made a generalisation of this analytic formalism to three-dimensional space. Wessel presented his essay to the Royal Danish Academy on March 10th 1797 and it was published two years later in the Academy's journal.

At the symposium two key talks were given about Wessel and complex numbers: Bodil Branner and Nils Voje Johansen spoke about *Caspar Wessel (1745–1818). Surveyor and Mathematician*, and Kirsti Andersen spoke about *Wessel's Work on Complex Numbers and its Place in History.* These talks were based on more extensive papers that have since been published in volume 46:1 of these Matematisk-fysiske Meddelelser together with Flemming Damhus's new complete English translation of Wessel's essay[1]. The present proceedings contain the majority of the remaining papers given at the Wessel Symposium. It can therefore be considered as a supplement to the above mentioned book, which in turn embodies the central themes around which these proceedings turn.

The Wessel Symposium and this book deal with subjects from many different historical fields: literary history, military history, history of technology, history of astronomy and geodesy as well as history of mathematics. Such a variety of subjects may be appropriate in a publication by one of the few remaining academies of both sciences and letters. What binds the different papers together is their relation to the man Caspar Wessel, to his work as a surveyor, and to the subject of his mathematical essay: complex and hypercomplex numbers. The style of the papers varies as much as their content. A few papers are conversational in style whereas the majority are more scholarly.

[1]Caspar Wessel: *On the Analytical Representation of Direction. An attempt Applied Chiefly to Solving Plane and Spherical Polygons. 1797. Translated by Flemming Damhus. Introductory chapters by Bodil Branner, Nils Voje Johansen, and Kirsti Andersen.* Edited by Bodil Branner and Jesper Lützen. Matematisk-fysiske Meddelelser 46:1. Det Kongelige Danske Videnskabernes Selskab, Copenhagen 1999.

I wish to express my gratitude to the following foundations that supported the Wessel Symposium financially: The Danish Research Councils, Knud Højgaards Fond, Norske Selskab and last but not least The Carlsberg Foundation, which has also paid for the publication of these proceedings. I also wish to thank Pia Grüner and Flemming Lundgreen-Nielsen at the Royal Academy for their help with the project and the authors who produced good scholarly works and conscientiously refereed each other's papers. Finally I am obliged to Jan Caesar at the Department of Mathematics, Copenhagen University, for having produced a nice and uniform LaTeX layout of all the contributions and to Knud Sørensen, Aarhus University, for his careful correction of the language.

Jesper Lützen

The Naval Hero Peter Tordenskiold & the Wessel Family

Hans Christian Bjerg *

1 The Wessel Family

The Wessel Family has contributed in several ways to the historical development of the Double-monarchy of Denmark-Norway which ended in the year 1814 when the two kingdoms separated. This volume is primarily dedicated to an example in the area of science, and it also contains a contribution about Johann Herman Wessel, one of our famous poets. And in the Danish and Norwegian military history you will also find the name *Wessel*. One of our most famous naval heroes, Peter Tordenskiold, was christened Peter Wessel and was a true member of the mentioned family. This paper will deal with this man and his merits in the beginning of the 18th Century. Everyone in Denmark and Norway knows the name Tordenskiold. Several anecdotes are told about him, but his fame is probably mainly due to the fact that during this century his portrait has been reproduced on the most sold matchbox in Denmark.

But first a little about the Wessel Family. In the middle of the 18th Century the family was of the opinion that they had immigrated from the Netherlands, and indeed there is a Dutch locality called Wessel. But it is a fact that in the 16th Century one finds the family name Wessel in a couple of the

*Chief Archivist, The Military Archives, The Danish National Archives, Rigsdagsgården, DK–1218 Copenhagen K, Denmark.

Hanseatic cities in the Northern part of Germany, where the trade agents had good connections with Norway, especially with Bergen, where German merchants occupied a part of the city. It is also possible that the Wessel family could have immigrated this way. We find for the first time a Wessel from the Netherlands in Bergen in Norwegian files from 1593.

In any case, around 1620 a man with the name Jan Wessel lived in Bergen, and the grandfather of the naval hero Peter Tordenskiold named Henrik Jansson Wessel is traced in the archives from the middle of the 17th Century as a merchant and citizen of Bergen. Henrik Wessel's oldest son, Jan Wessel, was born in 1646 and went as a grown-up to Trondheim, a town in Norway north of Bergen. He married a very young girl, born 1656, Maren Scholler, who was out of one of the rich families in Trondheim. As a 16-year-old girl she already gave birth to her first child. Within the next 26 year she became a mother of 18 children — 12 sons and 6 daughters. An anecdote tells that at an advanged age Jan Wessel was on a trip sailing with a merchant ship to Spain. The ship was boarded by French privateers. Investigating the passengers they were told that Jan Wessel was a father of 18 children. The privateers were so impressed with a man of such a "capacité" that they left the ship without doing any harm.

Jan Wessel owned some buildings in Trondheim and some ships and was as a whole a respectable citizen. He became what we can call an alderman and a member of the Council of the City in 1693. His Bible is still kept in Norway and on the front page of the book we can follow the growth of the Wessel family. Every new child was carefully listed.

Son number 10 was Jan Wessel named after his father. He went to sea and became a chief pilot in Norway. This man became the grandfather of Caspar and Johann Herman Wessel. Peter Tordenskiold was in this way granduncle to Caspar Wessel.

Of the 6 daughters 2 died very young, 3 married merchants in Trondheim, and one married a vicar. Of the 12 sons 4 were educated as clergymen, 1 died young, and the rest went to sea. Of these, 2 died in the Dutch navy, 1 became captain in the Russian Navy and 2 became viceadmirals in the Royal Danish Navy, "but only one of these was Tordenskiold" — as an old song goes.

Child number 14 was Peter Jansen Wessel. He became a naval officer just when the Great Northern War 1709–1720 broke out. In this war Sweden's

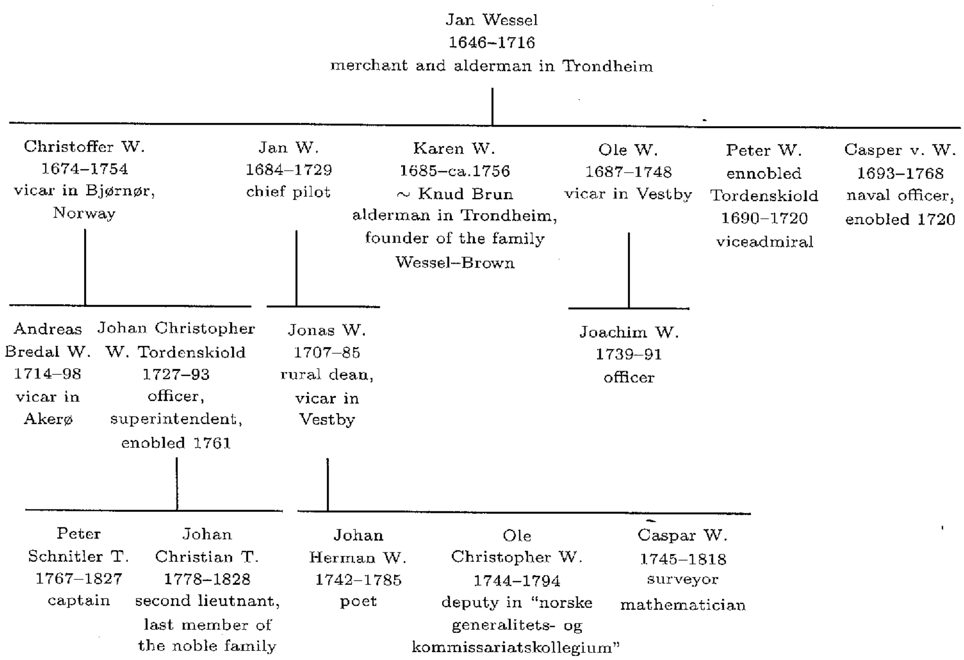

Figure 1: The genealogical tree of the Wessel family

neighbours tried to reduce the Swedish dominance in the Baltic area. The Danish king was primarily interested in getting the former Danish areas in the western part of Sweden back to Denmark. Because of his merits Peter Wessel was ennobled in 1716 with the name TORDENSKIOLD. It means thunder-shield or freely translated Thunderbolt. On that occasion he got a coat of arms which illustrates his profession and background. He was a thunder for the enemy and a shield for his king and country as an old interpretation of the name goes.

Caspar Wessel (1693–1768) was Peter's younger brother. He was promoted to viceadmiral, which allowed him to marry a rich widow and get rid of his great debt. However, he was not as strong a character as Peter and was promoted as a result of his older brother.

I have mentioned a couple of names from the Wessel family and maybe you have observed that the family re-use their Christian names. This tradition

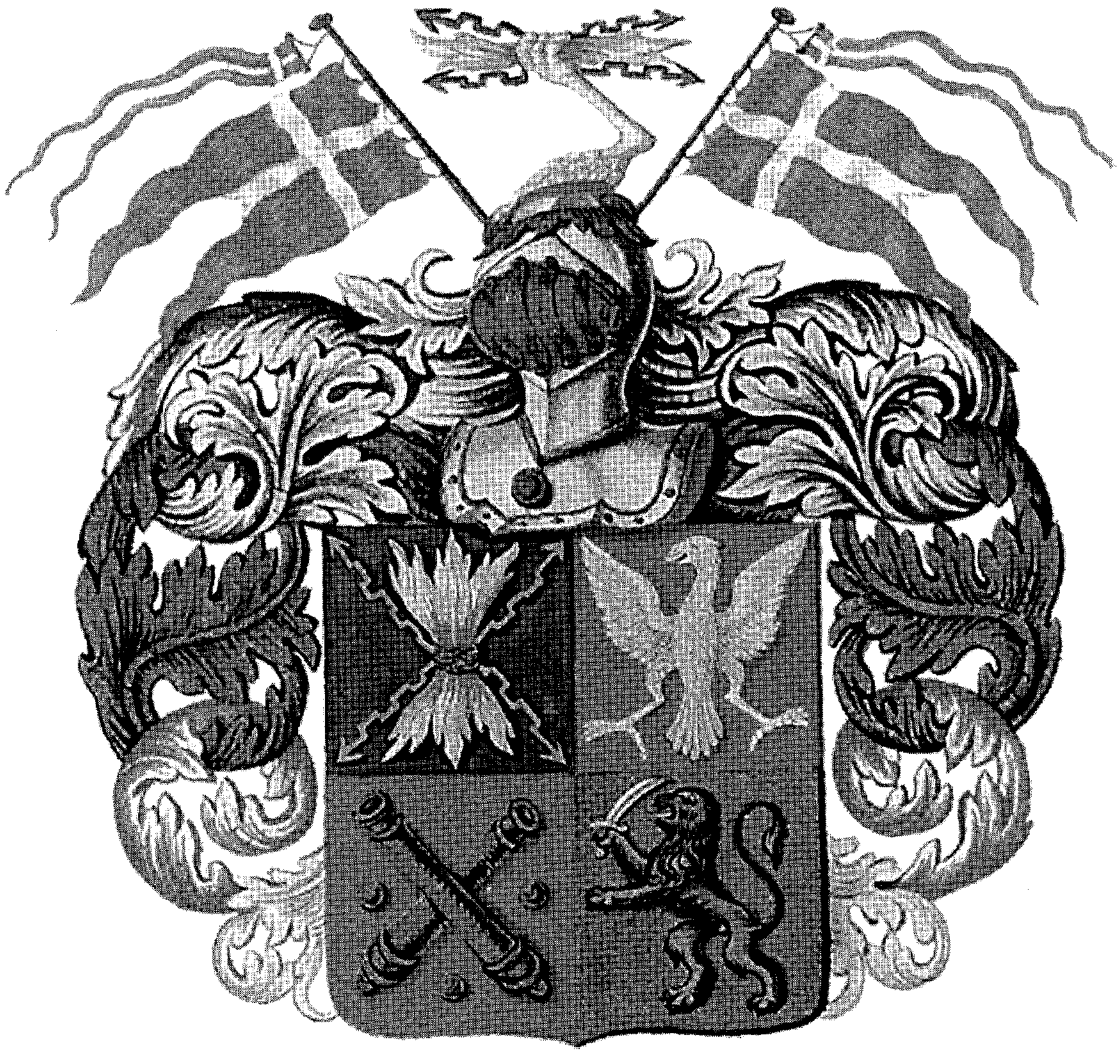

Figure 2: Tordenskiold's Coat of Arms. 1.) Shows the symbol of lightning and thunder on a blue background referring to the name Tordenskiold. 2.) Shows a white eagle on a red background referring to Tordenskiold's capture of the Swedish warship VITA ÖRN in 1715. 3.) Shows two cannons and three cannon balls on a red background symbolizing the three gunshots which were the special mark for Danish warships in former time. 4.) The Norwegian Lion Statant in gold on a red background refers to the Danish-Norwegian nationality of Tordenskiold. It is the Norwegian coat of arms in the colours used in the Danish coat of arms.

has continued up to these days.

2 Peter Wessel

We have a lot of anecdotes and stories about Peter Wessel as a child. He was apparently a wild boy according to the first biography about the Danish-Norwegian naval hero published 20 years after his death. By nature he was an adventurer and he stayed often by the harbour and on board his father's ships. We know that as a 14-year-old boy he arrived in Copenhagen. Indeed at that time he visited one of his father's friends in the Danish capital, Dr. Peder Jespersen, who was then a spiritual adviser to the Danish King Frederik IV, a man of importance. Peter Wessel stayed in the house of Dr. Jespersen, who wrote to the parents in Trondheim that their son was now in Copenhagen. According to information in the archives Peter ran away from home without giving any messages to his parents. We do not know how he made his way to distant Copenhagen. The Danish king made a voyage to Norway in 1704 and passed Trondheim on that occasion. We suppose that Peter Wessel joined the king's retinue one way or another and followed the company back to Denmark.

Peter Wessel' s passion was the sea. Through Dr. Jespersen Peter Wessel had had the opportunity to meet the King, and in 1706 he applied to the King to become a midshipman in the Royal Navy. The Royal Naval Academy had been established in 1701 in Copenhagen as one of the first of that kind in Europe. However, there was no place vacant at that time and Peter Wessel was urged to get practical experience as a sailor. He therefore signed articles on board a ship going to Africa and the West Indies as a Slave transport. He was on board this ship until 1708. In 1709 he became a midshipman, but at that time he was on his way with a new ship to East India so he did not get the message about his new position until he returned to Bergen in 1710.

The war with Sweden broke out in 1709, and the need for naval officers was very acute. Peter Wessel therefore got only a very brief and quick training at the Naval Academy. In spite of that he was already an experienced navigator, and in 1711 he was promoted to lieutenant and worked as the second in command on one of the small frigates. He then was detailed for duty in the coastal navy in Norway. In 1712, when he was 21 years old he became captain of a small frigate called LØWENDAHLS GALEY of 20 guns.

At that time his superiors began to observe Peter Wessel's special talent for naval warfare and tactics. To understand this fact it is necessary to explain a little about the naval tactics practised in the very beginning of the 18th Century.

The armoured square-rigged ship became common from the beginning of the 17th Century as the normal platform in naval warfare. The technology of this type of ship indicated that the guns were placed along the sides of the ships and that the stern and the bow were practically without any guns. The warships of that time fought in the so-called line ahead in order to maximise the shooting angles of the guns and to minimise the dead angles at the stem and at the bow. At the end of the 17th Century there were developed two different tactical ways to use the warship. The Mêlée tactics and the formal tactics. In the beginning of the 18th Century the formal tactics were accepted by all great naval powers as the correct way to make warfare at sea. The drawback of the formal tactics was that the battles at sea stagnated. The formations of the ships became too defensive and it was difficult to destroy the enemy. Yet, in all the naval academies at that time formal tactics were taught as the only one and it was the only one which was allowed according to the fighting instructions and the rules.

As I mentioned, Peter Wessel's stay at the naval academy was too short to indoctrinate him with the lessons and rules of the time. He saw that the best way to attack the ships of the enemy was to attack them in their dead angles, which was not the normal way to do it. His tactics were very surprising for the opponent, and as a consequence were very successful. In short, Peter Wessel followed his own tactical instinct instead of rules given at the academy.

In 1712 he was called on duty in the Main Fleet of the Danish Navy which operated in the Baltic Sea against the Swedish Navy. His efforts were remarkable and he became famous for his risky but effective way to do scout raids for the Admiral in order to gather intelligence about the enemy. As an award he was promoted to lieutenant commander jumping over 51 other lieutenants placed before him on the Officers' Roll. In 1713 he worked in the Norwegian Sea and was hunting Swedish merchant ships and privateers. His results were remarkable.

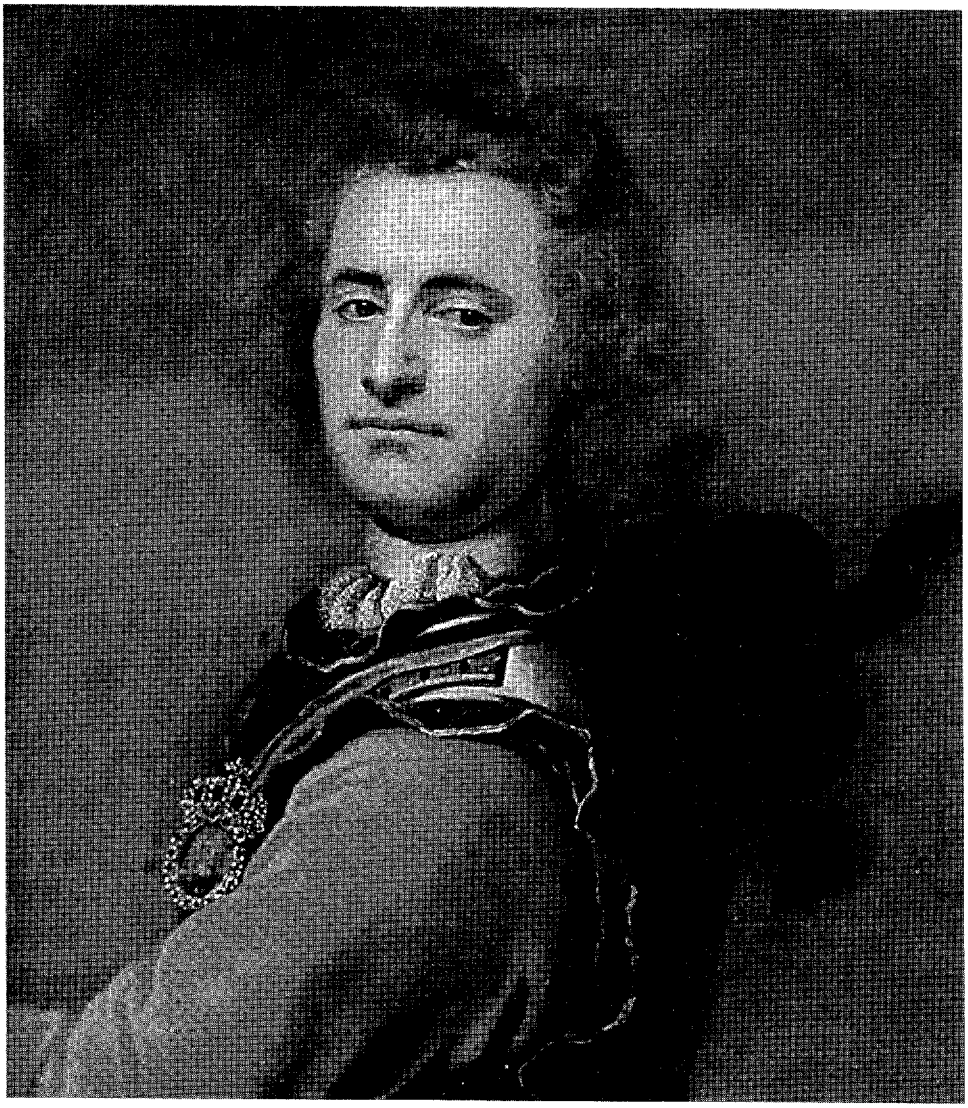

Figure 4: A portrait of Peter Tordenskiold as a Vice-Admiral, painted in the year 1719

In July 1714 Peter Wessel observed a ship under British colours. He himself sailed under Dutch colours! The other ship struck the colours and flew the Swedish flag. Peter Wessel then showed the Danish colours. The other frigate was bigger than Wessel's, and according to the Danish fighting instruction it was not allowed to fight with ships superior to one's own. Nevertheless, Peter Wessel continued the fight. During the fight the other ship was heavily damaged, and Peter Wessel decided to fly the white flag, the flag of truce, communicating that he wanted to talk with the captain on board the other ship. Wessel sent out a small vessel with a negotiator, who brought greetings from Wessel. The negotiator told the enemy captain on behalf of Wessel that the Danish ship had only very few cannonballs left, and therefore he would ask the enemy captain if he could lend him cannonballs so that the fight could go on. To this surprising question the enemy captain answered that he only had what was necessary for himself, but that he would like to present a toast to the Danish captain. After Wessel's negotiator had returned to the Danish ship the two enemies sailed very close to each other, toasted over the bulwarks of the ships and asked each other to give friends in Copenhagen and Gothenburg respectively their regards. Through this manoeuvre Wessel succeeded in gathering information about the enemy. The Swedish ship was bought in England and was on its way to Gothenburg under the command of a British captain.

When he returned to his base in Copenhagen, Peter Wessel wrote a complete report about the fight and told about the message to the enemy captain and the toast afterwards. Some of the members of the Danish board of Admiralty became very angry reading this report. From their point of view Peter Wessel had uncovered his own weakness in front of the enemy, a weakness which the enemy captain could have used to take Peter Wessel's ship. The young naval officer had offended against the Naval Articles and he was court-martialled.

The point here is that we are not dealing with an anecdote, but with facts. All the papers in this case can be found in the archives. In the files we can follow the court-martial. Wessel explained that the enemy ship was so damaged that there was no risk for him to go closer to the other ship. On the other hand the ship was too superior to board. He had sent the negotiator over in order to get information about the ship because it was not known in the theatre before. Peter Wessel was cleared off. Just after the acquittal he went to see the king, who promoted him to Commander in the Navy. He was 23 years old.

It is not surprising that he was already envied at that time. In a letter he wrote that he was not looking for a higher charge but for more responsibility. It was the challenge and its realization which fascinated Peter Wessel.

1715 became a crucial year for the young naval officer. He served in the main fleet in the Baltic Sea as a scout for the Admiral and a cruiser captain. Based on intelligence about the Swedes fleet provided by Peter Wessel it was decided to let the Danish fleet attack in April. The result was the Battle of Fernern, where the Swedish suffered a defeat. With his small frigate Peter Wessel was very active during the battle. He and his crew boarded a heavy and new Swedish frigate called VITA ÖRN, The white Eagle, and prevented the Swedish captain from destroying his own ship. Afterwards Peter Wessel observed that a group af enemy ships, which at first had escaped from the battle, had been grounded by its own crews. One of the ships was the Swedish flagship with the enemy admiral on board. Wessel sailed straight to the flagship, took the admiral prisoner and ordered him to stop the self-destruction of his ships.

As a reward for an excellent effort Peter Wessel got the command of the conquered Swedish frigate, which was incorporated in the Danish-Norwegian Navy under the name HVIDE ÖRN. In October of the same year he opened battle with a ship-of-the-line and a heavy frigate, which escorted a convoy in the Baltic Sea. In spite of the Swedish superiority Peter Wessel succeeded, thanks to his excellent tactical talents, in damaging the two ships seriously and he took a great part of the convoy as prizes.

After this event reports were sent from the fleet to the Admiralty in which Peter Wessel was praised for his tactical skill. In February the following year 1716 came his ennoblement.

3 Tordenskiold

When you study the history of Tordenskiold you will discover that the reality is often more fantastic and incredible than the myths and the anecdotes.

One evening when Tordenskiold was dining at court, the king expressed the wish to know the opinion on the on-going war among the former Danish subjects in Sweden. After the royal table Tordenskiold went out to Holmen, the naval base, appointed a crew to a gunboat, and sailed to the Swedish coast during the night. Very early in the morning he reached the enemy coast. The two nearest buildings turned out to belong to a vicar and the local coast Watch officer. Tordenskiold woke up the vicar and forced him to follow him back to the boat. A servant, a young boy, woke up and was forced to go to the boat too. The officer suffered the same fate as the vicar.

All three then sailed during the sunrise to Copenhagen, and were presented to the king during his breakfast. The king was very amused with Tordenskiold's crazy idea. He talked with the Swedish prisoners and gave them gifts. Afterwards Tordenskiold brought them to the Naval Base where the vicar and the officer were cross-examined. The vicar was an elderly man and could remember when the western coast of Sweden belonged to the Danish king. He did not mind telling all that he knew about the Swedish king, the army and the contemporary political conditions in Sweden. All the questions and answers were noted in a file. The officer refused to tell about the conditions in Sweden. The following night the three Swedes were brought back to their home by Tordenskiold's crew.

During the following days the Swedish authorities interrogated the vicar and the officer about their involuntary trip to Copenhagen and about the public conditions in the Danish capital. The vicar and the officer both told that the Danish Naval authorities had tried to interrogate them about Sweden but that they had told nothing. Actually the vicar said that his lips had been sealed during the interrogation.

This story is not a myth either. In the Swedish National Archives they keep the files about the Swedish inquiries, and in the Danish National Archives we have the protocols belonging to these interrogations in Copenhagen.

On a later occasion Tordenskiold amused the king in a similar way. Again he went to the Swedish coast. In the nearest house a wedding was being celebrated and Tordenskiold and his crew so to speak kidnapped the newly married couple and took them to Copenhagen and presented them to the king, who gave them gifts. They were also brought back to their home. The files do not tell us whether or not the wedding continued after the return of the kidnapped bride and groom.

Tordenskiold repaid the coat of arms in June 1716. A part of the master plan of the Swedish King Charles XII was to take Norway from the Danish king. In the middle of 1716 he began a campaign against Norway. To support his advancing army Charles XII ordered a big transport fleet with supplies along the Swedish western coast to meet the army near the border to Norway. The success of the campaign depended on this fleet.

At the end of May 1716 Tordenskiold investigated the Swedish coast south of Gothenburg when he got intelligence about the mentioned transport fleet. He followed the fleet and found out that it had anchored in a small and very narrow creek called Dynekilen. The entrance was only 100 metres wide. The Swedish officers were so sure that this place was safe that in the evening they went for a party on shore.

Tordenskiold had no orders to attack, and the correct behaviour would have been to get such orders from his superior. But he evaluated that the surprising moment would disappear if he waited for orders. The wind was favourable and he went into the creek and took a part of the enemy transport ships as prizes and destroyed the rest. This operation had a major influence on Charles XII's war campaign against Norway. The Swedish Army got no supplies and the army was forced to withdraw to Sweden. Dynekilen is considered the greatest and most important of Tordenskiold's merits.

During the year 1717 Tordenskiold again operated in the Baltic Sea but was not very successful. He also attacked the fortresses of Gothenburg without luck. The following year he commanded a scout squadron based on Christiansø with the task of observing the movements of the Swedish Fleet based in Karlskrona. In the last months of 1718 he was called to the Norwegian theatre of War again because of a message about a new campaign against Norway. In December he heard as one of the first commanders about the death of Charles XII at Frederiksstad. One of Tordenskiold's advantages was his talent for a permanent gathering of intelligence. He was very much aware of the importance of the death of the Swedish king for the whole war. He therefore immediately sailed on a fast-going vessel to the king in Copenhagen. Tordenskiold was the first to bring the message to the king, and as a reward he was promoted to rear-admiral on the spur of the moment.

In July 1719 Tordenskiold began to lay siege to the important fortress Karlsten at Marstrand on the Swedish west coast. After a month the fortress was captured without any fighting and due to a trick — very typical of Tordenskiold. An anecdote tells us that Tordenskiold in the disguise of a fisherman went into the enemy city, sold fish and interrogated the citizens about the troops in the fortress. The troops were German mercenaries.

In Denmark we have a special phrase: "Tordenskiolds soldater" (The soldiers of Tordenskiold), meaning that it is the same people you se all over: in the media and in the boards of societies and companies etc. This phrase comes from Tordenskiold's siege of Karlsten-Marstrand. It tells that Tordenskiold invited the commander of the fortress to inspect his troops which were lined up in the city streets below the fortress. The commander went through all the streets in the town and everywhere he saw troops lined up. He realised that he did not have a chance to resist Tordenskiold and so he decided to surrender under the condition that all af his troops were allowed to leave the fortress unharmed. In reality Tordenskiold's limited troops after the inspection by the Swedish commander ran around the corner and were lined up in another street where the commander then inspected the same troops for the second or the third time.

We know in fact that the commander was not really tricked by Tordenskiold on that occasion but was afraid of the reinforcements which he knew were coming up from Denmark.

After the capture Tordenskiold was promoted to viceadmiral and member of the Admiralty. As a supplement he received a portrait of the king framed in brilliants. This portrait was only given four times in the reign of Frederik IV.

When the peace came in 1720 Tordenskiold was restless. He was a typical man of war, so he planned a journey to Germany, France, England, and maybe also to Russia to look for new challenges and offer his naval expertise. In Hanover in Germany Tordenskiold was involved in a quarrel with a Swedish-Lithuanian colonel. It ended in a duel in which Tordenskiold was killed on the 12th of November 1720. There have been a lot of discussions about how the duel was settled but we shall not go into the details here.

4 Remembering Tordenskiold

The body of Tordenskiold was brought home, but there were difficulties with the funeral because according to Danish law duels were forbidden, and victims of duels were therefore not allowed to be buried in consecrated earth. In spite of his great and many merits for the country many important and envious persons blocked a funeral ceremony of the great naval hero. He was placed in a coffin in the cellar of Holmen's Church in Copenhagen where he rested until 1819 when the Danish king Frederik VI erected a sarcophagus for his body.

In 1864 a statue of Tordenskiold by the sculptor H.W.Bissen was erected in front of the Cathedral in Trondheim in Norway. Later, in 1879, a similar statue was erected near Holmen's Church.

Already during his lifetime Tordenskiold was the subject of several publications. Since his death nearly 1,000 books and articles have been published about the naval hero. And the bibliography still gets new titles about Tordenskiold. A book with all his letters — around 1,200 — written in the service has been published.

The administration of his estate has its own history, because it became the longest and the biggest in Danish history. It took 24 years to carry it out. The State owed Tordenskiold a lot of money for the many prizes he took during the war, but he didn't leave any heirs, so the State was not interested in giving the many brothers and sisters the money. Therefore the state authorities tried to avoid its obligations. As for the family, they had a lot of quarrels over the money and other values left by Tordenskiold.

On Tordenskiold's sarcophagus in Holmen's Church is engraved the names of his victories: "Dynekilen – Marstrand – Elfsborg" together with the following inscription:

> HIS KING MENTIONED HIM WITH HONOUR HIS ENEMIES WITH FEAR
> THE ANNALS OF DENMARK KEEP HIS MERITS
> IN REMEMBRANCE
> FREDERICH VI ERECTED THIS SARCOPHAGUS TO HIM.

5 The Chronology of Peter Tordenskiold

1690, 20 October, Born in Trondheim, Christened as Peter Janse Wessel

1704–06 In Copenhagen

1706, 28 March, First application to the King in order to become a midshipman

1706–08 On board a slave ship bound for Africa and the West Indies

1708–10 On board a merchant ship bound for Tranquebar, India

1709, 11 January, Received into the Naval Academy

1709–20 The Great Northern War

1711, 17 July, Sub-lieutenant

1712, 14 October, Lieutenant-Commander

1714, 28 December, Commander

1716, 24 February, Ennobled as **Tordenskiold**

1716, 8 July, The Destruction of the Swedish Fleet in Dynekilen

1716, 18 July, Captain in the Danish Royal Navy

1718, 30 December, Rear-Admiral

1719, 25 July, The Capture of **Marstrand** Fortress

1719, 17 August, Vice-Admiral and member of the Admiralty

1720, 7 September, Leaving Denmark for a trip to Germany, France and England

1720, 12 November, Death of Tordenskiold in a duel in Germany

1819, 17 July, Unveiling of the sarcophagus in Holmen's Church

1879 Unveiling of the Statue of Tordenskiold at Holmen's Church

Johan Herman Wessel

Bjørn Linnestad *

I would like to start with the back scene, so to speak, for the performance of the Wessel brothers in Copenhagen.

The four hundred years' history of the Danish-Norwegian twin monarchy has been, and still is, a mental complex to many Norwegians. The united kingdoms were the result, not of conquest and war, but of legal heritage. I think this background is important for the understanding of the Norwegians' position in eighteenth-century Denmark.

It was not a disadvantage at that time to be Norwegian born. The Jutlanders and the Norwegians were both to some degree alien to the Copenhageners with their different language and manners. However, there was a difference in how the two provinces were represented in the capital: Jutland broadly and unselected, the whole scale of persons from the manual worker and the maid to the tops of society. Norway on the other hand was selectively represented, mainly by two groups, the university men and the guardsmen — both pick of the crop, either intellectually or physically.

It has also been claimed that the Norwegian in general had a wider horizon, geographically, than the Dane. Denmark's income was based on inland farming, while Norway for a large part made a living of trade and shipping. The Norwegian, if not the average, knew London and had a cousin in Bordeaux. The Jutlander represented to the Copenhageners simplicity, the Norwegian complexity. However, as Johan Herman Wessel so tolerantly put it: "we are all Jutlanders to our Lord."

At the time of the Wessel brothers, Copenhagen had 50,000 inhabitants, and the university men were clearly visible in the town. Around 1770 the literarily interested Danes made their circle around the poet Johannes Ewald,

*Medisinsk Afdeling, Sykehuset Østfold Askim, N–1800 Askim, Norway

and the Norwegians gathered around Johan Herman Wessel. The former group cultivated pre-romanticism, the latter the rational tradition from the Greeks and the Romans. Both circles willingly leaked their wit and irony to the outer public, and the group around Wessel had not the minor admiration. The literarily conscious Copenhageners used Norwegian words and expressions to signal that he was up to date.

The Norwegian literary circle became formally Norske Selskab — the Norwegian Society — in 1772, and through the following years it was constantly watched over by the authorities. This group of young idealists could represent the seed of Norwegian independence. The members of the Society, however, were in general loyal to the King, but they worked for more extensive home rule and a Norwegian university.

The Norwegians were, like today, very patriotic. They sang "For Norge, kjempers fødeland" — "To Norway, birthplace of giants" — written by Johan Nordahl Brun, a later bishop of Bergen. Johan Vibe, Claus Fasting, Niels Bredal were the other well known names. Whether they were students of law, of theology or something else, they were all writers, poets and quite a few also playwrights. In sum they offered a substantial impact on Danish society.

From the beginning of the 1760's the four Wessel brothers from Vestby in Norway were gathered in Copenhagen, and inside the circle that later became Norske Selskab. Jonas, the eldest, had low ambitions:

> Ett jeg vet for visst
> jeg vil blive copiist

In my translation:

> All I want, in this I'm stark
> I shall be a common clerk

Those two lines are, to my knowledge, his complete works — and he succeeded, he became a clerk in the Ministry of Finance. The three younger brothers, Johan Herman, Ole Christopher and Caspar all did excellently at the University. Strangely enough, Johan Herman broke off after two lower grade exams. His knowledge of Hebrew had not been convincing, but apart from that he got top marks in physics, arithmetic, geography, metaphysics, history, logic and Latin. He was obviously broadly gifted.

Why Johan Herman stopped in his academic career has never been satisfactorily explained. It could be due to a shift in mood and a longing for isolation. His life during the following ten years is very obscure. The only thing known to posterity is that he taught himself the living languages, English and French, later in life also Italian and Spanish.

Ole Christopher became an outstanding capacity within the law. Caspar also became a lawyer, but first and foremost an excellent topographer and mathematician. Johan Herman Wessel, however, is my main topic.

There are some mysterious or at least strange features in this man's life and in our apprehension of him. I am going to focus on three questions and thereby I hope to throw some light on the fascinating Johan Herman.

1) Why is so little known about him?
2) Why has he, as a writer, survived his contemporaries?
3) Why did this brilliant poet produce so little?

It is a striking fact that very little is known about Johan Herman Wessel in the biographical sense. He himself wrote nothing in that respect apart from a few ironical lines. Here I have to put in an apology: We have, if any few translations of Wessel's work. His expression is cunning with many shades, and translation is difficult. If I were to try to put him into English words, I would certainly not convince you of his linguistic dexterity. I have therefore chosen to quote him in his own language — and I ask English-speaking readers to excuse this.

> Han syntes født til bagateller
> og noget stort han ble ei heller

He thought himself born to trifle away his life — and subjectively so he did.

Why then did his contemporaries not write about him? After all he was surrounded by academics with pens. And he was the most prominent of his circle, the "Blossom and Crown" in the Danish poet Oehlenschläger's words. This reserve could be due to Wessel's overt alcoholism and represent a considerate discretion. In addition, with the 1770's came a shift in attitudes in the Danish-Norwegian society. The previous decades had been liberal in Copenhagen as in the rest of Western Europe: Rousseau's philosophy and writings, Henry Fielding's novels, and John Cleland's "Fanny Hill". Copenhagen had a lag, but in 1746 the puritan king Christian VI died and was succeeded by his son Frederik V. The grave and stiff life at court changed into orgies, and under the next king, the mentally insane Christian VII, it grew worse still. Dr. Struensee became in reality head of State. He liberalized society, but also made the Queen pregnant. It had to end in a tragedy and Struensee was executed in 1772. It took 200 years before the state of Norway the next time appointed a medical doctor prime minister. The reaction to the dramatic events came, not only politically but also morally. To enjoy Wessel grew, at least officially, out of fashion.

Another fact difficult to explain is Wessel's popularity through more than two centuries. He presented no deep thoughts, he shunned topics more or less compulsory to poets: the praise of God, the praise of Nature, the praise of Women and Love. Unlike his fellow poets he avoided patriotism and heroism. Why then is he quoted more often than any other poet of his time? I think the answer simply is his mastery of sentences. He often used the alexandrine verse with the same number of syllables in each line and a rhyme at the end. In "Kierlighed uden Stømper" — "Love without Stockings" — it sounds like the title of an erotic movie today — we are told that Johan the tailor has been away to mend the trousers of a major, but one can suspect additional activities. On his return he addresses his girlfriend Grethe with the smooth, elegant and flattering alexandrine lines:

> Hvor ser jeg glad igen
> de mange yndigheder
> som den må være blind
> der kan ej se hos Eder

In my translation:

> Now glad again I see
> the many charms of you.
> The blinded only doubts
> that this is fully true

As I said, Wessel avoided the traditional topics of a poet, God, Nature etcetera, with very rare exceptions. Wessel's God is a tolerant power:

> Det sanne vel og ve er frukt av dyd og last
> og denne sannhet står til Himlens ære fast

True happiness is based on a combination of virtue and vice and Wessel's God has accepted this.

Born in a rural community in Norway, Wessel became ultra-urban. He rarely and unwillingly left the streets of Copenhagen. Once in spring, however, his friends had persuaded him outside the town walls to make homage to flourishing nature. The result was two of Wessel's most wonderful lines:

Og så, den grønne mark, den var
ja, Herregud
man ved hvordan en mark
når den er grøn
ser ud

In my translation:

Look — The meadows, the fields.
Oh, good old Lord.
The nature yields
what the eyes afford

Even if nature was delightful, it deserved no more than a couple of lines from him.

So to the field of love and eroticism. He avoided those themes, the historians of literature tell us. Israel Levin, the great connoisseur of Wessel, said that in all his writing we do not find a vulgar word, not an obscene thought, his Muse was entirely pure. Levin worked and wrote in the Victorian era, and I think he has voluntarily overlooked a little poem: Let others enjoy gold, Wessel himself found Heaven in the crotch of his girl.

La andre tenke, sige
Gull giør oss lykkelige
jeg fant mitt Himmerige
min pige
— i ditt skjød

It was to me unthinkable that a young man surrounded by other young men should not cultivate at least the conception of eroticism. So I started my own hunt, simply to try to rehabilitate Wessel as a normal, healthy man. In the archives of the University Library of Oslo I found handwritten notes after Gregers Lundh, the first professor of economic history in Norway. He wrote a never published "Contribution to a Collection of J. H. Wessel's Obscene Poems." I shall not give you the most vigorous verses, only a few lines that describe the little "thing" a girl possesses:

En pige er en ting
som nok en ting beskytter
og uten denne ting
hun mannfolk ikke nyter

og vil man i den ting
en annen ting innføre,
fremkommer der en ting
som nye ting kan gjøre

It is an old speculation why this brilliant and gifted poet did not produce more than he did. His complete works constitute little more than one volume. Wessel described himself as lazy:

Han åt og drakk
var sjelden glad
sine støvlehele gikk han skjeve
Han ingen ting bestille gad
tilsist han gad ei heller leve

At last he did not even bother to live.

He also compared himself to his diligent brother Caspar:

Han tegner landkort og leser loven
er lig'så flittig som jeg er doven

For several reasons I do not believe in the "laziness theory". There must have been another explanation for his scarce production.

When Johan Herman Wessel died 43 years of age, he left an impressive debt, a widow and a son he doubted was his own. Then his friends published his complete works to support the widow with the income. In the preface Christen Pram regrets that his gifted friend did not write more than he did. Neither he nor the other contemporaries can explain this. Pram thinks Wessel was hampered by ill health, and tells that the poet was bedridden for more than a year due to a painful fistula in the jaw. This fistula must have been caused by a tooth infection. Wessel suffered from toothache which he described as the Devil's stand-in on earth:

Du er på Jorden vise-Fanden
og halve værre enn den annen

However, once a fistula opens, the pain goes. Christian Pram's explanation was wrong. What else makes a 34-year-old man go to bed and stay there for a year? It could be a palsy, an orthopaedic problem or heart or lung disease. However, all these conditions give symptoms easy to observe — and no such observations were made. What to look for then? My diagnosis is a grave and deep depression, most likely the genuine melancholy seen in persons suffering from manic-depressive disease. Do we have other facts to support this? Yes, certainly. Shortly before his long stay in bed Wessel had written his only two serious poems "Ode til Søvnen" — "Ode to sleep" and "Nøisomhet" — "Contentment". In many verses he overtly describes his way into darkness:

> Som jeg den halve klode var
> innsvøpt i mørkhets tause tåge
> og på de lukte øyenlåge din grumme broders bilde bar

On the back of his eyelids he saw the picture of death — the grim brother of sleep. In another verse he indirectly tells us that he has experienced the same before. He knows that light will come back:

> Men ilden ulmer, livet gror
> Du snart igjen skal krefter vinne
> Min ånd du snart igjen skal finne
> en morgen skjønn, din Skaper stor

I shall not tire you with all the pieces of the puzzle that in my mind document his deep depression. But what about the maniac phases if he were manic-depressive? His "ups" must most likely have been less pronounced than his "downs" and therefore less obvious. I find it however bordering on the pathological to write a masterpiece like "Love without stockings" in six weeks. Maniac phases can also manifest themselves as quarrels difficult to understand. Now we have to remember that Johan Herman was a shy person, very polite and very considerate in all his being. A couple of years after "Stockings", perhaps he was still "upgoing", Wessel published "Herremanden": a squire dies and ends up in Hell because he has exploited his peasants:

> En herremann sov engang hen
> og så skal alle herremenn
> hvor gjerne de enn leve ville
> Og det er ille
> å dø når man ennu ei ville

In hell the squire is very astonished to meet his personal servant Jochum. It turns out that Jochum had made the squire's wife pregnant and therefore ended up underground.

In Copenhagen the rumours started: the poem was to be read as an obituary of the late general Nergaard. The general had a daughter and two sons, one of whom was accused of adultery — if the poem really had a concrete address. After some hesitation the son attacked Wessel crossly in the paper, and the provoked poet answered with a proposed epigram for the squire's tombstone:

Her hviler Krigsraad Nergaard
den store, store, store
Kun Himlen vet hva godt han
gjorde, gjorde, gjorde
At tale om hans kjære frue
så var hun vennlig som en due
At tale om hans tvende sønner
det ei umaken lønner
Men når jeg tenker på hans datter
kan jeg ei bare meg for latter

Here rests general Nergaard. If he ever did a good thing, only Heaven knows.

Not only Nergaard but also his wife, sons and daughter are exposed to disgrace. The mentally sound Wessel would never do such a thing.

On another occasion, we are now in the 1780's, Wessel also acted very inconsiderately and very unlike himself. At that time he rented a small flat in Fiolstræde where he lived with his wife and son. The landlady had the habit of steadily creating rows with everybody around her — Wessel included. Then the landlady died, and a collective sigh of relief must have been heard. Wessel owed her money, but he was broke. The family of the deceased then suggested that instead of payment he should write an epigram for her grave. From a famous poet that gave prestige woth more than money. Wessel had no money and therefore no choice — and he presented the lines:

Hun er død, hun er begravet
Jeg har fred, og nabolavet

In my translation:

Now she rests in the coffin's wood
I'm at peace like the neighbourhood

It certainly was tactless, no matter who the lady was.

The Johan Herman known by his friends had a mild, intelligent irony without any aggression. Once a pompous officer said to Wessel: "If I had a stupid son, he should become a poet." The answer came immediately: "Your father obviously had another opinion."

One could suspect that Wessel's premature death was linked to his psychiatric condition, but that was not the case. During his last months he was in a good mood, and it was an accidental infection that killed him. Nerve fever was the diagnosis in those days, today it is typhoid fever.

From Johan Herman's deathbed we have the last glimpses of his irony. A friend visiting him noticed a big jug of water: "Is it really you drinking water?" "Yes," the poet replied, "if this is going to be the end, I must reconcile myself to my foe."

It is likely that Johan Herman left this world with a smile, even if that was gallows humour. His life was in ruins: a bottomless debt, great marital difficulties and progressive alcoholism. But worst of all: his literary vein had dried up.

He was full of days, but still his friends missed him very much. The Danish poet Baggesen wrote the last and the lasting epigram for his friend:

> Gråt smelted hen i smil, når Wessels lune bød
> Og glædens smil forsvant i tårer ved hans død

It is close to an offence, but I have tried a translation:

> Wessel's wit melted weeping into laughter
> He left and smile became tears thereafter

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Thomas Bugge's Journal of a Voyage through Germany, Holland and England, 1777.

Kurt Møller Pedersen *

1 Travels to England in the 18th Century

England had a high reputation among scientists in the 18th Century. This was not least due to Sir Isaac Newton's scientific achievements that were known and admired everywhere on the Continent. This, however, is not to say that they were accepted. In France, Cartesian doctrines of science still flourished though more and more of the younger scientists were taken by the Newtonian system, e.g. Maupertuis and Clairaut. The results from their scientific expedition to Lapland were taken as a proof of the Newtonian world view and scientific notions. It was not only scientists who held a high esteem for England. Voltaire's being exiled to England from 1726 to 1729 made him an ardent admirer of all English science and the way scientific enterprise was organized. This became clear from his writings, *Lettres sur les Anglais* (1734), and *Éléments de la philosophie de Newton* (1738).

Scientific expeditions and travels by scientists were interrupted by the Seven Years War (1756–1763) between England and France and their respective allies Prussia and Austria. When the war ended, there seems to have been quite an interest in visiting England, particularly London, Greenwich, Oxford, and Cambridge. England had been closed for years, and it now became *à la mode* to travel there. The visitors were all well received by the English.[1] It was neither the defeat of the Cartesian world view and its replacement

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[1]Lalande 1980, 12.

by the Newtonian one, nor was it Voltaire's writings alone that made London attractive. England had a reputation for producing excellent scientific instruments. Maurice Daumas gave reasons for this:[2]

> "The English instrument makers benefited from abundant basic materials such as steel, brass, copper and tin, of consistently high quality and for certain work the English were for a time unsurpassed. John Bird was the best workman of his period for dividing scales on the limbs of instruments, and the most expert at constructing astronomical quadrants; instruments from his workshop had a universally high reputation, and for almost half a century were used in all observatories, practically to the exclusion of instruments by other makers."

It was of the uttermost importance for astronomers on the Continent to follow all improvements in scientific instrumentation.
Jérôme de Lalande was the first scientist who went there after the war had ended, from March 15 to June 10, 1763. Jean Bernoulli was there from the beginning of December 1768 until late April 1769. Thomas Bugge arrived in England on September 7 and left on November 10, 1777. There were many other visitors to England at that time, but I have chosen to mention these three travellers because they were all astronomers, and they kept a diary.[3] Jean Bernoulli was the only one of them to have his diary published, but not in an edited version. He had it printed just as it was kept. Lalande's diary was published many years later, first in an English translation[4] in 1923, and then in a French version[5] in 1980. Bugge's diary was published in a preliminary edition with an English translation[6] in 1997.

2 Lalande's travels in England, 1763

Lalande, Bernoulli, and Bugge visited almost the same places in England, but the style and content of their diaries are very different. For Lalande,

[2] Daumas 1972, 92.

[3] A Swedish astronomer, Bengt Ferner, travelled through Europe during the Seven Years War. He was also in London, and his diary contains several remarks on instruments in different observatories and collections of physical apparatus. See Ferner 1956.

[4] Green 1926.

[5] Lalande 1980. A preliminary version is in Monod-Cassidy 1967.

[6] Bugge 1997. A brief summary with extract including some of Bugges's drawings from the diary was published in Pedersen 1982.

the main objective was to re-establish connections with British scientists and scholars. His diary is therefore mainly a list of people he had met. Personal contacts were the most important part of his journey. There are many descriptions of dinner parties that he enjoyed with colleagues and other important people. Let me give a brief example of the style and content of Lalande's diary:

> At the museum, with Lord Maclesfield, at the Royal Society, dinner at the Mitre Tavern[7] close to Temple with Lord Maclesfield, the ambassador of Venice, Lord Willoughbi, the bishop of Clarence, Short, Morton, Birch, Ellicot, Watson, Maskelyne, etc. Ordinary, [the dinner] costs three shillings and a sol, but this time, because of the claret, that is the wine from Bordeaux, which costs five shillings a bottle, we had to pay four and a half shillings. Plum pudding, marque potinger, etc.

Lalande also wrote down small talk about other scientists: "Bradley was tough, jealous, avaricious, melancholic according to doctor Bevis." We do not find any professionel descriptions of observatories, instruments, and other scientific institutions, as we do in Bugge's diary.

3 Bernoulli's travels in England, 1768–69

Jean Bernoulli visited several observatories in Europe, and provides details of the many instruments he saw in his *Lettres astronomique*. He also reported about the research projects taking place at the various observatories by interwieving so to speak directors and scientists. In the fall of 1768 he was in Göttingen, Kassel, Frankfurt am Main, Mannheim, and from the beginning of December he was in London. During his visits to European observatories it became more and more obvious to Bernoulli that many of the fine instruments he saw came from London instrument makers. So it must have been with great expectations that he arrived in the English capital, and he was most certainly not disappointed. Not many scientists have been more outspoken in their enthusiasm for their trade than Bernoulli when he came to London:[8]

[7]Later, Bugge also had dinner with members of the Royal Society. See Bugge 1997, 191: "September second I was introduced into the Royal Society Club in Mitre Tavern by Dr. Solander. The Society has dinner every Thursday, whereas a non-member is allowed to come only every fortnight."

[8]Bernoulli 1771, 63–64: "je vous ferai part de la surprise agréable où est jeté un Astronome en parcourant les rues de cette Capitale. Vous avés sûrement ouï parler de la

> I am pleased to share with you the agreeable surprise which meets an astronomer when he walks in the streets of this capital. You have certainly heard about the richness and vividness of the boutiques in London, but I doubt you would imagine how much astronomy contributes to the beauty of this spectacle. London has a great number of opticians, and the shops of these artists are filled with telescopes, octants, and all these instruments are well-kept, and they flatter the eye just as much as the reflections they produce.

Bernoulli does not give any detailed descriptions of the instruments, but he does evaluate the instruments and their makers. We are told that the young Dollond is not so well versed in mathematics as his father, though he maintains a high reputation for his telescopes with acromatic lenses. When it comes to dividing the arcs, he is helped by his brother-in-law, Mr. Ramsden, "who is considered one of the best instrument makers in London."[9] Also we learn that "Mr. Nairne is known as a man who produces very good telescopes and other instruments."[10] Bernoulli spent three days at Greenwich giving a rather detailed description of the buildings and instruments. He was shown around by Mr. Baily, assistant to the astronomer royal, Dr. Maskelyne. From there we find the few drawings he included in his diary. He also visited Oxford and Cambridge:[11] "Literature is primarily studied at Oxford, and looking for geometers, astronomers, physicists, &c. one must go to Cambridge, and there one can convince oneself that science is considered to be of importance for the young English student's studies." Bernoulli is in no doubt who is the best instrument maker in England:[12]

> The skilled people who are most distinguished for their exactness in dividing instruments are Bird, Sisson, and Ramsden,[13] but although

richesse & de l'éclat des boutiques de Londres, mais je doute que vous vous représentiés combien l'Astronomie contribue à la beauté du spectacle: Londres a un grand nombre d'Opticiens; les Magazins de ces artistes sont remplis de Télescopes, de Lunettes, d'Octans &c. Tous ces instrumens, rangés & tenus proprement, flattent l'oeil autant qu'ils imposent par les réflexions auxquelles ils donnent lieu."

[9]Bernoulli 1771, 69.

[10]Ibid. 70.

[11]Ibid, 115–116.

[12]Ibid. 126: "Les habiles gens qui se distinguent le plus pour l'exactitude des instrumens à division sont Mrs. Bird, Sisson & Ramsden, mais quoique ces trois rivaux se disputent la palme, je crois, en vous les nommant, leur avoir en même tems assigné le rang dans le quel ils sont mis par le plus grand nombre de ceux qui connoissent leurs ouvrages."

[13]The imstrument makers mentioned are: John Bird (1709–76), Jeremiah Sisson (1720?–

the three rivals contest the palm, I think, having mentioned them to you, that at the same time I have allocated their rank according to those who best know their work.

4 Bugge's travels in England, 1777

"Odense poorly built ... Assens is a poor town ... Rendsburg and Slesvig are very beautiful towns." The new director of the observatory in Copenhagen, Thomas Bugge, described in such simple terms some of the cities and towns he passed through when in 1777 he set out for a study tour to Germany, Holland, and England. Bugge kept a diary and it is full of descriptions of all kinds of episodes, incidents, and experiences, written in brief statements and with a lot of drawings. He left Copenhagen on August 2nd and his first longish stop was in Hamburg, where Bugge observed:[14]

> In the evenings the citizens are occupying the ramparts. Everywhere in Hamburg luxuriousness of clothing, eating, and game-playing has increased enormously. Moreover, there is little politeness and kindness to foreigners, and these are constantly pestered by hucksters, tailors, and Jews, all of them waiting to gain a profit.

Bugge also went to the theater, which he described as follows:[15]

> The playhouse is situated in a miserable corner and has only one exit. The indoor decorations are tasteless. This evening's performance was *zu gut ist zu gut*, translated from the English. The director Schrøder played very well. And a certain Brukman played the part of Lofstead Bille fairly well. The females were rather poor. The ballet was Vauxhall; the representation was good; but the composition and the dancing were poor; the best dancers were the director's wife and a Frenchman.

At that time also young people sometimes got a little crazy:[16]

> The last evening I went to the Vauxhall. It is a small garden which is fairly well illuminated. There were music and singing. The audience was not numerous, but beautiful. The entertainment was poor. This evening some young Englishmen from the Commercial School behaved rather badly, pushing other people etc.

83), and Jesse Ramsden (1735–1800).

[14]Bugge 1997, 13.

[15]Bugge 1997, 9.

[16]Bugge 1997, 13.

From Hamburg he continued via Bremen, Oldenburg, Leer, Nienhrants, Windschoten, Groningen, Lemmer, Amsterdam, Leiden, Den Haag, Delft, Rotterdam to London where he arrived on September 7th, 1777.

One of the first institutions he visited was the Foundling Hospital, established in 1739 "for the maintenance and education of exposed and deserted young children ... to prevent the frequent murders of poor miserable children at their birth, and to suppress the inhuman custom of exposing newborn infants to perish in the streets."[17] When Bugge visited the Hospital, we are told there were "120 of each sex. It was said that about the same numbers were in the country for their health."[18] The children's state of mind could be improved, so it was thought, by exposing them to the more obscure sides of social life, and thus giving the children a dislike for the vulgar life. Whether this was the official strategy or not I do not know, only that the children every day had a view of a characteristic picture by Hogarth:[19]

> In a western room is a painting by Hogarth; it is called Hogarth's Master Piece. It represents the English Guard Regiment's march against the Scotch rebels. Two women, one with a child and another who is pregnant quarrel over a soldier. One soldier is drunk and falls on the ground; one of his companions wants to give him some water, but he reaches for a glass of aquavit which a sales apprentice offers him. Another soldier overturns a girl's milk pail and lets it run into his hat. A chimney sweeper's boy holds his black skullcap underneath. Another soldier fingers a girl underneath her skirts, as she climbs up in order to watch two men boxing.

Thomas Bugge did not set out on his journey to study art and sociology. He wanted primarily to study observatories and their instruments so that, after his return, he could initiate a restoration of the observatory at the Round Tower in Copenhagen. The expectations of the outcome of his journey were great. Danish astronomy should again be brought back into the front line, as the heritage from Tycho, Longomontanus, Rømer, and Horrebow demanded. These demands to Bugge were not rooted in his theological degree from the university of Copenhagen, but in his background as a mathematician,

[17]The Encyclopædia Britannica, Thirteenth Edition, 1926, volume 9, 747. Entry: Foundling Hospital.

[18]Bugge 1997, 119.

[19]Bugge 1997, 121. I have found a print resembling this description, see Fig. 1.

Figure 1: In 1745 a regular and volunteer force encamped at Finchley, 7 miles from St. Paul's Cathedral to resist the Pretender. The gathering of this force inspired Hogarth's famous picture "The March to Finchley". From Engravings by Hogarth, ed. Sean Shesgreen, Dover Publications, New York 1973.

having been supervised by Professor Hee, and since 1759 as an observer at the Round Tower. From 1762 to 1777 he was director of the surveying of Denmark. More important, however, was his appointment as professor of mathematics, and director of the observatory in 1777, the year when he travelled to England.

The new director immediately saw that the observatory had to be modernized and better equipped. It is quite interesting to observe that almost all the new instruments that Bugge procured after his return from England were either bought in England or were constructed by the local instrument maker, Johannes Ahl, in accordance with Bugge's instructions which originated in what he saw in England. In what follows I will show how some of the new instruments and devices at the Round Tower can be traced back to drawings and descriptions found in Bugge's diary.

The diary has 184 pages and is full of drawings of the many instruments he studied. It is a very important historical document allowing historians of astronomy to register instruments that later disappeared. It is not only the drawings that are of importance. Bugge's descriptions of what he studied also allow a better understanding of instruments still in existence in British museums. He visited the observatories in London, Oxford, and Cambridge. Just as important, however, were the contacts he established with leading English astronomers, and he was introduced to the Royal Society and the Royal Agricultural Society. In what follows I shall concentrate on such descriptions and drawings as pertain to instruments and devices later to be found in the observatory at the Round Tower in Copenhagen.

5 Illumination of wires in transit instruments

On September 23rd Bugge visited Richmond, about 14 kilometers southwest of London where the king had his summer residence. In a corner of the park was an observatory:[20]

> I went to Richmond where I got the opportunity to make the acquaintance of Doctor de Membray[21] and his son-in-law Mr. Rigaud. They were both kind enough to take me up to the observatory which the king has erected for his own pleasure[22]. On the bottom floor or in the

[20] Bugge 1997, 147.

[21] Stephen Demainbray (1710–1782).

[22] King George III had become very interested in the transit of Venus in 1769 and built his own private observatory at Richmond (now Kew Observatory) where he could observe the transit himself. The observatory was designed by Sir William Chambers and was ready by the time of transit in June 1769. Stephen Demainbray was the observatory's first superintendent. When he died, he was succeeded by his son, also called Stephen Demainbray. See Morton and Wess 1993, 29 and 117.

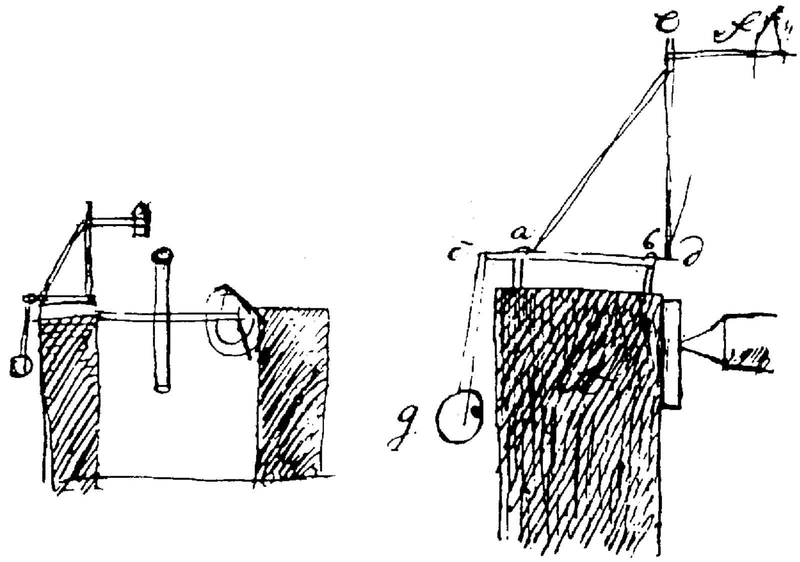

Figure 2: The transit instrument at the observatory in Richmond.

> basement there are mathematical workshops. On the first floor there are several rooms. a) one housing the transit instrument; the telescope is 5 feet, the axis $3\frac{1}{2}$. At the top of the solid block A was a device for the lantern [see Fig. 2]. a and b are two stationary brass sticks with holes, through which the triangle *cde* can be moved. At the end F the lantern illuminating the filaments is placed. At the other end is placed a bar *cg* with a counter-weight, so that the lantern remains in the wanted position.

Bugge gives us here a description of a transit instrument that can move around a horizontal axis in the north-south meridian. Inside the telescope is a thin horizontal and several vertical wires, the middle of which determines the meridian. These wires serve to determine the coordinates of the stars.

The method requires that the wires can be seen, and therefore they must be illuminated by a lamp, and this is what interested Bugge. He carefully tells how this is done at the observatory in Richmond. Whenever the telescope is rotated the lamp must follow and that mechanism Bugge carefully drew in his diary. But how does the light from the lamp enter the telescope? If the light from the lamp should enter the telescope properly, the lamp should be placed in front of the telescope, in which case the stars could no longer be

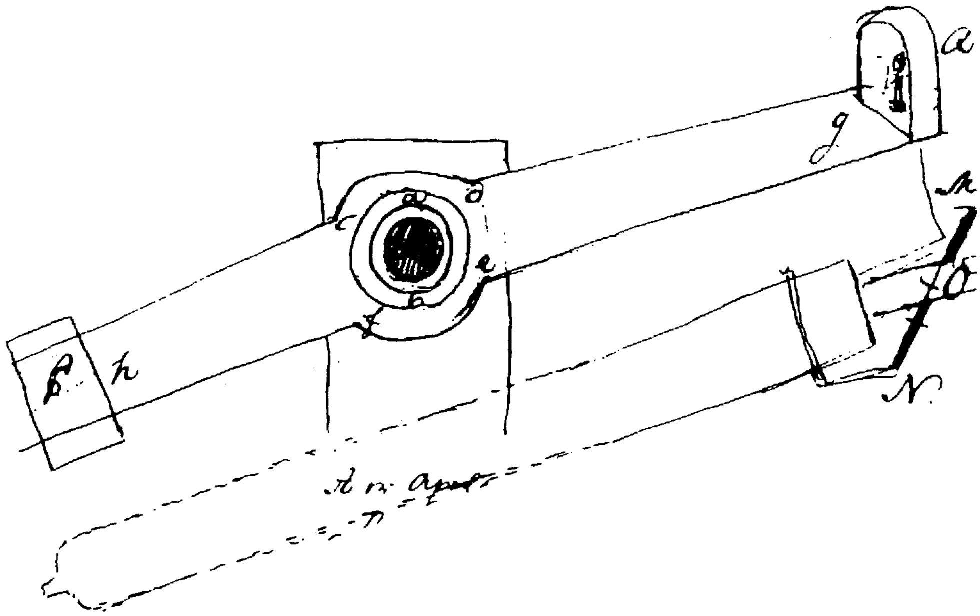

Figure 3: A transit instrument in Oxford.

seen. What they did at Richmond, as can be seen from Fig. 2, was to place the lamp close to the axis of the telescope so that sufficient light entered the telescope. That was, indeed, a rather simple solution to the problem of illuminating the wires. In general, the collection of instruments at Richmond was not of the highest quality; they were made by "many people, some of whom were not highly skilled."[23]

However, in Oxford Bugge found another, more ingenious device for illuminating the wires:[24]

> *A* is the axis of the transit instrument [see Fig. 3]. *ab* is a circle of mahogany around the axis attached to the pier without being connected to the axis. Round it is another circle *cdef*, connected to the piece *hg* which is of about the same length as the telescope. *Q* is the lamp, illuminating the circular piece of polished brass *MN* with the requisite aperture *O*. At *P* a weight counterbalances the lamp. When the telescope has been adjusted to focus upon the star, the observer,

[23]Morton and Wess 1993, 123–24.

[24]Bugge 1997, 225.

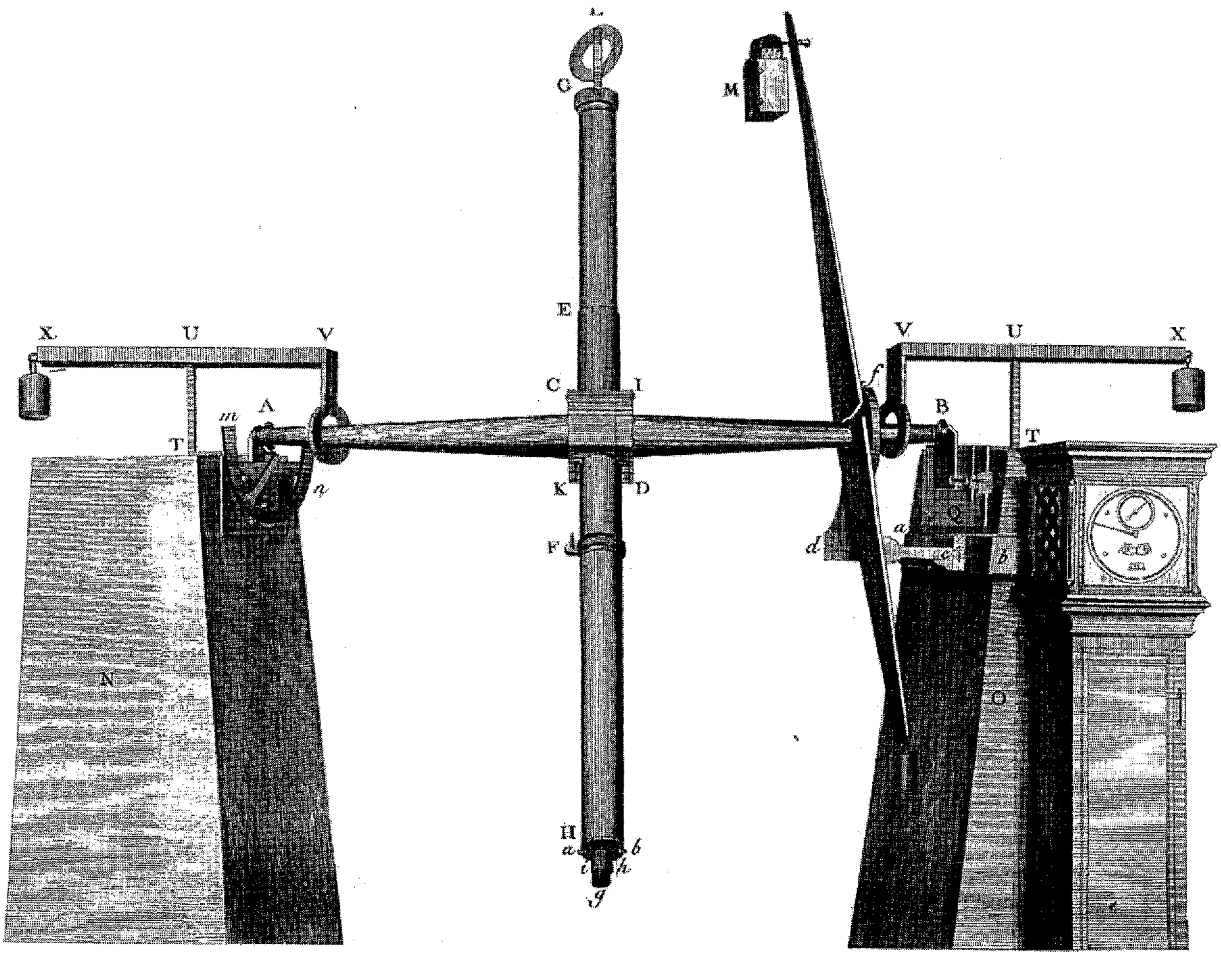

Figure 4: Ahl's transit instrument made for the Round Tower in Copenhagen. From Thomas Bugge: Observationes astronomicæ annis 1781, 1782 & 1783. Hauniæ 1784.

> with one hand, moves the piece *hg* and the lamp *Q* up and down until he finds that the wires are well illuminated.

It is easier to see how the device worked from a drawing of the transit instrument that Ahl made at Bugge's request and in accordance with his instructions [see Fig. 4]. The lamp is suspended from a rod that can move around the horizontal axis of the telescope. Bugge did not use the rather complicated suspension he saw while at Richmond, but the simpler device

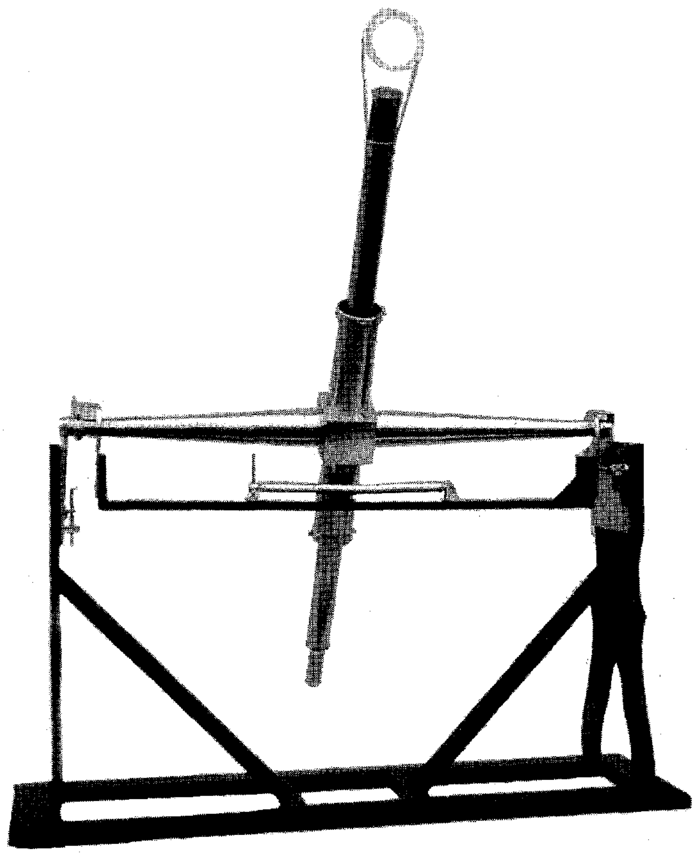

Figure 5: A transit instrument from the Geodetic Department in Copenhagen, now in the Steno Museum, Aarhus. Photo: Hanne Teglhus.

from Oxford. The light from the lantern is reflected in a brass mirror that has a hole in the middle such that the light from the star and the lantern can enter the telescope. The same device illuminating the wires is found on a transit instrument from the Geodetic Department in Copenhagen, now at the Steno Museum in Aarhus shown in Fig. 5. From the drawing of Ahl's instrument we can see that two counterweights are suspended at each extremity of the horizontal axis of the telescope, a device Bugge also saw at Oxford:[25]

[25] Bugge 1997, 221.

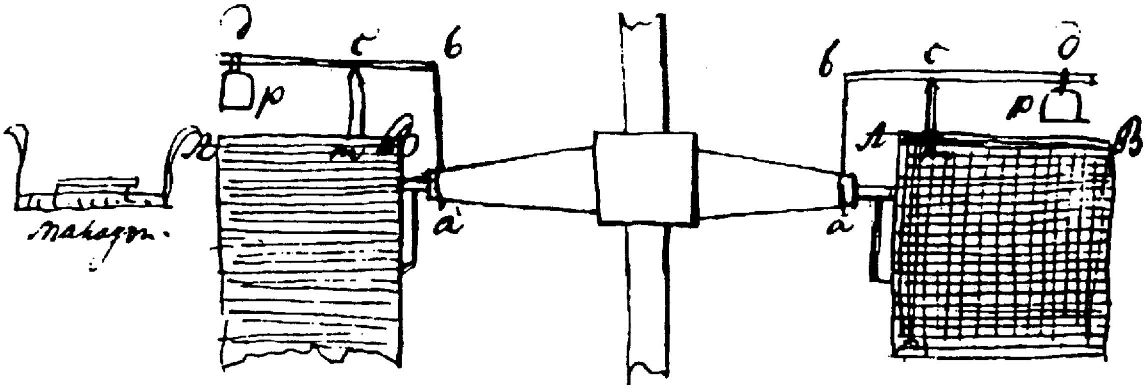

Figure 6: A transit instrument with counterweights from Oxford.

> The whole weight of the instrument was more than 100 pounds, and in order to relieve the weight from the pivots, levers (*bcd*) are placed at its extremities [see Fig. 6]. Their centres of suspension *c* rest on the iron bars *AB*. *bd* and *ba* are made of mahogany. The latter (*ba*) is shown in figure 2 [here Fig. 6]. The weight *p* counterbalances half of the weight. With one finger I could lift the instrument out of its bearings at *a*.

The director of the observatory at Oxford, Dr. Hornsby[26], was proud to guide Bugge through the buildings. The observatory was new and modern, in fact it was only completed the year after Bugge's visit. Hornsby had asked the trustees for astronomical instruments "to be made by the best instrument maker of the time, John Bird"[27], and it was accepted. The observatory was "one of the best equipped of its time."[28] Bugge made a lot of notes and drawings in his diary. Immediately after his return to Copenhagen Bugge began reconstructing the observatory, and it was completed in 1780.[29] It is difficult to relate details in the new construction to particular constructions

[26]Thomas Hornsby (1733–1810) is remembered for his active part in the foundation of the Radcliffe Observatory in Oxford of which he became the first director. The buildings were completed in 1778.

[27]North 1972. That Bird was the best instrument maker is a statement in accordance with Bernoulli's observation as I quoted in section 3. The observatory's mural quadrant was one of the best instruments produced by Bird, see Bernoulli 1771, 116.

[28]Daumas 1972, 124.

[29]Bugge 1784, xxvii and Tab. I. A reconstruction of Bugge's observatory is in Gyldenkerne & Darnell 1990, vol. 2, 214–215.

in England, but we can see from Ahl's instrument (Fig. 4) that the axis was counterbalanced in the same way as in Oxford (See Fig. 6). We do not know anything about the instrument shown on Fig. 5, but it might well be that it is an instrument made at Bugge's request, since it has a circular, reflecting ring in front of the telescope, as described by Bugge and shown in Fig. 3. Moreover, the spirit level is suspended from the axis in a way similar to the ones shown in Figs. 6 and 7. Bugge was very impressed by the observatory in Oxford and its equipment:[30]

> Not without regret did I leave Oxford Observatory which is no doubt the best in Europe, both as regards the arrangement and instruments. Professor Hornsby most courteously assured me of his friendship and correspondance.

6 A spirit level

Already in September, Bugge visited watchmaker Cumming[31] where he saw a spirit level:[32]

> When the spirit level *ab* [see Fig. 7] had been adjusted by proper reversal in the horizontal position of the meridian circle *cd*, Mr. Cumming noticed that the bubble did not remain in the centre when the telescope was elevated to 20, 30, 40, etc. From this he concluded that the axis was not on the same level or on a level parallel with the length of the spirit level. In order to change this he had fitted a screw *gh*, so that the entire spirit level can be moved to both sides.
> The adjustment of this screw must then be continued until the bubble remains at its marks in all positions of the meridian circle.

Before Bugge went to Oxford he had paid John Russell[33] a visit and he saw him again after he had returned from Oxford. Bugge noticed a spirit level in his possession and made a drawing of it and described it as follows:[34]

[30]Bugge 1997, 261.

[31]Alexander Cumming (1733–1814).

[32]Bugge 1997, 169.

[33]John Russell (1745–1806) was a famous painter who was also interested in astronomy. He drew an exceedingly accurate map of the moon. Bugge's description of many of Russell's astronomical instruments is of great importance, since they had never before been recorded.

[34]Bugge 1997, 275.

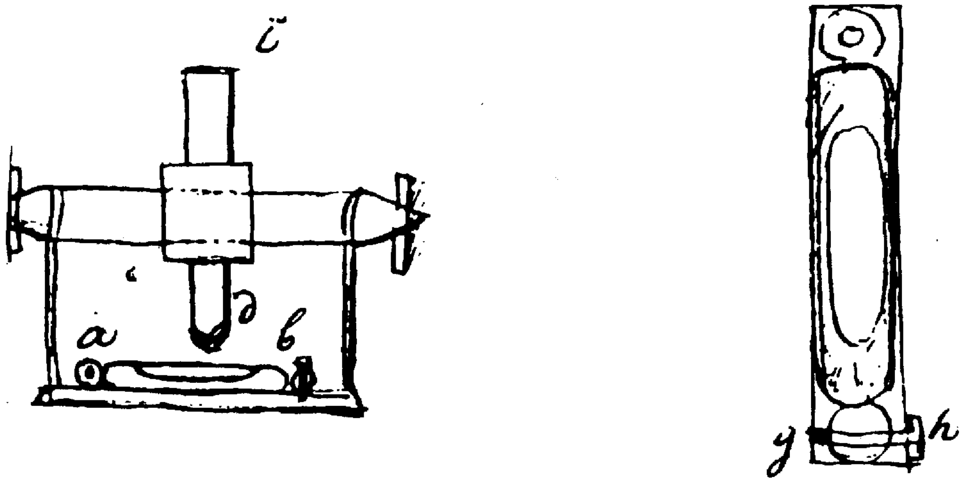

Figure 7: A transit instrument with spirit level as seen in watchmaker Cumming's shop in London.

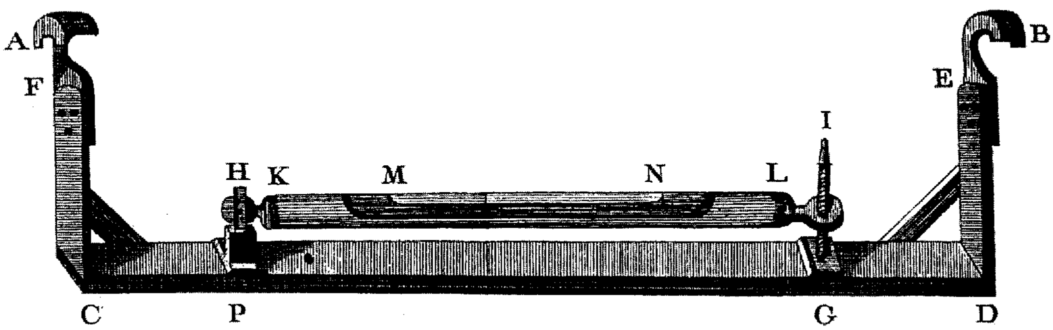

Figure 8: Ahl's spirit level made in Copenhagen. From Thomas Bugge: Observationes astronomicæ annis 1781, 1782 & 1783, Hauniæ 1784.

> He [Russell] also had an excellent spirit level, 6 inches long, made by Bird, and constructed in the same way as the Centre or Trial Telescope described by Smith in his Optics. He places the spirit level on the quadrangular plates as shown in the following figure [see Fig. 9].

In Copenhagen Bugge had Ahl construct a spirit level in accordance with the ideas he had learned about in England, as can be seen from the drawing of Ahl's instrument [see Fig. 8].

7 Conclusion

It is clear from the diary that Bugge learned much about observatories and the new equipment in their possession. He brought back new ideas that eventually were used in constructing new instruments in Copenhagen. It is, however, important to make clear that Bugge never went into details when it concerned precision instruments and their working. Perhaps, he was not told about them. It was kept as a secret how the more subtle details were produced and manufactured, as Dan C. Christensen made it clear in his paper about spying on scientific instruments.[35] Furthermore, as Christensen has also pointed out, it would have been impossible to construct precision instruments in Copenhagen because of lack of knowledge and skill. After Bugge's return, Bidstrup was sent to London to learn the trade, and his journey to and stay in England were recommended by Bugge. From his stay in England Bugge had learned, I believe, the importance of having skilled instrument makers in Copenhagen, if the observatory was again to be recognized as a leading one in Europe. The observatory did not obtain that position while Bugge was its director. He was not able to supply the funds for it, and Bidstrup died before he could begin his trade. Bugge's diary is a much more valuable document than those of Lalande and Bernoulli. It contains a more detailed description of how the observatories were equipped. Today his diary is recognized as a major source of English astronomy and scientific instrumentation. On the 300th anniversary of Greenwich Observatory in 1975 Bugge's diary returned to England to be exhibited for some weeks in Greenwich where it attracted much attention from the hundreds of historians of science and astronomy gathered to celebrate this important event.

Acknowledgment

Bugge's diary is in The Royal Library in Copenhagen (Ny kgl. Saml. 3777 e4o) and I am indebted to the Library for permission to reproduce figs. 2,3,6,7 and 9.

[35] Christensen 1993 and 1999.

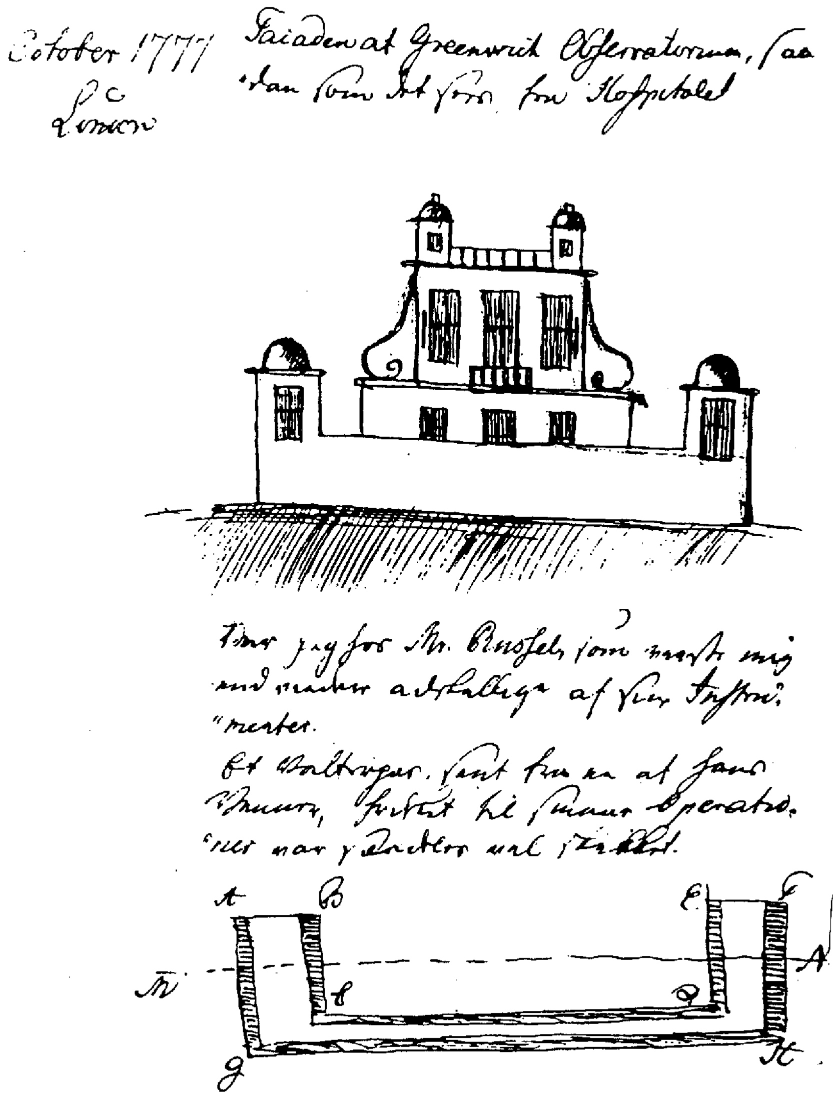

Figure 9: Bugge's drawing of Greenwich Observatory, October 1777. Text: *Facaden at Greenwich Observatorium, saadan som det sees fra Hospitalet. Var jeg hos Mr. Russel, som viiste mig end viidere adskellige af sine Instrumenter. Et watterpas, sent fra en af hans venner, hvilket til smaae operationer var særdeles vel skikket.* Translation of the text: *The front of Greenwich Observatory, seen from the Hospital. I visited Mr. Russel who showed me several of his instruments. A spirit level sent from one of his friends. It is very suitable for smaller operations.*

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English Instrument Makers Observed by Predatory Danes

Dan Ch. Christensen *

The title of this paper is hinting at the fact that for a middle sized monarchy like Denmark-Norway scientific instruments were a prime requisite for the progress of scientific research, i.e. astronomy, experimental physics, geodesy, as well as navigation. For shortage of domestic producers the obvious resource to turn to would be the instrument makers in London renowned for their leading position. In theory, there were four methods of transferring technology:

1. buying instruments abroad,
2. enticing skilled artisans to emigrate and work in Denmark,
3. spying on instrument makers abroad, and
4. sending talented artisans abroad for training.

The first method simply consisted in buying instruments abroad. Professional astronomers like Thomas Bugge[1] or amateur natural philosophers like A.W. Hauch[2] and the Reventlow brothers[3] went to the boutiques of Ramsden, Nairne & Blunt, and Adams to buy the instruments they adored. These Danes were on their grand tours or study tours and usually they were law-abiding travellers. But purchasing instruments was a simple activity that meant little for promoting a national production.

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[1] *Thomas Bugge, Journal of a Voyage through Holland and England, 1777.* Ed. by Kurt Møller Pedersen, History of Science department, University of Aarhus, 1997. See also Kurt Møller Pedersen's paper in this volume.

[2] Hemming Andersen, *En videnskabsmand af rang, A.W. Hauch, 1755–1838*, Århus.

[3] Unpublished travel diary by C.D.F. Reventlow, 1769–1770, The Reventlow Museum, Pederstrup, Denmark.

Secondly, the government might send out agents to try to entice skilled workers to emigrate to Copenhagen. In fact, the only instrument maker of competence in Denmark before 1800 was a Swede, Johann Ahl[4], the journeyman of Daniel Ekström, Stockholm, who left Sweden possibly to rid himself of a personal debt. Bugge, who had seen the most advanced instruments on his journey to Paris and London during 1777, had Ahl imitate crafty innovations in his Copenhagen workshop on the basis of his sketches.[5]

This strategy, however, if employed in England, would violate the tool acts which illegitimated the enticement of skilled artisans to go abroad. The objective, of course, was to maintain a leading position on the world market. There are many examples of British artisans entering into Danish-Norwegian service and setting up workshops in the twin-monarchy with government support.[6] The glass, machine tool, iron foundry, and agricultural implement industries are typical examples, but to my knowledge no British instrument makers immigrated here.

The third strategy was industrial espionage. The objective of the spy would be to make descriptions and drawings or to get hold of models or prototypes of advanced tools or machines from leading manufacturers. Once this had been achieved, however, another problem arose: how to smuggle the goods out of Britain? Spies belong to the historian's most appreciated agents. Not only because the historian shares everybody's fascination with spy stories, but because the spy is the eminent supplier of reliable evidence for the simple reason that in order for his intelligence to be useful for plagiarism it had to be thoroughly understood. Hence his notebooks are usually superior in quality to the information supplied by manufacturers who took an interest in shrouding their trade in mystery.

In connection with espionage it deserves mentioning that the tool acts also illegitimated the exportation of core machinery of production.[7] The objec-

[4]See Olov Amelin's paper in this anthology.

[5]See Kurt Møller Pedersen's paper in this anthology.

[6][illegible]

tive was to protect outstanding inventions from falling into foreign hands, just as patent bills protected the inventor from being exploited by domestic plagiarism. It should furthermore be noticed that British manufacturers were divided according to opposed interests. As we shall see some manufacturers like for instance Wedgwood would put pressure on Parliament to extend and tighten the tool acts, whereas a group of members of *The Society for the Encouragement of Science, Agriculture, &Commerce*, London, demonstrated their idealistic belief in a universal brotherhood of man by publishing patent drawings and descriptions in an international journal titled *The Repertory of Arts, Manufactures, and Agriculture.* The paradox was that in order to have a patent legally recognised a description and sometimes a drawing of the patented artefact had to be submitted to the court; this procedure made the patent public knowledge. Since the patent was valid in Britain, but not abroad, there was no legal obstacle to use this knowledge to plagiarise the innovation abroad. An illustrative example would be Bryan Donkin's paper-making machine from 1808. An issue of *The Repertory* containing patent drawings of this machine appeared in Denmark and a skilled artisan, Ole Winstrup, made efforts to imitate it.[8]

There were a number of Danish-Norwegian civil servants spying on British manufacturers between 1760–1807. 15 spies have been identified, many of them professors and free masons.[9]

The most eminent Scandinavian spy, no doubt, was Professor J.M. Ljungberg, a Swede by birth, operating on behalf of the Danish government and Count Schimmelmann, Chancellor of the Exchequer and industrialist in a wide range of manufacture. He had several sojourns in England in the last quarter of the eighteenth century procuring intelligence from a good many inventors behind the Industrial Revolution, e.g. Boulton, Watt, Chippendale, Wilkinson, Arkwright, Bramah, Wedgwood, Garbett, Smeaton, Ramsden, and Troughton. In fact, during 1788–89 he stayed for more than a year in Birmingham getting well acquainted with Boulton, who employed him as an agent to sell furnishings to Chippendale and issued letters of recommendation to manufacturers using his tools and steam engines, like Wedgwood and Wilkinson. The intelligence he collected was meticulously taken down

1998.

[8] *The Repertory of Arts, Manufactures, & Agriculture*, vol. 13, 2nd series, Pl.ix, London 1808. The Danish attempt is described in DMP (see note 6), p. 460–462.

[9] cf. note 6.

in his notebooks[10] and he acquired a large quantity of tools (a consignment of four boxes weighing almost half a ton), including some 'clock maker's tools', which was a euphemism for precision tools for instrument making that were blacklisted by the tool acts.

Now, just as the making of scientific instruments demands its skills and experience, so does the noble business of espionage. And just as Ljungberg knew that industrial secrecy made spying a high-risk enterprise which challenged the ingenuity of the predacious agent, so he was himself utterly reluctant to disclose his methods. His notebooks are extremely scant with information on how he contrived his business. However, his spying on Ramsden's tube drawing machine reveals an interesting case. As Ljungberg well knew, Ramsden was very secretive. At first he managed to get one of Ramsden's workers to open his mouth and took down his oral report as follows:

> Instrument makers use brass tubes for telescoping. The tubes must be exceedingly accurate in order to be drawn out and pushed in smoothly. Mr. Ramsden and Mr. Wright have recently drawn patents for plated telescopes. This is done in a certain way similar to the drawing of a wire and the process is as follows:
> The brass sheet for the tubes is made twice as thick as it is going to be. It is then hammered and soldered with the diameter it is going to retain; the soldering is brass and spianter (***what this is I'm not fully aware***). One then strikes it on to a steel mandrel which is perfectly cylindrical and indeed very accurately turned and well polished. (added marginal note: ***I think that*** the mandrel is in one piece and that the split pin only enters at the neck cc to fix the mandrel). This mandrel has a neck in cc into which one beats firmly the edge of the tube. Two halves of a certain pair of thongs fit into this neck. They are pressed together and afterwards they are fixed with two split pins. The mandrel is part of a drawing machine like the usual ones, and ***if I understood it correctly*** it is operated by means of a chain, similar to a clock's chain, i.e. a crank handle and a gear. But this operation is vertical, not horizontal as usually. A steel disc with a round hole of appropriate size is fixed in the groove, and the brass tube is drawn

[10] J.M. Ljungberg's notebook, Kungl. Myntverkets arkiv, Varia I d, Riksarkivet, Stockholm. The notes amount to 273 pages, written in German apart from a few pages in English, richly illustrated. The entry on Ramsden's tube drawing machine is from June 1789.

> between this steel disc and the mandrel, so that it stretches and becomes smooth and round. This process is repeated several times; and usually, at least as far as the plated tubes are concerned, they come out twice as long as they entered.
> The cost of this machine is £200.
> ***This intelligence stems from an oral account only, because the machine cannot be observed.*** [11]

Now, one might think that a tube drawing machine is a comparatively simple device. But reconstructing one on the basis of this oral account would hardly be possible, since it contains too much doubt, possible misunderstandings, and lacks measurements. Furthermore, it leaves no clue as to the construction either of the transmission from handle to chain or of the disc-hole through which the tube is drawn. So, upon second thoughts Ljungberg must have found his account rather flimsy and requested a drawing stating all the precise measurements and a detailed knowledge of how to operate it and how to tackle its most frequent malfunctions. And here it is:

> AAB is a stand of wood in which a groove B is cut out, 5 inches deep, 5 inches wide. The inside of the upper half is covered by iron rulers aa on two sides, and where the stand ends, bent in b and as they ascend they also widen and serve as a frame, c, for pivots for the wheels, CDEF. In order to obtain more strength with less mass, rulers of 3/8" are forged on the edge upwards vertically and similarly horizontally.
> C is a cylinder of wood sitting together with an iron cog wheel, F, on a shaft having its pivots lying in the stand cc. Around this cylinder rolls an iron chain, h k l, which in l is bolted very firmly on the cylinder and descends freely in the groove edged by the rulers, ab. The form and property of the chain is apparent in the drawing, and at its lower end is a shackle, h, of strong iron.
> D is a large wheel of wood, nearly 3' in diameter, and it serves both as a drive for the transmission and as a flywheel. It has its shaft in the drive d, which gears into the iron cogwheel, E. On the shaft of the wheel E there is also the drive, f, which gears into the wheel F, and turns the cylinder C to which it is fixed so that the force on C gets very strong because the drives are as small as the requirements of strength permit.
> Fig. 3 shows the disc which is abt. 4 1/2" square and 3/4-1" thick. One section of the inner ring is conical, nn, whereas the other, mm, is cylindrical, and it is this latter section that works on the tube. The

[11] Ibid., p. 147, translation and fat italics are mine.

width of section mm actually touching the tube is only small, abt. 1/15-1/8". The disc consists of two parts: the square G and the ring mn,mn. The former is made of iron, only the latter is of steel. This ring is turned and soldered with brass on the iron G. Then one glows the whole disc and puts it into water by which the ring mn obtains the appropriate level. One still needs to polish it afterwards.
Fig. 5 is a steel mandrel that is turned perfectly round and smooth. At both ends, rs, it is turned thinner and is pierced with two oblong holes, p and q. These serve to fix to the shackles tt by putting the pin, u, through all three holes. The mandrel H is made of steel and strongly heated, then turned and well polished. One has as many mandrels as one wants to make tubes of different diameters, and so with the discs, G. The pin is quite easy to put in and out. The machine is fixed to the floor and to the wall to make it stable.

The machine is used as follows:
The brass for the tubes is rolled sheet. One cuts it in strips of appropriate size and hammers it to the shape of a tube on a long steel cylinder which is only fixed at one end. When the tube is appropriately large and round it is hard-soldered by brass and spianter which makes the brass relatively more fluid. After the soldering one hammers it on a polished cylindrical bar the one end of which one puts into the hole of a round vertical wooden beam to fix it. This rod appeared slightly conical to me, and on this one hammers the tube perfectly round, and the soldered places thin and even. The bar is as thick as the inner diameter of the tube, 4 to 5' long and while it was beaten it lay, presumably, askew relative to the horizon, abt. 30°. I suppose that he turns the tubes quite often.
One puts the tube being 1 1/4" thick and 5" long on the steel mandrel H, so that the edge can be folded 1" or 3/4" wide over the neck, r. One beats this edge with a wooden hammer to fix the tube firmly. I think the cylinder was probably tallowed beforehand.

One then took the shackle t and put it through the shackle of the chain, h, in the machine, kept the neck H r of the mandrel H in the shackle t, so that the holes fitted into one another and inserted the split pin u. Before this, however, the upper end of the tube had been tallowed and a disc like G with a hole slightly narrower than the diameter of the tube was put on top of the neck r. Obviously, the chain had been lowered by turning the wheel D so that the neck r was in the lower part of the groove B, where the disc G has sufficient room, because higher up between the rulers aa the distance is smaller than the width

[12] Ibid., p. 149.

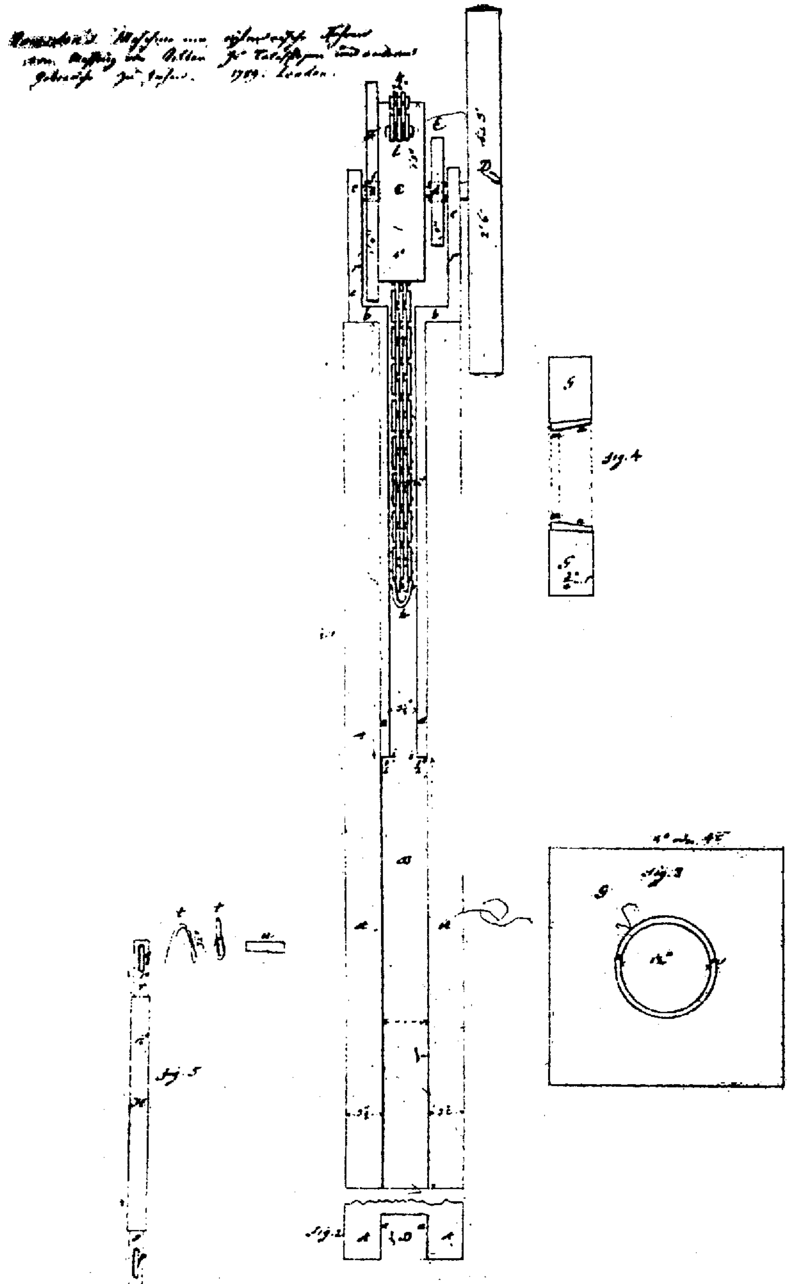

Figure 1: Ill. Ramsden's tube drawing machine [12]

> of the disc G.
>
> By turning the chain upwards the mandrel H ascends together with its fixed tube and the disc G. Only the disc eventually hits the section ii between the rulers and is stopped while the mandrel with the tube continues to move. In this way the tube is drawn while stretching considerably. When it is through one puts the pin out, takes the cylinder from the shackle and removes the disc and inserts another one yet slightly more narrow; each time one must be careful to tallow the upper part of the tube and when the disc is stopped at ii, direct the mandrel H in the vertical position manually. In this way the tube passes 4 or 5 rings when, finally, it has become twice as long as it was in the beginning and so even, smooth and round as if it had been turned on a lathe with the utmost care.
> Mr. Ramsden is said to have had this machine for ten years.
>
> Normally, the tube is fixed very firmly to the cylinder or the mandrel making it quite laborious to remove it. This is done in the following way: One takes the mandrel out, turns it around and puts on it a disc that fits the mandrel quite accurately, but does not let the tube slip through. One hangs the mandrel on the shackle and turns upwards again, so that the ring removes the tube, which is not possible without employing considerable force. However, in case the upper end of the mandrel rises very high in the machine, so that the space is insufficient, a loose wooden frame, three-sided and about 1' long, is available to be placed under the section ii in order to extend the narrow groove; and the disc G hits this frame like earlier it hit the section ii. The drawing shows the chain too long to enable one to see how it is fixed to the cylinder C. Obviously, it is no longer than the periphery of the cylinder C.[13]

Now, throughout the historiography of technology there has been a lot of fuss about internalism which has been loathed as 'nuts & bolts' history — a despicable hobby-activity usually practised by retired engineers. Good history of technology should focus on the context and hence it can be researched and written by ordinary historians unfamiliar with the intricacies of the technological artefact itself. The case of the tube drawing machine shows, however, that unless we pay attention to the very function of tech-

[13] Ibid., pp. 148–152.

nology it is hard to appreciate fully the task of technology transfer. In other words: an internalist approach is not a superfluous alternative to an externalist approach, but a necessary and indeed complementary asset to the contextual history of technology.

The first and the second piece of intelligence collected by Ljungberg are worlds apart, and nobody was more aware of the importance of exactitude for successful technology transfer than Ljungberg himself. As far as I know this drawing and description of a tube drawing machine is the only existing piece of evidence illuminating a basic technology of the production of scientific instruments left to the historian. Furthermore, the step from the oral to the written account marks two stages in espionage methodology. It must have demanded exquisite gifts of persuasion to entice one of Ramsden's workers to reveal the patent of the tube drawing machine. But when we learn how Ramsden treated his workers we begin to understand that Ljungberg must have spent quite a sum to bribe his informant to obtain sketch and measurements as well. Here is what he reports from inside the walls of Ramsden's workshop.[14]

> The ordinary workers who file, turn, plane, etc. earn from 18 to 21 s. per week of 6 days. [The foremen earn a lot more according to their skills]. They work daily 12 hours, from morning at 6 o'clock till 8.30 p.m., since they have 1 hour from 1 till 2 p.m. for lunch, half an hour for breakfast, and out of the 12 hours he gives them a quarter of an hour in the afternoon for tea.[15]
> In the workshop there is a slate on which everybody writes down the time of his arrival. In case he comes 2 minutes past 6 he will write 6.15 and is paid accordingly. If he writes a quarter of an hour too much he is fined 7d., and for each hour 28d., or 2s. 4d. There is a clock in the shop and the other workers usually control the slate.– If one worker pushes another he pays 1s., if he beats him 2s. 6d. To show up drunk in the shop is fined, and to bring a stranger into the shop costs 2s. 6d.– Every Saturday the workers receive their wages with a deduction for the hours they have not been working and for fines, which money is given to the workers who will then drink ale or porter for part of it. #

[14] Anita McConnell, "From Craft Workshop to Big Business — The London Scientific Instrument Trade's Response to Increasing Demand, 1750–1820", *London Journal 19, 1*, pp. 36–53.

[15] Here Ljungberg is one hour wrong, ibid., p. 152.

> If a worker shows up in the shop with dirty hands or without having shaved he will get fined. In other workshops the rules are different. Ramsden's rules are pinned up in his workshop.

Is this so remarkable that it deserves mentioning? Well, to Ljungberg these labour conditions differed from those he knew in Denmark where the guild system protected the journeymen and required masters to abide by the rules common to all members of the guild. Hence, when he found that individual instrument makers in London were capable of setting up private rules of conduct he was astonished. The secrecy and the costs and risks to circumvent it were another surprise that Ljungberg got used to during his stay in Britain. He reported to Schimmelmann that this modern profession of instrument making could not be contained within the framework of the guild system.[16] Here in London a new division of labour, a new system of credit,[17] a new market of competition and secrecy and a new power-relation between employer and worker had established the pillars of modern capitalism.

> A great improvement by the local instrument makers consists in the fact that they make smooth and even work such as rulers, discs of brass with a plane. The plane is made of cast iron smoothed over below. The iron is adjustable by one screw and can be fixed by another. This provides for the wide, smooth planes, the straight edges and sharp corners, and much time is gained. The plane costs 1 1/2 Guinea. Ramsden employs about 40 workers, of whom 9 glass polishers, ... filers, ... planers ... , turners, 1 draws the tubes, ... divide the instruments. He manufactures almost anything at least as far as telescopes and big instruments are concerned, in his own workshop, whereas Adams and Nairne contract most of their products from other masters in the city.[18]

Ljungberg's notebooks abound in useful intelligence. Ljungberg was of course well aware that his espionage and shipment of four heavy boxes of contraband were a violation of the tool acts. To avoid jeopardising his own person he dissociated himself from his consignment and entrusted it to a shipping agent. So, in August 1789 when it was seized in London by Thomas Cross, a customs and excise officer, several years of hard work collecting it seemed to have been entirely wasted. Cross, however, was particularly interested in the notebooks since they might contain evidence proving that its

[16] Ibid., p. 134.
[17] Ibid., p. 133.
[18] Ibid., p. 152.

owner had rendered himself liable to prosecution. The tool acts contained a carrot and a stick. The carrot was a premium of fifty per cent of the value of the seized goods sold at a public auction. The stick was the dismissal of the civil servant if he let contraband slip through. Unfortunately, the notebooks were written in gothic German, a writing and a language Cross was unable to read. So he turned to Thomas Byerley for help. He was Josiah Wedgwood's partner and agent in London and his factory, Etruria, Staffordshire, appeared in the notebooks as one of Ljungberg's targets of espionage. Byerley reported to the Committee of Staffordshire Potters presided over by Wedgwood, who was alarmed and took immediate action.

Wedgwood contacted Matthew Boulton for support. Ljungberg had obtained a letter of recommendation to Wedgwood from Boulton since

> he knew him well as a very ingenious man and a good chymist. Ljungberg lived at Birmingham about a year and was esteemed for his ingenuity, modesty and gentleman-like behaviour. But we all suspected that he was employed by the Court of Denmark to collect such knowledge in this country as might be useful in that. And although I have some regard for Mr. Ljungberg, yet nevertheless I hope such decided measures will be taken as will prevent the transplantation of any of our manufactures ... [19]

Boulton and Ljungberg had in fact been on friendly terms during the latter's stay in Birmingham. A few months before his departure Ljungberg expressed his

> sincere thanks for the favour and the kind reception you pleased to show me in Your house during my stay in Birmingham; and also for the many happy hours, which with the loss of your own time, have been so very instructive to me.[20]

To deserve Boulton's confidence Ljungberg had been an intermediary between him and Chippendale's in London, and other favours were returned as well. But according to Wedgwood, Ljungberg had already sneaked around at Etruria for several weeks trying to snatch information on kiln technology,

[19] Boulton's letter of recommendation of 23.9.1789 attached to letter of 1.9.1789 from Neale & Byerley to the Custom's House, London; The Treasury, T1/673, XC 14104, Public Record Office, Kew.

[20] Birmingham Public Library, The Boulton & Watt Collection.

raw materials and know-how when he handed in Boulton's letter of recommendation. Finally, however, Ljungberg had run away before he could be apprehended, which was Wedgwood's intention as he discovered that Ljungberg was bribing his workers to extract intelligence concerning his mode of production.[21]

When Wedgwood had learned about the seizure of Ljungberg's notebooks uncovering the wide range of targets of espionage, he planned to pass on this information to his fellow-victims to gain their support to persuade Parliament to have the tool acts tightened and extended. With this in mind he had approached Boulton, and he now voiced his plan to Samuel Garbett, the arms manufacturer and head of the Commercial Committee of Birmingham who had prompted the 1785 extension of the list of contraband.[22] Wedgwood aimed at nation-wide support for his claims, but to achieve this he needed a translation of the notebooks. The intelligence collected by Ljungberg might enable the victimised employers to prosecute their corrupt workers. It might also reveal the names of workers whom this Danish spy had suborned or tried to suborn to sell their skills to a foreign power. If Wedgwood succeeded in uniting British manufacturers they might be able to convince Parliament that Britain's industrial head start was at stake unless stricter preventive measures were taken.

Wedgwood's strategy was only a partial success. He did obtain the permission to translate Ljungberg's notebooks. After all, it was in the interest of Thomas Cross, the seizure officer, to be able to submit evidence to the court to have Ljungberg convicted and his premium paid out. Nevertheless, Wedgwood failed to unite his fellow-victims. He was very disappointed to learn how unaffectedly the majority of them reacted to his warnings. I have not been able to trace the replies from manufacturers in the Wedgwood files, but I think it reasonable to summarise their reactions into three categories on the basis of their assumed interests:

1) Wedgwood was enjoying a virtual monopoly on the world market of queen's ware, but his mode of production could be imitated once his tools, kilns and raw materials were known.

[21]Wedgwood manuscripts 39/28404, pp. 81–88, University of Keele Library. My thanks are due to the late Prof. John R. Harris for this reference.

[22]S. Garbett's letter to W. Stiles, Commissioner of the Treasury, 25.8.1789, The Treasury, T1/673, XC 14104, Public Record Office, Kew.

2) Boulton's production of steam engines and minting presses was second to none. He knew that he and Watt had a head start of thirty years or more, and hence it was in his interest to define a visitor like Ljungberg as a potential customer rather than as a dangerous spy. Boulton's attitude to the tool acts changed like the direction of a weathercock. On the one hand he had nothing to fear from manufacturers abroad, since his technological leadership was beyond emulation; on the other hand he paid lip service to the policies of those fellow-manufacturers who were also buyers of his steam engines, e.g. Wedgwood and Arkwright.
3) A large group of textile manufacturers and instrument makers appeared to be sceptical about the effectiveness of the tool acts and preferred to rely on their own secrecy and to take their own precautions. Examples of this are Ramsden's workshop rules and Arkwright's letting the dead body of an apprehended spy hang in a tree outside one of his factories as a deterrent.[23]

If this suggestion about the diverging interest of British manufacturers is approved, how could they possibly establish a united front vis-à-vis intruders? Hence, Wedgwood's initiative petered out.[24] Ljungberg escaped and was never charged by British authorities.[25] Four years later he was back in London on a residence-permit to reassume espionage.

What happened to his four boxes of tools? Well, as soon as Ljungberg was back in Copenhagen he reported to his superior, Count Schimmelmann, and Count Bernstorff, the foreign secretary, instructed the Danish-Norwegian consul in London to do his utmost to have Ljungberg's consignment released. The consul was sceptical since the seizure had been reported by a newspaper agitating the public. The consignment was confiscated, but part of it showed up at an auction where the consul or his puppets offered the highest bid for a number of books, queen's ware and some rather innocent objects. Most of the goods, however, were lost, including a lathe, some 'clock-maker's tools', and unspecified tools and scientific instruments, estimated at £250. This means that Thomas Cross, the customs and excise officer, received a premium of approx. £125, or twice his per annum salary. The seizure of Ljungberg's boxes is one of the few known manifestations of the effect of

[23] Ljungberg's notebook (note 8), p. 223.
[24] Wedgwood manuscripts 39/28404, p. 88, University of Keele Library.
[25] I regret to confess that my statement about his imprisonment in *Centaurus, vol. 37, 1994*, pp. 290–321 was based on conjecture and must be refuted.

the tool acts.

What happened to the notebooks? For a long time their fate was a riddle to me. Had they survived at all? I searched them here; I searched them there; I searched them all and everywhere. Were they in Britain? Were they in Denmark? Coincidentally, I learned that a notebook by Ljungberg was kept by the Public Record Office in Stockholm. But was it *the* notebook? I requested The Public Record Office in Copenhagen to obtain a loan of it, but our Swedish friends refused the request, estimating that the item was too valuable to be sent abroad. So, I went to Stockholm, repeated my request and sat down waiting in the reading room for one hour, for two hours, when an archivist informed me that this valuable source material was untraceable. From my looks the archivist must have realised that I was somewhat perplexed by the fashion a Nordic brother and this unique piece of evidence were being treated. So, the head of the Public Record Office sent out his army of archivists to search several kilometres of shelves. He called me at my hotel in the evening. Ljungberg's notebooks had surfaced. The next morning I found myself in the reading room staring at 273 pages mostly in gothic German and dozens of outstanding technical drawings and descriptions written by a master spy and last glared at more than two hundred years ago by Wedgwood, the vindictive potter from Staffordshire. Ironically, on the last page of his notebook Ljungberg had glued his legal residence-permit dated 1793.

The case of Ljungberg, who has left us with probably more source material than any other spy, shows how dangerous and troublesome it was to transfer technology. But even the secrecy of instrument makers and the deterrent of the tool acts could not prevent the know-how from falling into foreign hands.

The tube drawing machine was inaccessible to the public in contrast to Ramsden's dividing engine, which was at the disposal of British instrument makers only. This is why Jesper Bidstrup, of whom we shall soon hear a lot more, was thrown upon his chamber master in London when he wanted to divide his sextant in the most accurate fashion possible. Bidstrup could not go to Troughton's workshop himself to have his sextant divided, since Troughton was only obliged to divide for British instrument makers. Secondly, although Ramsden's dividing engine was in print, the published drawings were inadequate as models for an instrument maker wishing to copy it. We know that Henrik Gerner and Thomas Bugge, both members of *The Society for the Encouragement of Science, Agriculture, &Commerce,*

London, the English parent organization of *The Royal Danish Society for Agriculture*, were in possession of Ramsden's book, but Bidstrup, operating in London in the decade 1787–1798 could not retrieve a copy.[26] And even if he had been able to locate it he would not have been able to copy a dividing engine. A workshop equipped with one of Ramsden's other notorious tools, the screw-cutting lathe, was the necessary prerequisite. This leads us on to

The fourth strategy consisting in sending a person to London to be trained in the making of instruments. In this instance Jesper Bidstrup is the main character who has left clues of his activities in a number of preserved letters.[27] Thomas Bugge, secretary of the Academy of Science, and Chief surveyor, then selected him as being one of his talented students of mathematics and astronomy to go to London to acquire the skills of an instrument maker. Bugge contacted his colleague, Sir Joseph Banks, asking him to recommend Bidstrup for an apprenticeship in one of the excellent workshops. This proved to be more difficult than anticipated. Banks sent notes to Bugge and Bidstrup that his inquiries had been unsuccessful.

London instrument makers maintained an attitude of reserve and secrecy against teaching a foreigner a craft that had procured so much wealth and reputation from abroad. An apprenticeship of seven years was costly even for a natural-born British subject who had to pay £50–200, a sum that the young man would lose to his master in case he left prematurely. The Swedish king had paid Ramsden £200 to apprentice Appelqvist, but they did not get on together. And although Cassini, the French astronomer, had bought scientific instruments from Ramsden for more than a thousand pounds, Ramsden refused to let any French people into his workshop.

So, Bidstrup turned towards Count Brühl, the envoy from Saxony, a well known amateur astronomer and consultant, who opened the doors to two of the many subcontractors in the instrument business of London, chamber master White, subcontractor to Nairne & Blunt for £100, and to Harbin, an optician and chamber master working for Dollond. After two years Bidstrup left White in friendship to join Higgins at Walworth. Higgins was Ramsden's first apprentice and worked long for him till they entered into some sort

[26] *Description of an Engine for Dividing Mathematical Instruments by Mr. J. Ramsden, Mathematical Instrument maker*, published by order of the Commissioners of Longitude, London, 1777.

[27] The account of Jesper Bidstrup is an abbreviated version of my article in *Centaurus, vol. 37, 1994, pp. 290–321* which contains all references and a few illustrations.

of companionship; Higgins moved into Ramsden's house in Piccadilly and took over as a shop steward, but according to Bidstrup Ramsden's mediocre moral character only allowed Higgins to stay for eight months. Higgins was said to be the best supplier to Nairne & Blunt, but also worked for Adams and was presently completing a polar sector and the biggest transit instrument yet made in England, initiated by Sisson.

Bidstrup obtained reduced working time in Higgins' workshop by two hours in order to open a workshop of his own at Leicester Square (in Newton's old house). Bidstrup talks to Herschel about his new discoveries and instruments and reports their conversation to Bugge. He then ships his sextant, an instrument of which he is very proud, divided by Troughton's engine, demonstrated to Joseph Banks and in Copenhagen assessed as matching the best of English instruments. At Leicester Sq. Bidstrup also makes microscopes, a 4" achromatic telescope, and 'a mechanical power' for Hauch. Bidstrup soon realised that in order to set up an up-to-date workshop in Copenhagen at least five British inventions were prerequisite, viz. the dividing engine (to reduce costs), a glass polishing machine, a tube drawing machine, a cupola furnace and a screw cutting lathe.

It proved most complicated for Bidstrup to have steel foundries in Sussex and Sheffield cast the mandrels for a tube drawing machine and the various discs for glass polishing. He had to make the moulds himself, which he did, but his journey on foot to Sussex to collect the items (he communicated with Sheffield via an agent) was postponed by lack of transfer of money from Denmark, so that, when finally he showed up capable of paying, he learned that the steel mandrels had indeed been made, but since they had not been collected the steel had subsequently been recast into other items, his moulds had been ruined and had to be reconstructed by himself from scratch.

When the mandrels, now having been cast twice, were to be shipped to Copenhagen from Yarmouth the utmost care had to be observed, since their exportation was illegal according to the tool acts, so the consignment was handled by a trustworthy agent, Mr. Wheeler. Bidstrup, of course, would learn from the bad luck Ljungberg had experienced in 1789. An additional complication, however, occurred. The Duke of Portland had established a private corps of guards to enforce the tool acts, and Bidstrup, although in possession of a valid passport and having cleverly dissociated himself from his consignment, felt obliged to hide and change his itinerary. Nevertheless, he did escape and his tools, so arduously acquired, arrived safely in Copenhagen.

However, this is not a happy ending story. Bidstrup established his workshop at least partially, and the magnificent tools were ready to be operated. Then, in 1802, he fell ill and died. His widow had to sell his equipment by auction because she owed its value to the government. Five years later the British bombardment of Copenhagen hit the Round Tower, and Bugge's library and collection of instruments were partly blown up. McKenzie, a Scottish surveyor working on Iceland and a friend of Bugge's, assured him that he and his fellow scientists in Britain had talked about the incident with much concern and all felt convinced that the bombardment must be some misunderstanding. After all Bugge was a fellow of the Royal Society.

In conclusion I would like to stress two points. Firstly, instrument making was revolutionised during the second half of the eighteenth century. Before that time all parts of the instruments were made by hand, but from now on most parts were made by improved tools and machines. This transition changed the division of labour in the workshop and the old guild system only survived by name. Hence, transferring modern instrument making involved the acquisition of the innovations of the trade such as the tube-drawing machine, the glass polishing machine, the cupola furnace, the screw-cutting lathe, and the dividing engine. It also involved the training of skills to operate these innovations, and since there was nobody capable of this in Copenhagen, where, furthermore, the old guild system still prevailed, these skills had to be appropriated by one person. Secondly, the cases of Ljungberg and Bidstrup indicate that no matter how secretive manufacturers were and no matter how carefully the tool acts were observed by determined civil servants they did not succeed in preventing their precious technology from falling into foreign hands. All they could do was to make the life of predacious spies and trainees more troublesome and dangerous.

Instrument Maker on the Run: A Case of Technology Transfer

Olov Amelin *

Abstract

This paper will deal with a number of design principles and an instrument maker. The most important parts are played by a circle, the number 96 and the craftsman Johan Ahl (1729–1795), who made the instruments that were used by the Norwegian surveyor and mathematician Caspar Wessel.

Whether Ahl is to be regarded as a deserter or not depends on the view one takes of the relationship between the Royal Swedish Academy of Sciences and the Royal Danish Academy of Sciences and Letters in the 1760's. The question of technology transfer can also be seen from two points of view. From a Swedish point of view it was highly undesirable; from a Danish point of view it was most welcome and wanted.

Introduction

Johan Ahl came to the workshop of the instrument maker Daniel Ekström in Stockholm in its heyday around 1750. Most likely he was one of the "mathematically trained workmen" who together with three younger apprentices were in the workshop in 1754.[1]

After the death of Ekström in 1755 there was no obvious heir to the business. Various solutions were discussed, but it appears that Ekström himself

*The Nobel Museum, P.O. Box 2245, SE–10316 Stockholm, Sweden

[1]The tax records of 1754, City archive in Stockholm.

wanted Johan Ahl and Johan Zacharias Steinholtz, one of the other apprentices, to take over the workshop, which was actually owned by the Royal Swedish Academy of Sciences. The Academy agreed with Ekström and appointed Steinholtz as director and Ahl as his partner.

However, joint management by Ahl and Steinholtz does not appear to have worked successfully. Steinholtz took over the business and Ahl found himself making more simple instruments such as thermometers, compasses and drawing instruments.[2] In 1760 we find Ahl mentioned in the tax register of "Lower West Klara", an area in central Stockholm.[3] He is mentioned as "The partner in the workshop Johan Ahl, married, 1 powder, 30–31 years of age". The living quarters and the workshop were located in the block Kajan No. 49, which also included a herb garden. Ahl and Steinholtz had moved from Ekström's old workshop in the observatory to new premises in Stockholm. Altogether twelve people worked here, including Ahl's partner, the three-year-younger Steinholtz, and Steinholtz' wife. Two maids, five apprentices and a lodger are mentioned in the register. The information "1 powder" indicates that they could afford the luxury of powdering their wigs, for which a tax was paid. But the wealth was illusory. In March 1760 the relations between Ahl and Steinholtz had deteriorated to such an extent that the Academy thought it would be best to separate them. Financially, Steinholtz benefited from the separation. Two-thirds of the allowance, which since Ekström's day had gone to the workshop, now stayed with Steinholtz. Conditions for Ahl were difficult, as may be seen from the Academy minutes. By May 1760 he was having to ask for 600 copper dalers in advance, which he was granted.[4] Ahl's requests to be paid in advance appear frequently in the minutes and suggest that his terms of employment were poor.[5] However Ahl probably owed money to others besides the Academy and he was also dabbling in high finance and was defrauded by a clerk at the Bank of Stockholm.[6]

[2]Lindroth, Sten, Kungl Vetenskapsakademiens historia 1739–1818, p. 799, Stockholm 1967.

[3]Fataburen 1915, Stockholm 1915.

[4]The Royal Swedish Academy of Sciences, Minutes 21.5.1760.

[5]Ibid. 21.5.1760 (600 copper dalers), 14/10 1761 (2000 copper dalers), 18.11.1761 (1500 copper dalers).

[6]The Royal Swedish Academy of Sciences, Secretary, Paper 33:1 Johan Ahl, Memorandum, not dated but probably 1763; Promissory note signed by Colliander, Stockholm 8/10 1760. Colliander returns to the bank and Ahl tries to pay his debts to the Academy

The last notes we can find in the Academy minutes about Ahl's work in Stockholm date from 17 March 1762. The Academy's experimental physicist Johan Carl Wilcke demonstrated an inclination compass, an instrument for vertical measurement of the earth's magnetic field, that he had designed and Ahl had manufactured.

The next note in the minutes tells of Ahl's escape from his creditors. The Danish government promised the Swedish minister in Copenhagen to pay Ahl's debt to the Academy. This is as early as June 1763. In other words, Ahl must have established himself quickly in Denmark and managed to make contact with the right people. Little is known about his departure, but in June 1762 Ahl is mentioned in a memorandum written by Henrik Hielmstierne, secretary to the Royal Danish Academy of Sciences and Letters in Copenhagen. According to the memorandum he had "[...] with great difficulty arranged for immigration of a very skilful instrument maker from Stockholm"[7] (author's translation). The letter does not tell what this difficulty was. Maybe Ahl had hesitated, but more important was the fact that Ahl's escape to Copenhagen meant that Sweden lost one of her two most competent instrument makers, also one in whom the Royal Swedish Academy of Sciences had invested large amounts of money. The state of Swedish instrument-making was poor, even before Ahl's disappearance. Important knowledge had now fallen into the hands of a foreign country. The Danes, on the other hand, had everything to gain. On 26 June 1761 the Royal Danish Academy of Sciences and Letters had been granted funds by the King for the cartographic surveying of Denmark and the publishing of a nation-wide map. Behind the project were the professor of mathematics and member of the Academy Christen Hee (1728–1782), and the young assistant at the Round Tower Observatory Thomas Bugge (1740–1815).

The need for surveying instruments was great in Denmark after the King had approved the project. At this time Muth was the royal instrument maker, but he was considered too old and too slow to be able to provide

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and Steinholtz using a promissory note. The promissory note was not accepted by the Academy, see **33:1** ***Utdrag af kongl. secreteraren Faxels skrivelse till h:s ex:ce ... Cantzlie presidenten grefve Ekeblad*** **dated Copenhagen 20 November, 1763.**

[7]Memorandum 27.6.1762 from H. Hielmstierne to J. L. Holstein, archive of the Royal Danish Academy of Sciences and Letters, Special archive, Geografisk landmaaling 1761–1765. Written at the top of the document in another hand is a statement about salary for Ahl (150 Rdlr) paid as from 21 May 1762. From these sources we can date his "escape" to Denmark to April or early May 1762.

the Royal Academy of Sciences and Letters with the necessary equipment.[8] Johan Ahl, or Johannes Ahl as he was called in Danish, came to Copenhagen in the spring of 1762. He immediately started to provide the Academy with instruments. Ahl must have been asked to come to Copenhagen to provide Bugge with equipment. The Swedes' talk about desertion therefore gives a misleading impression of the course of events. It is likely to have been an easy decision to make for Ahl, who was burdened with debt, and also on poor terms with his former partner Steinholtz.

Furthermore, orders placed by the Royal Danish Academy of Sciences and Letters had been received in Stockholm as early as the winter of 1761–62. Parts of them had been carried out, probably by Ahl and Steinholtz, and delivered to Joachim Otto Schack, the Danish agent in Stockholm. Some of the larger instruments, though, probably made by Steinholtz, had not been delivered. The Royal Swedish Academy of Sciences, with its mining consultant Daniel Tilas and its secretary Per Wilhelm Wargentin leading the way, retained the ordered instruments, to try, if possible, to force the Danes to return Ahl to Stockholm. In Denmark Ahl immediately received an annual salary of 100 Rigsdaler, which was raised after a couple of months to 150 Rigsdaler. By 1769 Ahl was receiving 300 Rigsdaler a year plus another 50 Rigsdaler for the employment of an apprentice.[9] Ahl quickly reached the position of being the foremost instrument maker in Denmark, and was spoken of as "[...] our skilful Ahl" or "[...] our skilful and deserving Ahl".[10]

Ahl's debt to the Royal Swedish Academy of Sciences was paid off in 1764, when the Swedish envoy, Mr. Sprengtporten, informed the Academy member Count Ekeblad, that "after long drawn out proceedings" he had received the 4000 copper dalers that the departed Ahl owed the Academy. For his work in connection with the recovery of this debt Sprengtporten received a specimen

[8]Ibid: "og den her værende naunlig Muth som har aarlig pension af Hs Majesteet, findes deels at være meget uaccurat og langsom deels kostbar med Hs arbeide, det og ofte forefalder at der gaaer Slid og fall paa deslige Instrumenter, som i en Hast maa giøres til Rette for ei at sinke arbeidet."

[9]C. Molbech, *Det Kongelige Danske Videnskabernes Selskabs Historie 1742–1842.* Copenhagen, 1843, p. 77.

[10]The information on Ahl's stay in Denmark is, unless otherwise stated, taken from Keld Nielsen, *Hvordan Danmarkskortet kom til at ligne Danmark, Videnskabernes Selskabs opmåling 1762–1820,* Foreningen Videnskabshistorisk Museums Venner, Århus, 1982.

of every medal minted by the Academy.[11]

1 The mapping of Denmark

The implementation of this project has been described carefully by Keld Nielsen, but a brief reference to the methods used may be useful.

The Danish mapping project was large and ambitious. It undoubtedly made Denmark one of the leading nations in the area of land surveying. The aim was to make a nation-wide triangulation net. The method used came from France, where it had been in use since Jean Picard (1620–1682) had measured a part of the meridian through Paris and Amiens in 1669–1670. The Cassini family then used this method when the mapping of France started in 1747. It involved connecting dots along a meridian with a series of triangles with carefully measured angles, after which it was enough to carefully measure the length of one side of one of the triangles — the baseline. With simple trigonometric calculations, the lengths of all other sides of the triangles could then be calculated and finally also the length of the meridian. The disadvantage of this method was that any mistake in measurement was transferred to the final determination of the meridian. This made extremely accurate angular measurements absolutely essential, and the instruments had to be manufactured and used with great skill. To meet this need, Denmark began an extensive training programme, in which land surveyors were taught to perform the triangulation measurements. The whole project was described by Thomas Bugge in 1779 in *Beskrivelse over den Opmaalings Maade, som er brugt ved de Danske geographiske Karter; med tilføiet trigonometrisk Karte over Siæland, og de der henhørende Triangler, beregnede Longituder og Latituder, samt astronomiske Observationer.*[12]

2 The Ekström circle in Denmark

Johan Ahl was trained in Ekström's workshop, a workshop that can be said to have specialised in land-surveying instruments. At least this was the area

[11] The Royal Swedish Academy of Sciences, Minutes 9.1.1765.

[12] A description of the surveying method used for the Danish geographical maps with an attached map of Zealand, including the triangulation net, calculated longitudes and latitudes, and astronomical observations (author's translation).

in which Ekström made his most significant technical innovations with the introduction of the levelling tube and the circumferentor, also called the Ekström circle. The instrument was much appreciated in Sweden and was Ekström's most successful invention. The circle was presented to the Royal

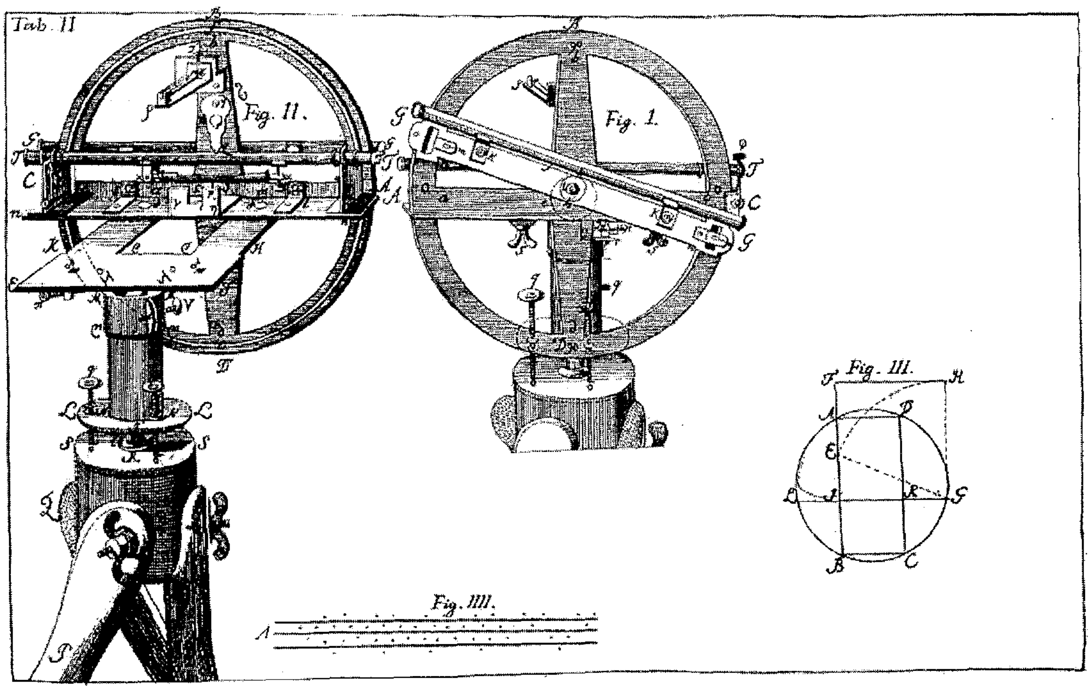

Figure 1: Daniel Ekström's geographical circle; illustration from his article in the Transactions of the Royal Swedish Academy of Sciences 1750.

Swedish Academy of Sciences in 1750 and was described in an article in the academy's transactions the same year.[13] An interesting detail was that it incorporated elements of a Danish tradition of instrument-making.

In England and France parts of circles (quadrants, sextants etc.) were preferred for different angular measuring instruments. Ole Römer (1644–1710) on the other hand favoured complete circles and designed his own instruments of this type. Complete circles were more stable and the scales were

[13]Ekström, Daniel, Beskrifning på Et nyt Geographiskt Instrument, Kungliga Vetenskapsakademiens Handlingar 1750 (Transactions of the Royal Swedish Academy of Sciences), Stockholm 1750.

easier to provide if the division of the scales could be checked against the diagonal of the circle. Whether or not Ekström was influenced by Römer's designs when he made his circle is not clear. In 1763 Ahl had made a circumferentor for the Royal Danish Academy of Sciences and Letters, for measuring the angles of the triangles mentioned earlier. The instrument was much appreciated by Bugge, because it was so easy to handle. In his description of the instrument he says that the right angles were divided both into 90° and into 96 sections, in other words using George Graham's method of dividing the circle by bisection.[14]

The scales on the smaller circular instruments (to which category the surviving examples belong) were probably divided by using a template and bear no traces of division into 96 sections. The instrument depicted in Bugge's book differed on several accounts from the original design by Ekström. The stand was more stable and the circle was provided with a semicircle with a cog positioned at a right angle to the surface of the large circle. This made it possible to place the circle at any angle between 0° and 90° by using an adjustable screw. The scales could be read, according to Bugge, by using four vernier scales. At both ends of the adjustable tube there was a vernier scale for the 90° scale and one for the scale divided into 96 sections. With these, readings as small as 15 seconds of arc could be taken. As a complement

[14]Thomas Bugge, *Beskrivelse over den Opmaalings maade som er brugt ved Danske geographiske karter,* Copenhagen, 1779.

The circle made by Ekström, now in the Old Observatory in Stockholm, does not have a scale divided into 96 sections. In Denmark three circumferentors are preserved. The one mentioned in the text, signed by Ahl and later modified by his apprentice Jesper Bidstrup who added more bubble levels, does not have a scale divided into 96 sections (Bugge is referring to Bidstrup's work with Ahl in a letter to J. Banks in 1787, see Ny Kgl. S. 287.1, 4°, Royal Library, Copenhagen). The instrument is now in the Ole Römer museum in Taastrup. Another instrument, much like the one mentioned earlier, is at *Kort och matrikelverket* in Copenhagen. There is no sign of a scale divided into 96 sections on this instrument either. The third instrument at the Aalborg University Centre is of a somewhat simpler and probably earlier construction. It is reminiscent of the original instrument made by Ekström. Even the construction of the stand is identical with that of Ekström. All three of these instruments were probably made by Ahl. Yet another circle by Ahl is at the National Administration of Shipping and Navigation in Norrköping. There is no trace of a scale divided into 96 sections on this instrument either.

On both contemporary illustrations of Ahl's circular instruments in Thomas Bugge's *Beskrivelse over den Opmaalings Maade ...* (table 1) and *Observationes Astronomicae Annis 1781, 1782 & 1783* (table VIII) (in the latter a somewhat larger circle mainly intended for astronomical use) the scale is shown as divided into 96 sections.

to the vernier scales there was also a micrometer placed in the adjustable tube. Ahl made his circle in different versions, a simple variant much like the one Ekström had constructed and a couple of more advanced versions both for geographical and astronomical use. The large circle used by Bugge has disappeared; however, if we compare Bugge's description with the two surviving geographical circles in Stockholm and Uppsala signed by Johan Zacharias Steinholtz, there are several similarities. The adjustable tube has a micrometer and the adjustment of the circle is the same as on Bugge's instrument. The stand is also similar to Bugge's. Steinholtz' instrument No. 11 was made in 1761 when Ahl was still in Sweden. Today the instrument is at the Royal Swedish Academy of Sciences in Stockholm. Instrument No. 10 is in the Observatory Library in Uppsala. Most likely Ahl participated in the making of No. 10, even though Steinholtz signed it. The improvements to the Ekström circle described by Bugge had therefore already been developed when Ahl decamped to Denmark. Perhaps some of the instruments preserved in Sweden were even originally made for Denmark as parts of the order executed but not delivered by the instrument makers in 1761.

Bugge touched again upon Ahl's circle in an account of his visit to Paris in 1798–1799 as a member of the "international" commission of weights and measurements. There he had come into contact with the repeating circle made by Borda. Bugge wrote that the instrument in question which had a two-foot radius, gave very good results, but that the reports of its accuracy were exaggerated. After analysing a few of the results he found a measuring error of six seconds of arc. Bugge writes proudly that circle instruments of similar design have been used in Denmark for nearly 40 years. He describes some design differences, but goes on to say that in his opinion Ahl's circular instrument generally gives the same accuracy of reading as those of Borda.[15]

Bugge's description of Borda's instrument is both interesting and revealing.[16] There were major differences in design between Ahl's and Borda's scales. Ahl's best instruments had two scales, one 90° scale and one divided into 96 sections. The technique was that employed in the making of quadrants, using a pair of compasses to divide smaller and smaller angles. This was the method developed by Graham in the 1720's, which Ahl had learned from Ekström in Stockholm. In Borda's case there was a scale on which the quarter circle was divided into 100 sections. Bugge saw this as something

[15] Thomas Bugge, *Reise til Paris i Aarene 1798 og 1799,* (Copenhagen, 1800, pp. 553).
[16] Allan Chapman, *Dividing The Circle* (London, 1990, p. 118).

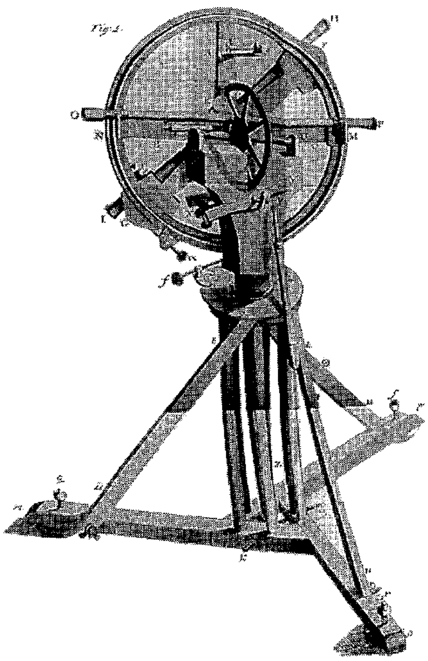

Figure 2: Ahl's geographical circle depicted in Bugge's "Beskrivelse over den Opmaalings Maade … ", 1779.

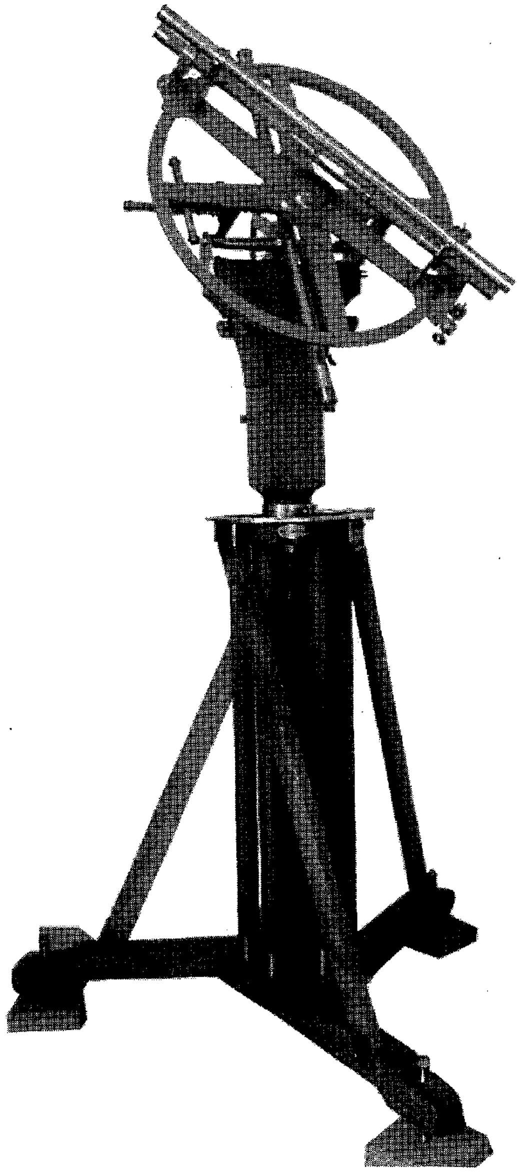

Figure 3: J.Z. Steinholtz's geographical circle made around 1761. Now in the collections of the Royal Swedish Academy of Sciences. (Courtesy of the Royal Swedish Academy of Sciences).

negative, but Borda most probably divided his scale with more modern tools, i.e. some kind of dividing engine giving high precision without any need for another scale. When Bugge visited Paris in 1798–1799 he found circles that were divided by machines equipped with microscopes. These were also read with microscopes equipped with micrometers, giving a much higher precision than Ahl's instrument. Both Bugge and Ahl had missed out on this development. Circles and, more particularly, the technique for dividing the scale continued to develop elsewhere in Europe.

3 Ahl's Quadrant

The Danish triangulation network that was measured with the circumferentor was orientated with the meridian passing through the observatory at the Round Tower in Copenhagen. The position of the meridian was fixed by solar observations. To orientate the meridian in the field Bugge measured the angle between the meridian in the northern direction and the line of sight towards the south spire of the cathedral in Roskilde. Another line, at right angles to the meridian through the observatory, "the Perpendicular", was measured to enable the construction of what we would call a co-ordinate system with an x-axis and an y-axis. We shall not go deeper into the technical details of mapping, but the two lines through the Round Tower and the triangulation net made up the framework for the whole mapping project. To be able to give the map the exact degree of longitude and latitude it was necessary to perform a very careful determination of the Round Tower. For this purpose Bugge ordered several instruments from Ahl's workshop, one of which was a 3-ft quadrant (92.5 cm radius) delivered in 1778. With this quadrant Bugge could determine the polar altitude of the observatory after measuring 28 fixed stars during the winter of 1778/79. Complementary measurements were also made using a circumferentor. The result that Bugge arrived at was 55° 40'56", which may be compared with the modern measurement of 55° 40' 52"[17]. The work was done mainly with the quadrant made by Ahl. Once again we can find similarities with instruments that Ahl had seen or worked with in Stockholm. The stand was similar to the one made for the quadrant delivered to the Stockholm Observatory in 1757. The

[17]Nielsen, p. 40. Nielsen states that Bugge had calculated the value 55° 40' 52" but changed the seconds to 56", because it gave a more even number when working with six-digit logarithms. Log sin 55° 40' 56"=-0,083060, an even and "handier" number.

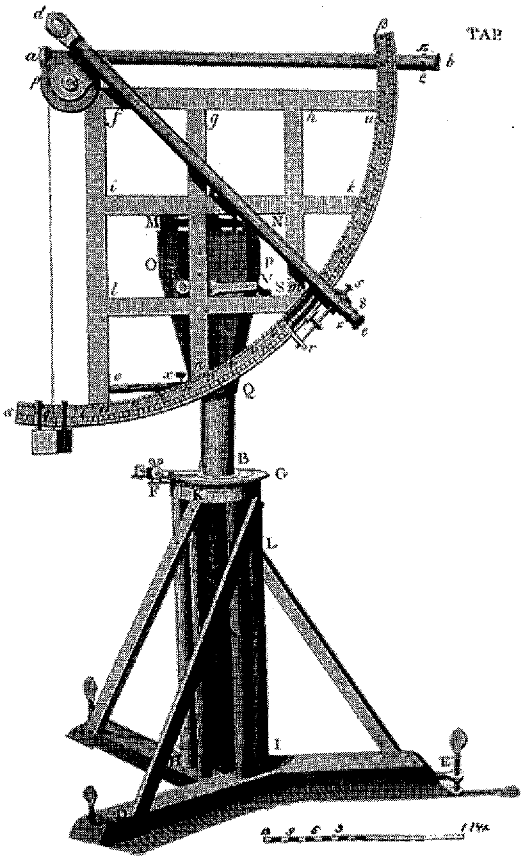

Figure 4: Quadrant made by Ahl in 1778.

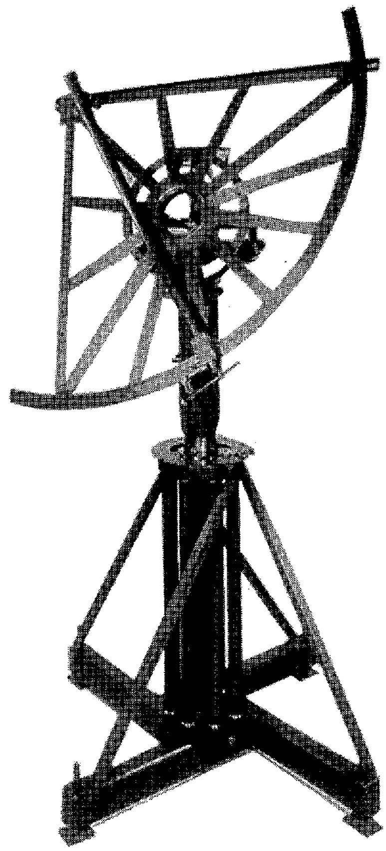

Figure 5: Quadrant made by Bird in 1757. (Courtesy of the Royal Swedish Academy of Sciences).

maker of this instrument was John Bird, the most famous of all instrument makers at the time. The size of the instrument differs by six inches from Bird's construction. One important point of difference is in the metal framework; Ahl's design has squares, whereas Bird's has spikes radiating from a central hub around which the quadrant could be turned. Both instruments had two scales, one divided into 96 sections and one 90° scale. Ahl's quadrant was provided with an adjustable tube with both a vernier scale and a micrometer. Today the instrument is at the Round Tower. Tubes and other parts are missing.

The longitude was of course harder to determine exactly, but based on the eclipse of the moons of Jupiter the position of the Round Tower could be determined in relation to the Paris Observatory and the Stockholm Observatory.

4 The restoration of the Observatory at the Round Tower – a comparison with the Stockholm Observatory

At the time of these measurements Bugge was busy restoring his observatory at the Round Tower. A new top storey with an octagonal room in the middle and two wings was built on the roof of the tower. Ahl continued to receive orders for more instruments. In addition to the quadrant, Ahl was asked to make a zenith sector, a mural quadrant and a transit instrument. A Swedish traveller, the astronomer Henrik Nicander, visited Bugge in the late summer of 1778 and reported back to Wargentin about the work on the observatory:

> The largest observatory room is a hexagon [finally the room was built like an octagon, author's note], with a window to the north and south. Inside a mural quadrant with an 8-ft radius will be placed. It has not yet been built, but has been ordered from the skilled instrument maker Ahl, who is residing in Copenhagen. A sector with a 16-ft radius will also be installed, but in such a way that it is placed in a hole from the bottom of the tower, all the way through the hexagon. Through this hole the stars can be observed clearly during daytime. On the sides of the hexagon, to the east and to the west, are two rooms. In the room to the east, a 6-ft transit instrument by Ahl will be placed on a marble pillar. On another pillar in the same room the observation clock has been placed. In the other room to the west, a quadrant with a 3-ft radius will be placed, which is now in the hands of Ahl [...] The view from all sides extends for miles and to adjust the mountings of the instruments on the meridian a sign will be placed on the beach of the island of Amager – a Swedish mile [10 km] from the observatory.[18]

[18]Nicander to Wargentin 5.9.1778, The Royal Swedish Academy of Sciences.

Bugge built the best-equipped observatory in northern Europe. The prototypes were mainly the English observatories that he had visited in 1777. The ambitious nature of the project forced the instrument maker Ahl to work on very demanding tasks, and he was able to develop his skills. In the letter quoted above, Nicander sends Ahl's compliments and states that Ahl "seems to be more appreciated in this place, than he was by us". Almost a year later Bugge himself writes to Wargentin and tells of his plans. Either Nicander had misunderstood some matters or Bugge had adjusted sizes and positions of the instruments. The mural quadrant was supposed to have a 6-ft radius and the zenith sector a 12-ft radius, not 8- and 16-ft respectively as stated by Nicander. Bugge does not mention anything about placing the zenith sector in the centre of the tower. The zenith sector was placed together with the other instruments in the observatory building on the roof.

Bugge's observatory contained all instruments that were used at this time in the large European observatories. A comparison with Wargentin's set of instruments in Stockholm shows that Bugge made greater use of precision angle instruments. During his tour of England he visited several of the most famous instrumentmakers and watchmakers in London. Bugge was able to learn the design details both at Edward Nairne's and at Jesse Ramsden's workshops. At the observatories in Oxford and Greenwich he was given the opportunity to make observations using both transit instruments and mural quadrants.[19] In the diary that Bugge kept during his tour, there are details of the instruments with drawings and comments. This knowledge was passed on to Ahl upon Bugge's return and we can find similarities between the instruments constructed by Ahl, the drawings in *Observationes Astronomicae Annis* 1781, 1782 and 1783, and the English instruments depicted in Bugge's sketches.[20] Ekström had also made similar instruments, and Bugge's contribution can be seen as a refinement of details. Ahl knew the basic principles already.

In Bugge's letters to Wargentin he describes several technical details of the instruments. The sight tube in the movable quadrant and the transit instrument had an achromatic lens system. The scales were divided into 96

[19]K. M. Pedersen, "Uddrag af Thomas Bugges Dagbog 1777", *Festskrift i Anledning af Universitetsbibliotekets 500 Års Jubilæum* (Copenhagen, 1982).

[20]Ibid. pp. 155. and Bugge, T. *Observationes Astronomicae annis 1781, 1782 & 1783*, Hauniae 1784.

sections and 90°. A special construction to turn the movable tube on the quadrant was designed by Bugge himself. He took a great interest in the instruments and participated in the development and construction work with Ahl in a way that Wargentin never did with Ekström. This also depended on the difference in scope between Bugge's and Wargentin's work. From the beginning Bugge worked with observations where precise calculations of position were of great importance. That Bugge had great confidence in his instrument maker is well expressed in the following letter:

> All these instruments have been made by Mr Johannes Ahl, Swedish by birth, whom I respect highly for his competence. I do not think that he will yield to any instrument-maker in Europe except to the deceased Bird and the yet living Ramsden.[21] (author's translation).

Wargentin's studies of the moons of Jupiter were not dependent upon angular measurements in the same way. But there was also a basic difference in attitude between the two astronomers; this can clearly be seen in Wargentin's reply, written in November 1779:

> I have not wanted to recommend the Royal Swedish Academy of Sciences to purchase such expensive instruments, because it is hardly worth the effort in our climate, where the opportunities to observe are very few. The three to four months of summer, are one long day, while the winter is either cloudy or so cold that one cannot handle such sensitive instruments with a steady hand. Also I think it is unnecessary to put so much money into astronomy when so many observatories in Europe, in better climates, are already equipped with the finest instruments and the best observers, which I could never hope to equal, much less surpass. They all have the same sky, the same work. When one does not think one can do better than them, less joy and honour comes out of the cost, the watching, the effort.[22] (author's translation).

The halcyon days of the Round Tower end around 1800. Thereafter Bugge had to devote more time to military work in connection with the war with England, and his astronomical work was therefore neglected. The instruments fell into decay and most were removed from the Round Tower. In 1808 The Copenhagen Fire commission decided to convert the tower into

[21] Bugge to Wargentin 20.6.1779, The Royal Danish Academy of Sciences and Letters.
[22] Wargentin to Bugge 2.11.1779, The Royal Library, Copenhagen.

a fire tower.[23] But Bugge stopped the plans and his building was able to continue to work as an observatory until the end of the 1850's.

5 Summary

When Ahl left Sweden, Swedish instrument-making was on the brink of ruin. In Denmark on the other hand, it began to flourish. Why?

Denmark had all that was required for the successful co-operation between instrument makers and scientists that was essential. In Sweden this co-operation lapsed when Ekström died. Wargentin did not have confidence in Steinholtz and Ahl, so large orders were placed abroad.

Was Ahl a deserter?

From a Swedish point of view Ahl's move to Denmark was most unfortunate. Every attempt was made to get him back. His departure caused both expense and the loss of valuable knowledge to Sweden. From a Swedish point of view the transfer of knowledge was therefore highly undesirable. Denmark, on the other hand, no longer had to import surveying instruments. Instead it became one of the largest manufacturers. The investment in Ahl enabled Denmark to build the most advanced instrument-making workshop in Scandinavia. The large mapping projects, both the theoretical and the technical aspects, placed Denmark far ahead in the field of cartography.

Ahl's work is an interesting and clear example of how technology could be transferred. First from English instrument makers to Ekström, then to Ahl, who took his knowledge with him to Denmark, where Bugge added the knowledge he acquired on his tour to England. The circumferentor, or Ekström circle, was a unique construction heralding theodolites and repeating circles. But the decisive factor in the successful cartographic project was the co-operation between artisans and scientists. Without this, the scientific image of Denmark would not have received the boost that it did.

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[23] Kjeld Gyldenkerne, Per Barner Darnell and Claus Thykier, *Dansk astronomi gennem firehundrede år* (Copenhagen, 1990), p. 103.

Wessel as a Cartographer

L. Kahl Kristensen *

Abstract

The primitive level of geodesy at the time the young Caspar Wessel became assistant surveyor is described. Fundamental geometric concepts were not yet developed and the shape of the Earth still debated. Many computations tacitly assumed a flat Earth. When Caspar Wessel took over the triangulation of Jutland he proposed a division of the country. His plan was approved by the Royal Danish Academy of Sciences and Letters at a meeting on February 5 1779. Although this plan is often mentioned, **the content of it is not known**. Inspection of the published maps, however, shows that it was a rational division, still in use, which allowed the sheets to be joined. This system is continued to the present day.

The kind of applied mathematics which filled Wessel's professional life is indicated by Thomas Bugge's writings.

A new geometric theorem shows simply, in terms of the Gauss curvature, the size of the error in regarding the Earth as flat. This "gap theorem" is invented for the present paper.

1 Introduction

Soon after the mathematically talented Caspar Wessel arrived in Copenhagen to study law he was employed (from 1764 to 1805) by the Royal Danish Academy of Sciences and Letters in the surveying of Denmark.

This project was an imitation of the mapping of France under the auspices of the French Academy. It involved practical work in the countryside, applied

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mathematics and much computing. The kind and level of this mathematics, on which Wessel must have spent much time, is shown in Chapter 2. The leader of the mapping was Thomas Bugge (1740–1815) whose published works show the state of the art of geodesy at this time.

In Chapter 3 the cartographical work of Wessel is shortly discussed. He continued Bugge's triangulation but used a much more rational system for the division of the maps.

Finally, in Chapter 4, we mention the improvements after the epochs of Bugge and Wessel, when the topographical survey became the responsibility of the General Staff of the army. Again the project was copying an earlier French project, but Wessel's rational division of the maps was continued and in some respects the maps even surpassed the French archetype.

2 The epoch of Thomas Bugge

In 1762, the Royal Danish Academy started an 80-year work on the mapping of Denmark in scale 1:120000 by engaging two surveyors, one of whom was Bugge. At the age of 19 he had assisted in a plane table survey where the position of the table was determined by a triangulation on the table itself. This gave the dearly bought experience that error accumulation rapidly distorted areas at a distance from the base lines. After this Bugge became a devotee of what he called "the method of parallel lines". Here parallel lines were marked out by sticks in the field at a distance of 10000 alen[1] and measured by a chain. The intention was that these lines should serve as a frame and be stations for the tables. In Zealand the parallel lines were as long as 200000 alen and aligned by sticks at a distance of 30–70 alen, and by higher signals at distances of the order of 1000 alen. The longest lines thus required the placing of a stick about 4000 times!

Bugge discussed observational errors very carefully. He distinguished between random and systematic errors, and estimated instrumental errors by the widths of threads and division lines. He also made psychophysical experiments [4, p.16] with different people to determine the visibility of signals under different illuminations and determined the resolution power of the

[1]Measures used: 1 alen = 2 fod (Rhine or Leyden feet) = 0.6277 m. 1 favn ("fathom") = 3 alen = 1.8831 m. 1 mil = 12000 alen = 7.532 km.

human eye to $\pm 1'$ in clear sunshine (from behind) and to $\pm 2'$ under less favourable conditions.

When possible, Bugge, after critical discussions, combined several independent determinations of the basic parameters. For the latitude of the Royal Observatory in Copenhagen (the "Round Tower") he even included Jean Picard's approx. 100 years older data. If the critical examination did not indicate that some data should be rejected, he used simple averages. He warns explicitly against the use of "raisonnerte" averages or a preference for values which happen to appear twice. Very reasonably he argues that, by chance, such values could be most in error [1, p.85].

Due to his expertise, Bugge was ordered by the king [5, p.12] to be a member of the International Commission on the Metric System. He pointed out that the definition of the metre in terms of the meridian quadrant could, at best, be realized to ± 0.1mm. Today we know he was right: the prototype metre is actually 0.2 mm too short! On a commission with starry-eyed theorists Bugge represented common sense. Among the radical proposals of the Commission were, for instance, that a day should have 10 rather than 12 hours, a mariner's compass point should be 1/10 rather than 1/8 of a right angle, and even the currency should be reformed to the metric system (1 franc = 5 g 90% silver). With his great practical experience Bugge pointed out that such reforms would cause much confusion, be very expensive and take a hundred years to be accepted and in common use. Fortunately, in his opinion, there was no need to introduce this system in Denmark. From the Elbe to Nordkap we had, since 1682, a uniform system of weights and measures [5, p.539] which was constructed by Ole Rømer, one of his predecessors as Astronomer Royal.

Despite his sound attitude to a theory of errors, Bugge badly lacked our days' statistical theory for the accumulation of errors. From 244 repeated weighings [5, p.632] he found, as mean value, 1 kg = 2.002769 Danish pounds and he estimated the error to be less than ± 0.000002 pound, but the concepts of "probable" — or "mean errors"— could not be used as they were not yet known to Bugge.

The want of a statistical theory of errors is clearly illustrated in his discussion of the accuracy of staking out straight lines [4, p.20]. The sticks should be sighted equally from both sides "so that the small errors would be now on one side, now on the other, and hence, ultimately the line will approach mathematical accuracy". If trees were in the way of the line they should be passed by parallel lines **alternatively to the right and the left** of the obstacle. These displacements were allowed because the diameter of a tree is too small to be drawn in the scale of a map!

Assume a sighting error $\pm 2'$ and a 1000 alen distance between the signals. Bugge is aware that each stick introduces an error so their number should be kept small by making the distances between them as large as possible. He could not know that the transverse error would increase as the inverse square-root of this distance and, for a main line across Zealand of 200000 alen, ultimately become:

$$2 \times \frac{2'}{3438'} \times 200000 \times \sqrt{\frac{200000}{3 \times 1000}} = \pm 1900 \quad \text{alen} \tag{1}$$

corresponding to ± 10 mm on the final map. Bugge was aware of an intolerable error but was so confident in his alignment method that he ascribed it to the 1/2–1% shrinkage of the paper when the draft maps in scale 1:20000 are cut from the plane tables. With his usual care, [1, p.5] this shrinkage was investigated as a function of time and humidity.

We cannot tell if it was expected that the "method of parallel lines" could stand alone and only needed a triangulation as a supplementary check. The original plan from 1761 [1, p.VIII] included a supporting triangulation, but the actual work started **after** the plane table survey. The sides of the triangles should not be less than about 2–3 mil and their angles were measured with a full circle with radius 1 fod constructed by Johannes Ahl (1729–1795)[2]. In a critical discussion of this instrument [1, p.21] Bugge estimated its accuracy to 8–10″, in good agreement with a mean error $\pm 9''$ estimated from the sum of the angles in his triangles.

Bugge's 80 main triangles on Zealand mainly followed the coast line and avoided the central part of the island. Angles less than 30° were measured several times if they could not be avoided. The angles were adjusted by judgment so that their sum in each triangle became 180° exactly. If the angles were considered equally well determined they obtained the same correction.[3] Haze or bad sightings during observations resulted in larger corrections. Small angles obtained smaller corrections, maybe due to the greater

[2]See Olov Amelin's paper in this volume.

[3]These corrections are due to observational errors. Legendre's theorem, published in 1772, was not used by Bugge. The spherical excess correction in Bugge's largest triangle, No. 79 [1, p.75], is only 0,7″. Incidentally, no correction at all was made in this triangle No. 79. This indicates that one of the angles was computed.

care in their measurement. A significant number of triangles (14 of 80) are not adjusted, indicating that one angle may have been computed. Not all geometric constraints were satisfied by this procedure.[4] It appears, however, to be a very reasonable method prior to the invention of the method of least squares.

Without any estimates of the degree of approximation involved, Bugge considered the Earth flat in his computation of the triangles. A justification he could have had in mind is the small difference between arcs and chords. In his textbook on surveying [4, p.2] he compares an arc $s = 7.505$ geographical miles on a sphere with radius $R = 860$ miles with the corresponding chord. Curiously enough he finds the difference arc minus chord to be 0.000124 miles, or five times the correct result 0.000024 ($= s^3/24R^2$). This error is due to excessive roundings and appears unchanged in the 1795 and 1814 version of the text.

Let us now prove that the flat Earth approximation can be justified for Bugge's task of producing a topographic map. This will be done by the useful, though rarely used, "gap theorem" probably formulated and proved here for the first time:

From a point O on a spheroid a traverse to a point P is made along geodesics through $A, B, \ldots$. The corresponding distances and angles, as measured on the spheroid, are then plotted in the plane as shown in Fig. 1. Let a different route from O to P follow $A', B', \ldots$. In the plane the end-point P' is different from P. If the area bounded by the two traverses is small then the gap PP' is obtained up to terms of a higher order by rotating P an angle ε around the centre of mass of the polygon $OAB \ldots PP' \ldots B'A'$ with area ω. The angle ε is equal to the excess ω/MN, where M denotes the radius of curvature of the meridian and N denotes the other principal radius of curvature. This theorem is proved in the appendix. Often M and N were multiplied by $\pi/180$ to give the length of a degree. This notion, which has given name to an entire branch of science, "measurement of degrees", was preferred by Bugge [1, p.104].

Zealand is about 90 by 130 km, $M \simeq 6379$ km and $N \simeq 6393$ km. The excess is $\varepsilon = 90 \cdot 130/(6379 \cdot 6393) \simeq 2.9 \cdot 10^{-4}$ rad. The maximal distance from

[4] For instance, the sum of angles covering the entire horizon around the trigonometric station "Hellingehøj ved Høje-Taastrup" (denoted H by Bugge in triangles Nos. 3, 4 and 6 [1, p.64]) is 10″ less than 360°.

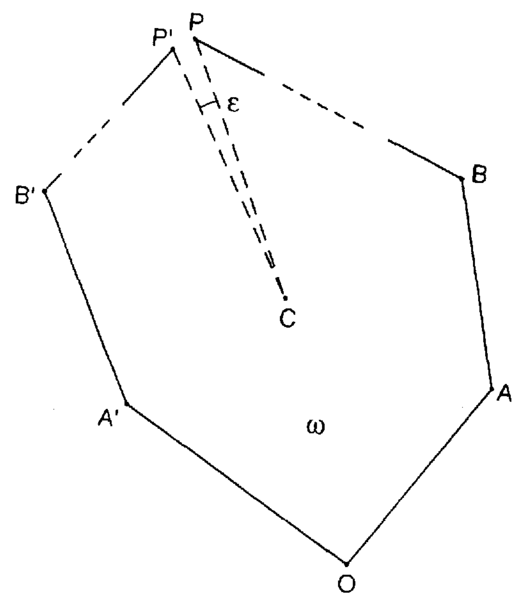

Figure 1: On a spheroid, with principal radii of curvature M and N, we follow a polygonal line from O to P through $A, B, \ldots$ and a different one through $A', B', \ldots$. The figure shows the tracks plotted in the plane with the conservation of angles and distances. The end-points P and P' will not coincide. The area of the closed polygon $OAB..PP'..B'A'O$ is ω and the excess is $\varepsilon = \omega/MN$. The centre of mass of the polygon is denoted by C. The gap PP' is the excess ε times the distance PC and is orthogonal to PC in the approximation (A.6).

the centre to an edge is 79 km so the gap (PP$'$) becomes $2.9 \cdot 10^{-4} \cdot 79000$ m $= 23$ m. This corresponds to $1/5$ mm on the final map which is hardly visible.

The example above is an extreme one and the excess ε will also be reduced because the sums of the angles in the individual triangles are adjusted to exactly 180°. Even today mathematical ambiguities are justified in practice if the errors are negligible for the purpose in mind or hidden by observational errors. This is for instance the case for the present cadastral coordinates in "System 1934".

The result of the computations in the plane was two coordinates: the distance x to the normal orthogonal to the meridian through the Round Tower and the distance y between the foot point and the Round Tower along this "perpendicular". These coordinates are not defined uniquely when computed in the plane but depend on the chosen path. In view of the observational errors these inconsistencies will hardly be noticed if the different paths cover small areas. The coordinates were used as rectangular coordinates in the plane and the draft maps adjusted to these points. Although the concept of a map projection was not mentioned this implies a projection of the Cassini-Soldner type.

The Earth could not be considered flat, however, in the computation of geographic latitude φ and longitude λ from the plane coordinates (x, y). Referring to Bouguer, Bugge used a non-elliptical spheroid with flattening 1:179 for the Earth. Its figure is defined by a table of the length (M) of a meridian degree and of the normal (N) as a function of φ.[5] The adopted geographical position for the origin $(x, y) = (0, 0)$ at the Round Tower was $\varphi_0 = 55°40'56''$, $\lambda_0 = 0°$.

To derive (φ, λ) from (x, y) is today a standard problem in geodesy and solved by an expansion in e^2, where e is the eccentricity of the ellipsoid. When the Earth is not an ellipsoid we must integrate the differential equation for the geodesic curve with arbitrary M and N. This requires a sharp definition of the rectangular coordinates not given — but implicitly used — by Bugge himself.

Expanding in terms of the small quantities x/M, y/N, $N' = dN/d\varphi$, and $M' = dM/d\varphi$, we have to third order:

$$\varphi = \varphi_0 + \frac{x}{M + \frac{x}{2M}M'} - \frac{y^2 \tan\varphi_0}{2MN} - \frac{xy^2 \tan^2\varphi_0}{2MN^2} \tag{2}$$

and to second order:

$$\lambda = \lambda_0 + \frac{y}{(N + \frac{x}{2M}N')\cos(\varphi_0 + \frac{x}{M})}. \tag{3}$$

If r denotes the distance along the geodesic and the polar angle α denotes the angle between the geodesic at the origin and the meridian, we have

$$x = r\cos\alpha \quad \text{and} \quad y = r\sin\alpha. \tag{4}$$

[5] In modern notation: $M = p + q\sin^4\varphi$, $N = p + q(1 - 2/3\cos^2\varphi + 1/5\cos^4\varphi)$; $p =$ 58756.14 favn, $q =$ 992.86 favn. $N > M$. The figure of the Earth was defined by M as given by Bouguer (1698–1758) [1, p.105]. N is derived from M and the flattening is: $5q/(15p + 8q) \simeq 1 : 179.1$. The numerical values of p and q are here adjusted to Bugge's table [1, p.104].

Formulae (2), (3) and (4) thus give a parameter representation of the geodesic in terms of the distance r.

Such formulae, or similar ones, were not derived by Bugge. It was not his style and he says explicitly that he did not "fill the book with theorems and formulae, which after my conviction, and long experience, are of no use and cannot be utilized in practice" [4, p.VII]. He always solved the problems by some kind of reasoning and geometric intuition.

The first term in (2), the distance x divided by the length of a meridian degree (M), seems obvious. The intuition does not, however, indicate that M should refer to the mean latitude. The next, y^2-term, is the distance between the perpendicular and the latitude circle and is given by a table with 12 entries [1, p.106]. This table is not smooth, the second differences have a random variation of the order $\pm 2''$. It was probably computed from 12 right-angle triangles on a sphere with 6-figure logarithms and given, in Bugge's habit, to only $1''$. The angle $y \tan \varphi_0 / N$ between the meridian and y-axis (meridian convergence) increases with y. The cosine of this angle must be applied on the main term to project it onto the axis. This gives the last third order term which is neither estimated nor mentioned by Bugge. An example [1, p.106] gives the details of the computation of the latitude difference between Copenhagen and the Lighthouse at Kullen. The mean latitude is 56.0° but the M used corresponds to 57.0° which is outside the range of latitudes. This is only one of several examples where Bugge did not interpolate but used tabular values, and not even with the nearest argument.

The computation of the longitude λ was not performed by the simple formula (3) but by a rather unwieldy method. First r and α were computed from (x, y) by (4) and hereby defining the precise meaning of these coordinates. The length (R) of 1° of the oblique arc with polar angle α was then computed by an approximation to Euler's formula

$$\frac{1}{R} = \frac{\sin^2 \alpha}{N} + \frac{\cos^2 \alpha}{M} \tag{5}$$

Now let $P = (0, 0)$ be the origin at the Round Tower, $S = (x, y)$ be the station in question and Z the North Pole. Bugge now imagined a spherical triangle PZS with sides $SZ = 90° - \varphi$, $PZ = 90° - \varphi_0$ and $PS = s = r/R$. The longitude is then computed as $\lambda = \angle PZS$. The problem is, however, that there is no such spherical triangle because the normals at P and S do not intersect the Earth axis in the same point due to the non-spherical spheroid and are not in the same plane. Nevertheless, the procedure gives some approximation to (3). The triangle PZS gives in modern analytic notation:

$$\begin{aligned}\left(\cos\frac{\varphi+\varphi_0}{2}\sin\frac{\lambda}{2}\right)^2 &= \sin^2\frac{s}{2}-\left(\cos\frac{\lambda}{2}\sin\frac{\varphi-\varphi_0}{2}\right)^2 \\ \simeq \frac{1}{4}\left(s^2-\frac{x^2}{M^2}\right) &= \frac{1}{4}\left(\frac{r^2}{R}\left(\frac{\sin^2\alpha}{N}+\frac{\cos^2\alpha}{M}\right)-\frac{x^2}{M^2}\right) \\ = \frac{y^2}{4RN}+\frac{x^2}{4M}\left(\frac{1}{N}-\frac{1}{M}\right)\sin^2\alpha &= \frac{y^2}{4N^2}\left(1-\cos^2\alpha\left(1-\frac{N}{M}\right)^2\right)\end{aligned} \tag{6}$$

The first line in (6) is derived from the spherical cosine-relation and is the exact form of the familiar approximation $s^2 = (\varphi-\varphi_0)^2 + (\lambda\cos\varphi)^2$. Expanding in small quantities and inserting (4) and (5) gives — after some reductions — the final term.

For $M \sim N$ this is an approximation to (3) but in the limit $r \to 0$ it does not converge correctly to this expression. The method does not represent a lasting solution to a main problem in geodesy.

An example with $y = 66850$ favn, $\varphi = 55°01'34''$ and $N = 59568$ is given in detail in [1, p.110]. Bugge's inconvenient method gives $1°57'29''$ while formula (3) simply gives:

$$66850/(59568 \times \cos 55°01'34'') = 1°57'28'' \tag{7}$$

We note that N refers to the mean latitude but the argument of the cosine in (3) refers to the actual latitude — a fact not evident by intuition. Again, in this example Bugge does not use the mean latitude (55.4°) but 56.0° outside the range.

Evidently Bugge did not waste time over such trifles. His goal was the printed maps and for economy and speed the tolerances were adjusted to this purpose. Bugge actually achieved his goal and the field work was finished in 1821, a few years after his death.

3 Caspar Wessel

From the very beginning it was the duty of every surveyor also to train apprentices in the field. One of Bugge's first apprentices was an elder brother of Caspar Wessel. From 1764 Wessel was himself involved in the work as his brother's assistant.

We have seen above the kind of theory and the standard of applied mathematics which met Wessel. There seems to be little stimulation for a gifted mathematician. There was no "higher mathematics" in the sense of analysis, differential equations, series expansions or so forth. Such topics were not a part of Bugge's curriculum — on the contrary! In his text-book [3, p.15] Bugge criticized the physicist H.C. Ørsted for using differential calculus as being both "superficial" and "ill-timed", because the problem in question could be clearly explained in more familiar terms.[6] Bugge's main tools remained the 6-7 figure logarithmic tables and trigonometry.

Wessel surveyed in the field during the summer and compiled the maps in 1:120000 from originals in scale 1:20000 during the winter. The first map, with his name on it, was finished in 1768 and covered NE Zealand. In rapid succession appeared the three maps covering the remaining parts of Zealand. Finally, in 1777, the four maps were put together to a single map in 1:240000.

In 1779 Wessel was appointed trigonometrical observer with the duty to push the triangles further west in order to cover the whole of Jutland to the Elbe, as Slesvig and Holstein were in those days in personal union with Denmark. When Bugge wrote his book [1] others had succeeded him in the field. The westernmost trigonometric station reached was denoted "Heslebierg" (nowadays "Hesselbjerg") in Jutland $3°11.5'$ west and $1.6'$ south of Copenhagen. Bugge erroneously placed this station on the island of Funen [1, p.113].

When Wessel took over and started from there he proposed a plan for the continuation of the work which was approved by The Royal Danish Academy of Sciences and Letters at a meeting on February 5 1779 [13, p.193]. This plan for the division of the country is often mentioned, among others, in a biography [16], but its content is not known [11, p.106]. Let us now try to reconstruct this plan from the distinct appearances of the published maps before and after Wessel was in charge of the triangulation.

[6] The university professor criticizing the work of a colleague in his text-books must have amused the students. Bugge also warns against using Ørsted's value for the length of the new French metre in terms of the Danish unit fod [3. p.132]. After his death, Bugge was himself criticized by the influential Ørsted brothers; the physicist's brother became prime minister.

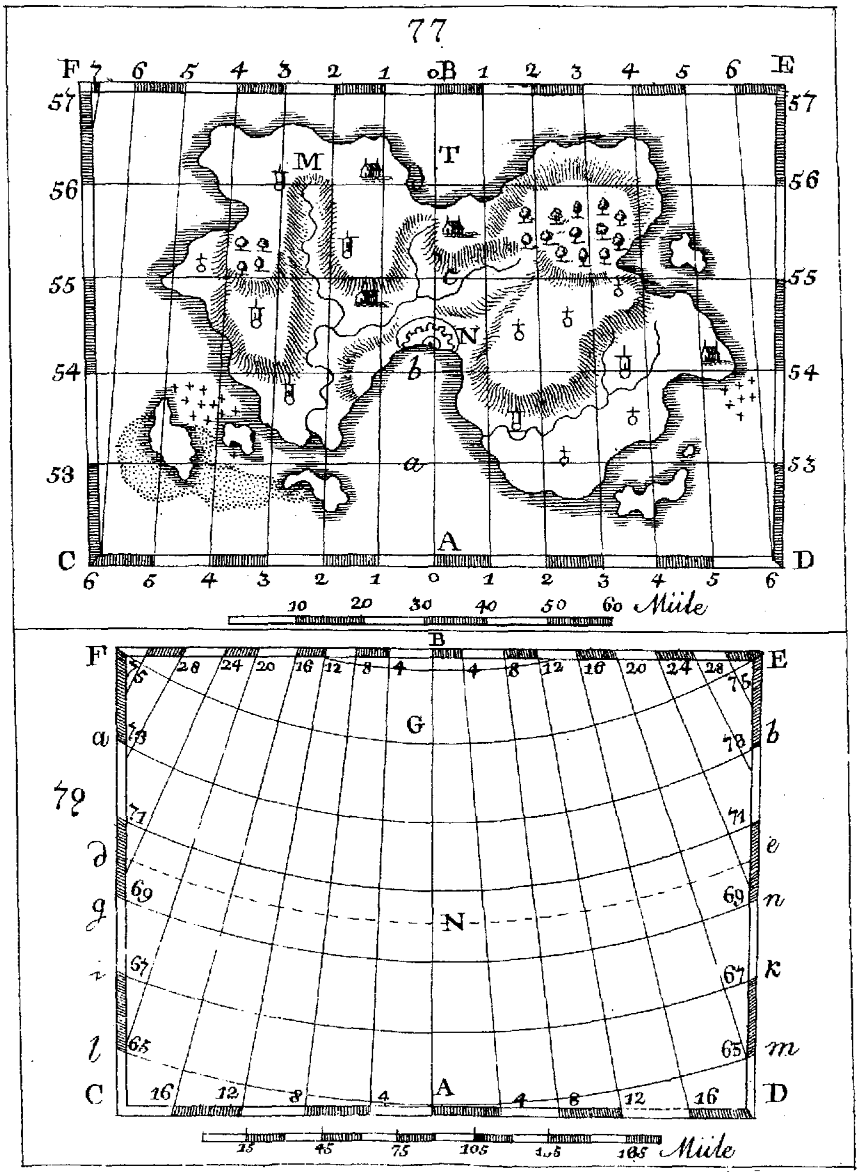

Figure 2: The copperplate of Thomas Bugge's textbook (ref. 2, copper XII) shows the outline of his concept of a map. The central meridian is AB, to which the borders ED and FG are parallel and the North and South borders FE and CD are orthogonal. Due to the meridian convergence such rectangular frames cannot be joined together with neighbouring sheets at their edges.

Bugge's maps had a rectangular shape which was symmetric relative to a central meridian (AB in Fig.2). The central meridian and the two latitude parallels (CD and EF), which were practically straight lines, were in true scale (Flamsteed projection). Due to the meridian convergence the upper boundary covers $1.1'$ more in longitude than the lower. A main point is that the sheets could not be joined together at the border lines due to an overlap and the non-parallel rectangular frames. The centre of Zealand, as covered by the four maps, is shown in Fig.3. It appears very confusing with a small area having as much as three times overlap [1, p.128].

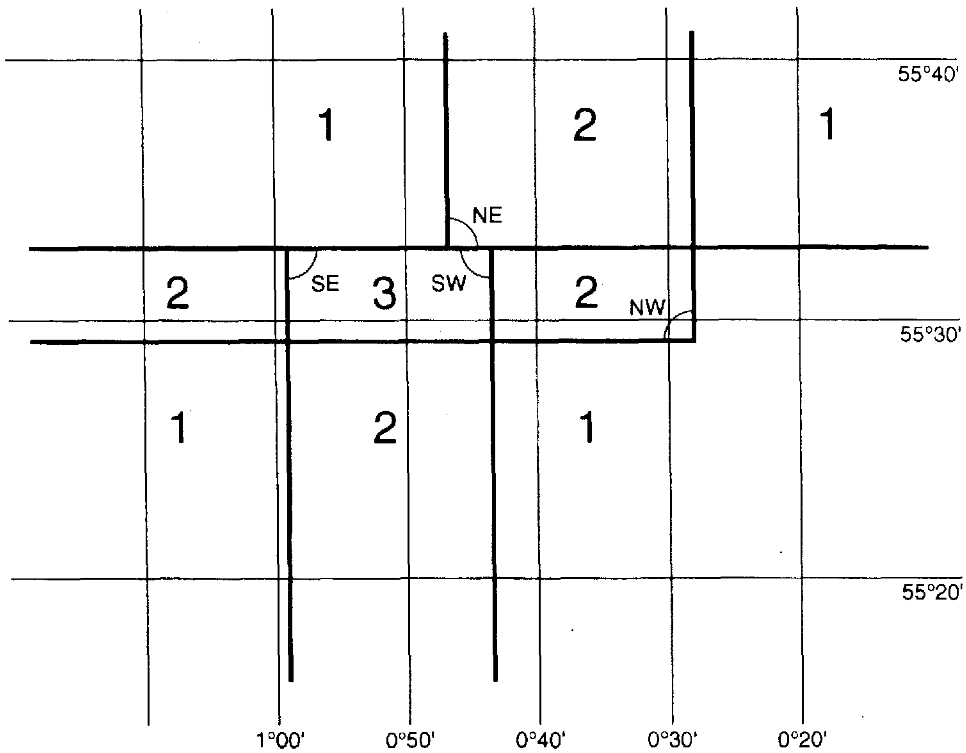

Figure 3: Zealand is covered by 4 maps (NE, NW, SE and SW) and the figure shows how they overlap. The numbers of overlaps (max. 3) are indicated. The maps are also slightly twisted due to meridian convergence. The appearance is confusing but Thomas Bugge made the best use of the given paper size ("Grand Aigle") and the scale 1:120000. Caspar Wessel's frames were equal rectangles (at first $13\frac{1}{2}$ by $7\frac{1}{2}$, later $13\frac{1}{2}$ by 8 mil^2) fitted together on both sides of the meridian $3°12'$ W of Copenhagen as a common border.

The new maps were divided by a rational system, and Bugge mentions [1, p.VI] that it was decided that the future maps could be joined together. Inspection of the published maps shows that their east or west borders are $3°12'$ west of Copenhagen. This is exactly the meridian through the trigonometric station Hesselbjerg mentioned above. This division is also alluded to several times in the literature [11, p.67,107,131]. The north and south borders are then not latitude parallels but orthogonals to the $3°12'$ W meridian. The latitude scales of the east and west borders are then not identical but displaced by the y^2-term in (2), or $0.66'$.

This rational system was probably invented by Wessel in 1779 and ever since rectangular maps, which could be joined, were used in Denmark. This idea may have been Wessel's main lasting contribution to cartography. His actual field work was necessarily surpassed. However, the production of maps which cannot be joined is continued elsewhere, for instance the Prussian polyeder projection and the International Map of the World.

In 1805, bowed down by age (59 years!) and feebleness Wessel applied for retirement with full salary [13, p.199]. His work had always had a high reputation and when in 1808 the French, then allied to Denmark, asked for the survey of Slesvig and Holstein Wessel undertook the copying. Actually the French only received the Holstein data. By a diplomatic excuse the king avoided forwarding a copy of the Slesvig survey! Wessel renounced the pay and received a silver-medal, books and maps instead [13, p.413]. The intention of Napoleon's France was to have maps from Trieste to Slesvig [11, p.156].

4 Developments after Bugge and Wessel

During the last part of Bugge's and Wessel's active working periods there was an increasing demand for better maps. All over Europe the enclosure movements in agriculture demanded large scale cadastral maps and the armies wanted relief maps with more topographical details.

Napoleon once said that a good map was half the victory. In 1808 he planned a new atlas of France, in scale 1:80000, to replace the old Cassini maps. The work on this "Carte de l'État Major" started after his fall in 1815 and continued until 1880. It was using Bonne's projection (after Rigobert Bonne (1727-95), in Denmark denoted: "modified Flamsteed projection") with the central meridian through the Paris observatory. The southern latitude (45°) of osculation reflects that Italy and Spain were within Napoleon's sphere of interest. Occupied German-speaking areas, like Hanover, were surveyed by the French army. As we have seen, Wessel provided similar data for Holstein.

Already during Bugge's last years, plans were made by the Danish General Staff to prepare improved maps based on the original measurements of the Royal Academy, [9, p.24]. In 1842 the Academy stopped the production of maps and transferred the archives to the General Staff which had now planned to produce an atlas in 1:80000 based on a quite new triangulation. Like the French, the projection should be Bonne's with the central meridian 2°12′ west of the Round Tower and osculation at 56°N. The relief was finally shown by equidistant level curves.

The French map in 1:80000 did not stand the test of the First World War. The trenches were so close that they could not be shown with accuracy on any scale smaller than 1:20000 so large scale maps were redrawn [14, p.255,261]. The generalization errors inherent in the signatures became too large for the artillery and the British expeditionary force had to re-survey their entire front lines. For this work a conformal projection would have been an advantage. The Bonne projection, however, has a shear

$$(\varphi - \varphi_0) \times (\lambda - \lambda_0) \times \cos\varphi_0 \tag{8}$$

which amounts to 16′ in the disputed area of western Germany and which was inadmissible in computations. According to Deetz: "In the rigorous tests of the military operations these errors became too serious for the purposes which the maps were intended to serve" [6, p.71]. The conical conformal Lambert projection was used in a later French survey and by learning this the Americans became enthusiastic supporters of conformal projections: "the excellent qualities of the Lambert projection, although not unknown, had been systematically forgotten for some time and were evidently not fully appreciated until the beginning of the First World War" [6, p.213,90].

However, in this respect the Danish survey had been far ahead. The theory of the Lambert projection on an ellipsoid appeared in 1868 [14]. About this time the survey by the General Staff was finished on the Danish islands and was about to start in Jutland. As previously this was taken as an opportunity for improvements and the conformal Lambert projection was introduced.

5 Conclusion

The geodetic work of Bugge and Wessel was destined to become obsolete already during their lifetime. The revolution of geodesy was then already in the **mind** of C. F. Gauss. In 1799 a letter [7, p.136] by Gauss to the editor of "Allgemeine Geographische Ephemeriden" (F. von Zach) treated the determination of the figure of the Earth by several arcs of the meridian combined by the method of least squares. Exactly the same data were simultaneously analysed by Bugge [5, p.586-588]. In a letter to Schumacher [8, p.345] from 1816 Gauss offered to compute the Danish first order triangles by his own (read: least squares methods). In the same letter he also proposed the problem of conformal projections for a prize essay — which he won himself in 1825. Incidentally he mentioned in the letter the great advantage that Denmark had already been surveyed once — but an advantage only in connection with the reconnaissance for trigonometric stations! He complained about this time-consuming work and that he could not obtain similar data from the French survey of Hanover.

There is a vast contrast between the good workmanship of Bugge and Wessel, which we have seen above, and the mathematical genius of Gauss. The importance of the work of the latter can only be put in the proper perspective by a comparison with that of his predecessors. We have seen how badly Bugge needed a statistical theory of errors and he explained [5, p.580] how the repartition of specific errors was considered quite arbitrary and was a matter of debate between the members of the International Commission of the Metric System. Gauss practically removed this arbitrariness and introduced rigour in the entire theory.

6 Appendix: Proof of the gap theorem

Bugge's triangles were computed in the plane. This was done at that time also in France and some German states. We shall here estimate the approximation of this procedure. The first rigorous definition of rectangular coordinates on the sphere was introduced by J. von Soldner (1776–1833) in 1809 in a survey of Bavaria. Let y, counted positive towards the east, be the shortest distance AA' from a given point A to its projection A' on a chosen prime meridian and x the distance from A' to a fixed point on the meridian measured positive towards the north.[7] From the point $A = (x, y)$ we now go to B by a distance s to a point B along a geodesic that at A makes an angle α with a curve equidistant to the prime meridian. Reference 10, p.265 (modified here by a generalization to a spheroid, essentially a substitution of r^2 by MN) gives for the Soldner coordinates of $B = (x', y')$:

$$x' = x + s\cos\alpha + sy^2\cos\alpha/2MN, \quad y' = y + s\sin\alpha \tag{A.1}$$

and the direction at the end-point

$$\alpha' = \alpha - sy\cos\alpha/MN. \tag{A.2}$$

If the same distance s is drawn as a straight line in the plane the endpoint will have the coordinates

$$x' = x + s\cos\alpha, \quad y' = y + s\sin\alpha$$

and the angle α will not be changed at B. We now consider AB as being a part of the traverse $OAB\ldots P$. By stretching s we have displaced the remaining part of the traverse by $(sy^2\cos\alpha/2MN, 0)$ and we have rotated it by an angle $sy\cos\alpha/MN$. This will displace the endpoint $P = (x_p, y_p)$ by

$$(sy^2\cos\alpha/2MN, 0) + sy\cos\alpha/MN \cdot ((y_p - y), -(x_p - x)) \tag{A.3}$$

Thus if we replace the entire geodesic polygon by its plane counterpart we can obtain the total displacement of P by integrating the expression (A.3) along the polygonal path $OAB\ldots P$. Setting $dx = s\cos\alpha$ we get:

$$\begin{aligned} & \frac{1}{MN}\int_o^P dx((yy_p - y^2/2), -y(x_p - x)) \\ = \; & \frac{1}{MN}\int_o^P dx \int_o^y dy((y_p - y), -(x_p - x)). \end{aligned} \tag{A.4}$$

If we subtract the corresponding integral for the alternative route $OA'B'$ $\ldots$ P we obtain a double integral bounded by the closed figure with area ω:

$$\frac{1}{MN}\int_\omega dx \int dy(y_p - y), -(x_p - x)). \tag{A.5}$$

[7]Note: Soldner's coordinates form a **left-hand** system.

Introducing the centre of mass coordinates (x_c, y_c) this integral is easily evaluated to the rotation ε around the centre of mass:

$$\varepsilon\left((y_p - y_c), -(x_p - x_c)\right), \tag{A.6}$$

where $\varepsilon = \omega/MN$ is the excess.

7 Addendum

At the Wessel Symposium I learned that Bodil Branner and Nils Voje Johansen had procured Wessel's original handwritten reports from the archives of "Kort- og Matrikel Styrelsen". The notes for 1787 describe the plan from 1779 mentioned above. What was concluded above from the published maps is essentially confirmed. The puzzling increase of the rectangular frames of the maps from $13\frac{1}{2}\times 7\frac{1}{2}\,\text{mil}^2$ to $13\frac{1}{2}\times 8\,\text{mil}^2$ is, however, explained in Wessel's notes. The country extended farther north than expected in advance of the survey! The spheroid with flattening 1:179 used by Bugge was replaced by an ellipsoid with flattening 1:230. This ellipsoid, based on older pendulum observations by Richer, is mentioned by Bugge [2, p.284]. Wessel's concepts of map projections do not seem clarified. It seems that he computed rectangular coordinates by zigzagging along narrow strips on the ellipsoid and then unfolded the strips in the plane. The computations were made by imaginary numbers and took into account the meridian convergence ignored by Bugge. The method was affected by the ambiguities described by the above "gap theorem".

8 Acknowledgement

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Heinrich Christian Schumacher — mediator between Denmark and Germany; Centre of Scientific Communication in Astronomy

Jürgen Hamel *

1 Communication in Astronomy

The structure of scientific communication in the field of astronomy has undergone manifold changes during the past 500 years. The main factors determining this process are the following:

1. The number of persons interested in scientific communication,
2. The amount of material suitable for scientific communication,
3. The traffic situation for scientific communication as far as speed, security and cost are concerned.

For centuries two forms of communication have been of great importance:

1. correspondence, and
2. academic lectures passing new points of view about scientific facts from master to student. Since important universities recruited students from a wide geographical area, and since students used to migrate from university to university, hand written lecture notes circulated among them, providing thereby efficient structures of communication.

The exchange of letters was of less importance. As an extraordinary example in this respect the letters exchanged between Johannes Regiomontan and Giovanni Bianchini in 1463 should be mentioned as a game of questions and

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answers measuring mental and creative powers. 120 years later a historically very important correspondence between Wilhelm IV, landgrave of Hessen in Kassel, and his astronomer Christoph Rothmann with Tycho Brahe took place.[1] Supported by Wilhelm, Tycho Brahe on May 23, 1576 had been granted the island of Hveen by his feudal lord the Danish King Frederik II in addition to 400 thalers for the construction of an observatory.[2] The letters exchanged between Hveen and Kassel between 1585 and 1590 were apparently delivered by Brahe's disciples when they travelled back and forth from the book fair in Frankfurt. They are still an important source for the history of astronomy.

Nevertheless the need for scientific communication was still very limited, except on special occasions such as the introduction of the Gregorian calendar in 1582 and the subsequent discussions in the Protestant countries, as well as the appearance of comets, eclipses or other celestial phenomena. On such occasions opinions and ideas were exchanged by letters or small printed volumes of high circulation and low price.

A hundred years later, by the end of the 18th century, firm structures of research and communication in the field of astronomy had been established. There existed a number of scientific research institutions of international importance and reputation. Among the earliest were the observatories in Greenwich, Paris, Copenhagen and, a bit later and as such rather modest, in Berlin. In Copenhagen an observatory was set up already under Brahe's student Christian Severin Longomontanus, author of the recognised manual *Astronomia Danica* (1622). This observatory became really famous under Ole Rømer, who had been able to measure the speed of light very precisely. He constructed and used new instruments, as for instance the prototypes of circle instruments which after 1750 led to the great success in celestial mechanics.

About 1750 astronomy with its modern structure was represented at universities and academies. Since practical and theoretical astronomy was practised in many places there was a great demand for higher quality in the exchange of observations and their mathematical treatment. In addition,

[1] Hamel, Jürgen: *Die astronomischen Forschungen in Kassel unter Wilhelm IV.* Mit einer Teiledition der deutschen Übersetzung des Hauptwerkes von Copernicus um 1586. Thun; Frankfurt a. Main 1988 (Acta Historica Astronomiae; 2)

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the changed social demand for astronomy had to be taken into consideration. It was no longer astrology, and casting of horoscopes, that brought high prestige to astronomy. The interest predominant came from the determination of the latitude and longitude at sea, surveying and improving the calendar. In addition, astronomy gained public interest due to Newton's physics, the very precise methods of orbit calculation and the new results in comet research, reflected in ideological, philosophical and theological systems.

As long as astronomical research was rather sporadic and the individual scientists did not have to discuss and agree with each other very much, the number of persons interested in communication had remained very small. This was now changing profoundly. Correspondence as a means of exchang information started to play an important role; membership in a circle of corespondents was of great importance as a pre-condition for one's own scientific research as well as for propagating it. Of course, all this went alongside the publications of the academies, independent printed matter and the first scientific journals, often founded in relation to scientific academies.

For the German language area the yearly *Ephemeriden* by Gottfried Kirch, published since 1681, should be mentioned. They had a literary annex where Kirch used to publish research of his own as well as results which he learned about in letters from his correspondents or in new books. Kirch continued these literary annexes in his calendars published by the Berlin Academy after 1700 and the tradition was carried on by Johann Heinrich Lambert and Johann Elert Bode in the *Berliner Astronomisches Jahrbuch* founded in 1774.[3]

The publication of an astronomical annual supplied a demand among astronomers. As a proof one should mention the great number of contributions that flooded Bode, who could hardly manage the contributions. Almost all contemporary astronomers were among his authors. He usually published their writings in the form of "From a letter from Mr. N.N. to the publisher". Nevertheless, a real handicap was the lack of a fast outlet for information due to the annual publication only.

[3]Hamel, Jürgen: *Geschichte der Astronomie. Von den Anfängen bis zur Gegenwart.* Basel 1998, 271; Herrmann, Dieter B.: *Die Entstehung der astronomischen Fachzeitschriften in Deutschland (1798–1821).* Berlin-Treptow 1972 (Archenhold-Sternwarte / Veröff.; 5), 15–74

This shortcoming was made up by Franz Xaver von Zach, whose work was actively supported by the Duke Ernst II of Sachsen-Gotha. In 1798 Zach founded an astronomical-geographical journal, the monthly *Allgemeine Geographische Ephemeriden*. The combination of astronomy, cartography and geography, however, turned out to be a failure very soon. Despite all the connections between these two disciplines mainly through the geographical determination of latitude and longitude, they had progressed in their specialisation to such an extent that they had to be covered by special journals.

Therefore, in 1800 Zach founded the *Monatliche Correspondenz zur Beförderung der Erd- und Himmelskunde*, a specialist journal of astronomy where geography was included mainly in so far as it was relevant for the geographical determination of latitude and longitude. With a change when Bernhard August von Lindenau became the publisher the *Monatliche Correspondenz* existed till 1814 as the central means of communication between the astronomers next to the literary annex of Bode's annual. It was discontinued because Zach went to France to found a new journal, the *Correspondance Astronomique*, which was published from 1818. In the beginning it contained papers from well-known astronomers, among them the young Heinrich Christian Schumacher. However, the journal could not gain real importance. It published articles in French and Italian only, Genua, where it was published was too far from the centres of astronomy, and the postal service was insecure due to political turmoil. Publisher Zach soon became isolated and in 1826 he stopped its publication.

At the same time Lindenau together with Johann Gottlieb Friedrich v. Bohnenberger published the *Zeitschrift für Astronomie und verwandte Wissenschaften*, with a circulation of 200 copies and strongly concentrated on astronomy. They received contributions from the most important scientists, but the two co-publishers did not collaborate very well. Moreover, Lindenau found himself in a difficult situation, being a state employee of Saxony with manifold political duties (he later became state minister of Saxony). The journal was discontinued in 1818, only three years after it was founded.

These detailed introductory remarks are necessary in order to understand the problems of scientific communication and the difficult problems of the foundation and maintenance of an astronomical journal. There was the need and demand, and whenever a publication was stopped it was regretted very much because the astronomers lost the possibility to publish their results of research. But nobody was ready to undertake the troublesome efforts of a publisher — neither Gauß nor Olbers, Encke or Bessel.

2 Heinrich Christian Schumacher

Now the time has come to turn to Heinrich Christian Schumacher.[4] Schumacher was born in 1780 in Bramstedt/Holstein near Hamburg as a son of the chamberlain and Danish senior civil servant in Segeberg Andreas Schumacher. His father had apparently a close relation to the King. Indeed, Heinrich Christian was introduced to King Frederik VI at the age of 7.[5] This acquaintance turned out to be important both for Schumacher and for astronomy, in so far as the well-educated King till his death protected and promoted Schumacher in many ways.

After Schumacher had lost his father at the age of ten, he was educated by the erudite pastor J.F.A. Dörfer who was known for his topography of Schleswig and Holstein. Later he attended the grammar school in Altona, whose headmaster J. Struve became the progenitor of a well-known family of astronomers. From 1799 he studied jurisprudence in Kiel and Göttingen and in 1804 he went to Livonia as a private tutor and soon became a lecturer of law in Dorpat. Simultaneously he studied mathematics and astronomy together with Johann Wilhelm Andreas Pfaff. His doctoral thesis in 1806 dealt with Roman Law.

Some remarks concerning the history of Holstein will not be amiss. Traditionally Holstein as a county, later duchy had its cultural and political position between Denmark and the German lands. Belonging to the German Reich on the one hand, it was under Danish fief on the other. At the Vienna Congress it came under the rule of Denmark and the Danish King became the Duke of Holstein. At the same time Holstein belonged to the loose German Alliance as for instance Hanover, too, politically belonging to the English Crown.

[4] Olufsen, Christian Friis Rottböll: Heinrich Christian Schumacher. *Oversigt over det Kgl. danske Videnskabernes Selskabs Forhandlinger og dets Medlemmers Arbeider i Aaret 1851.* København, 226–235, German transl.: *Astronomische Nachrichten* **36** (1853), 393–404; Petersen, Adolph Cornelius: Schumacher's Tod. *Astronomische Nachrichten* **31** (1851), 369–370; Repsold, Joh. A.: H.C. Schumacher. *Astronomische Nachrichten* **208** (1918), 17–34

[5] Briefwechsel zwischen C.F. Gauss und H.C. Schumacher. Hrsg. von C.A.F. Peters, vol. 3. Altona 1860, 345

Figure 1: Heinrich Christian Schumacher (Nationalhistoriske Museum, Frederiksborg)

This status quo was practised peacefully in Holstein. The Danish King respected the German language and culture and the population accepted the Danish rule. By birth Schumacher was a Danish subject, he felt himself as such and turned this circumstance to his own advantage and to the advantage of astronomy.

A promised employment at the King's chamber of finance came to nothing due to war accidents in Copenhagen, but in 1810 Schumacher was offered an extraordinary professorship of astronomy at the University of Copenhagen. At the same time he made the acquaintance of important astronomers and constructors of instruments such as Johann Georg Repsold in Hamburg and exchanged letters with Friedrich Wilhelm Bessel in Königsberg, Johann Elert Bode in Berlin and Wilhelm Olbers in Bremen. Having been granted a King's scholarship,[6] see figure 2), he worked in 1808/09 for a year with Gauß in Göttingen. In 1812 he got a leave of absence and became the director of the observatory in Mannheim because of disagreement with the professor of astronomy Thomas Bugge. This appointment must have been offered on the initiative of Bernhard August von Lindenau, who not only directly patronised him, but also personally supported this decision: "Surely you are absolutely right to prefer the friendly Mannheim with its excellent instruments to the harsh Copenhagen with, as I think, its bad instruments".[7] However, to his Danish compatriot Hans Christian Ørsted, secretary of the Royal Danish Academy of Sciences and Letters since 1815, Schumacher complained that his position in Mannheim was a bit difficult because the observatory was rather modestly equipped with instruments and "in an incomprehensible state of neglect and disorder".[8] But he did not get the time to modernise it because already in 1815, after the death of Bugge, he returned as ordinary professor of astronomy and director of the observatory in Copenhagen. On December 8 of that year the Royal Danish Academy of Sciences and Letters admitted him as a member.[9]

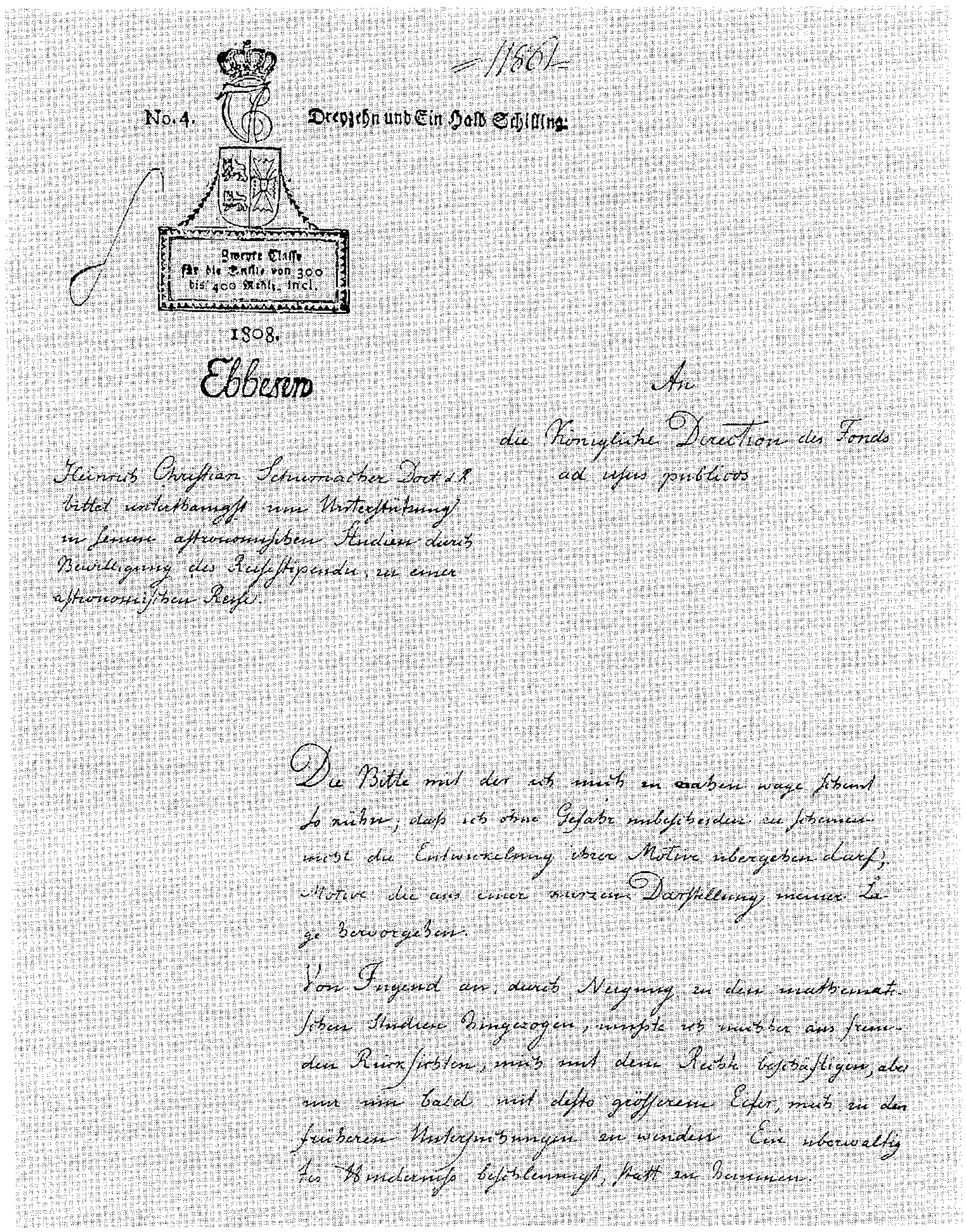
It was his desire to return to Denmark even with financial loss, because obviously Schumacher did not feel well in Mannheim, as he had told Ørsted before. "My dearest friend, maybe you could remind the King of me?", he

[6]Rigsarkivet, København. Fonden ad usus publicos 1810–1812, 1808 Apr. 26; Fonden ad usus publicos. Aktenmaessige bidrag til belysning af dens virksomhed. Udgivet af Rigsarkivet, vol. 2. København 1902, 123–124

[7]Schumacher-Nachlaß, Staatsbibl. Berlin, Preuß. Kulturbesitz, Lindenau 1, 15.2.1813 ("Gewiss haben Sie in jeder Hinsicht Recht, das freundliche Mannheim, mit seinen vortrefflichen Instrumenten, dem rauhen Copenhagen mit den dortigen wie ich glaube schlechten Instrumenten vorzuziehen.")

[8]Ørsted, Hans Christian: *Correspondance avec divers savants.* Publ. M.C. Harding, vol. 2. København 1920, 512 ("Mir wurden die Instrumente in einem unbegreiflichen Zustande der Verwahrlosung und Unordnung übergeben.")

[9]Det Kongelige Danske Videnskabernes Selskab 1742–1942, Samlinger til Selskabets Historie (ed. A. Lomholt), vol. 1. København 1942, 366



1186

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1808.

Ebbesen

An
die Königliche Direction des Fonds
ad usus publicos

Heinrich Christian Schumacher Doct. J.R.
bittet unterthänigst um Unterstützung
in seinen astronomischen Studien durch
Bewilligung des Reisestipendii, zu einer
astronomischen Reise.

Die Bitte mit der ich mich an [illegible] wage scheint
so kühn, daß ich ohne Gefahr unbescheiden zu scheinen
nicht die Entwickelung ihrer Motive übergehen darf;
Motive die aus einer kurzen Darstellung meiner La-
ge hervorgehen.
Von Jugend an, durch Neigung zu den mathemati-
schen Studien hingezogen, mußte ich nachher aus frem-
den Rücksichten, mich mit dem Recht beschäftigen, aber
nur um bald mit desto grösserem Eifer, mich zu den
früheren Untersuchungen zu wenden. Ein überwältig-
tes Hinderniß beschleunigt, statt zu hemmen.

Figure 2: Schumacher's application for a scholarship to visit foreign observatories, Altona 26.4.1808, 1 (Rigsarkivet, København. Fonden ad usus publicos 1810–1812, for the full text see the appendix)

asked Ørsted pleadingly. In fact immediately after Bugge's death on January 15, 1815 everything was prepared for the return of Schumacher. Already on March 2 he could inform Ørsted that he had received "a kind of recall".[10]

But in the "Round Tower" in Copernhagen he found antiquated instruments.[11] In order to take up important scientific work he conceived the project of a new Danish land surveying from Skagen to Lauenburg (at that time belonging to Denmark). It was to replace the hardly completed surveying done under the auspices of the Academy to which Bugge and Wessel had contributed. To get the King's support for the plan he argued in April of 1816 in the following clever way:

> If you would allow me to add a few words concerning my own person then I would like to say that here [in Copenhagen] I sit around in such complete idleness as an astronomer can ever do. The observatory is simply no good for any scientific use and I cannot make any observations there of the slightest value ... However, all my life and work is devoted to astronomy and my strongest desire urges me to do this work. Having left an excellent observatory in Mannheim, may I at least hope to find support for scientific work in my homeland, especially when it will reflect the major part of Denmark's glory? Even more, should the financial situation after some years make it possible to built an observatory that might serve science, this work would fill the time in between.[12]

One might consider his evaluation of the Mannheim observatory to be a tactical exaggeration. However, the plans of land surveying were approved, and before long an observatory was build in Altona (not in Copenhagen). For the project of a new observatory, started in 1818, Schumacher got first-class instruments by Repsold in Hamburg and practical support from the Danish officers Alexander Caroc (1784–1827), Johann David Nehus (1791–1844), Christian Wilhelm Nyegaard (1796–1846) and Christian Christopher

[10] Ørsted, H.C., *Correspondance*, 513, 516 ("Könnten Sie vielleicht nicht werthester Freund den König an mich erinnern und ihm den Wunsch äussern Ihren Freund wieder zu sehen?"); for Bugge's biography see: *Dansk Biografisk Leksikon*, vol. 3. København 1979, 59–61, his publications: Fortegnelse over det Kongelige Danske Videnskabernes Selskab publikationer 1742–1930. København 1930, 38–40

[11] Schumacher, H.C.: Sternbedeckungen in Copenhagen. *Astronomische Nachrichten* **1** (1823), 195–196

[12] Nielsen, Axel V.: H.C. Schumacher and the observatory at Altona during the war of 1848–50. Meddelelser fra Ole Rømer-Obs. Århus 22 (1951), Nr. 22, 267–268

Zahrtmann (1793–1853), later the minister of the Royal Danish Navy. Schumacher spent the winter of 1820 working in Copenhagen, where the King had allowed him to construct a small observatory at "Holmens Bastion" where he could measure latitude and longitude. According to information from 1837 the King granted 30 000 Reichsbankthaler per year for the land surveying in Holstein.[13] From Copenhagen it was rather troublesome to order instruments and to collaborate with Gauß. Therefore Schumacher rented a few rooms in Altona to which he moved in June of 1821, having been temporarily relieved from his duties and ordered to report regularly to the King. This was a very favourite situation for Schumacher. He was still officially professor of astronomy in Copenhagen and therefore financially safe, and at the same time he could deal with his extensive scientific work undisturbed by any hustle and bustle at the university.

In the garden of his roomy house that he bought in 1821 with the King's support at the "Palmaille" in a noble living quarter from 1800 Schumacher built a little practical observatory. Often he welcomed prominent astronomer colleagues there, among them Bessel and Gauß. Later he bought an additional neighbouring house, again with financial help from Copenhagen.[14] From his studios he could overlook the river Elbe.

The land surveying, the elaboration of a map of Holstein, the revision of the system of Danish weights and measures and its comparison to the Prussian and French ones occupied his time for many years. But this is not the subject of the present paper. Still let me make two comments: To give reasons for the Danish measurements of latitude and longitude Schumacher referred to the resulting possibility of fixing the shape of the earth. The former French and English measurements, he argued on September 14, 1816, had led to contradictory results. Therefore Denmark with its own activities in this field could bring the decision.[15] In fact, in 1840 Bessel used the Danish measurements of latitude and longitude for his very exact calculation of the flatting of the earth.

In his own works Schumacher, of course, knew about the results of his predecessors, among them Caspar Wessel. In a letter from Ørsted dated

[13] Encke-Nachlaß, Archiv der Berlin-Brandenb. Akademie der Wiss. Berlin, C.H.F. Peters, 18.7.1837

[14] Schumacher-Nachlaß, Mösting 1, June 1821, Jan./Feb. 1826

[15] Rigsarkivet. Fonden, 14.5.–14.9.1816; Fonden ad usus publicos. Aktenmaessige bidrag, 231–237

February 3 1827 one reads that the general staff had ordered him to send "the journals concerning the triangulation of Seeland to the general staff of the quarter master [Generalquartirmeisterstab] ... The quarter master says in his letter that he believes there exist 5 journals by Bugge and 5 by Wessel. He promises to return them very soon." The General combining several special maps "had found some important inaccuracies" that so far have not been clarified. In his letter dated February 20 Ørsted confirmed the receipt from Schumacher.[16]

Moreover it should mentioned that Schumacher together with Ørsted developed the plan to establish a workshop for precision instruments at the Polytechnical Institute of Copenhagen to reduce the dependence of Denmark in this field. "Under these circumstances, I am convinced you will have a workshop soon that can compete with any workshops abroad and replace them", Schumacher wrote on November 23, 1830.[17]

3 Astronomische Nachrichten

Let us return to the times of the failure of the astronomical journals founded around 1800. Since about 1810 Schumacher had been acquainted with the most important astronomers of his time. Despite the fact that in science Schumacher had not achieved much, he was highly regarded, and it was generally believed that he would become an efficient astronomer in the future. "In this Doctor Schumacher, who stayed with Gauß this summer I have met a very talented and clever man promising a lot for astronomy", Olbers wrote in November 1809 to Bode in Berlin.[18] Like many other astronomers Schumacher worried about the question how to guarantee the existence of a special astronomical journal. In September 1819 he asked Olbers in Bremen: "Would it be possible for you to take over the journal of astronomy now

[16] Schumacher-Nachlaß, Ørsted 1, 3.2., 20.2.1827 [the Schumacher estate in the Staatsbibl. Berlin contains 64 letters from Ørsted to Schumacher 1817–1850, not mentioned in Ørsted's printed Correspondance]

[17] Ørsted, H.C., Correspondance, 518–519; Schumacher-Nachlaß, Ørsted 2–3, 4.12. 1830–13.9.1831; Ørsted was the director of the Institute since 1829

[18] Olbers, Wilhelm: Aus einem Schreiben ... v. 21. Nov. 1809. *Astronomisches Jahrbuch für das Jahr 1813.* Berlin 1810, 256 ("An dem Doct. Schumacher, der sich diesen Sommer bey Gauß aufgehalten hat, habe ich einen sehr talentvollen und geschickten Mann kennen gelernt, von dem sich die Sternkunde noch viel zu versprechen hat".)

that Lindenau has resigned? It must be a man with excellent reputation, otherwise everything will fail soon".[19] The reply is not preserved but Olbers refused.

Schumacher was right to demand certain qualifications from the future publisher of an astronomical journal. On the one hand he had to be a well-known astronomer trusted by his colleagues, on the other hand he had to be ready to sacrifice much of his time for organisational and editorial work in the interest of the journal. And he had to be able to control this complicated project fully. In addition the publisher had to be able to evaluate critically the manuscripts and to collaborate sensitively and consistently with the authors — he should have, according to Lindenau "along with thoroughness and versatility of mathematical-astronomical education at the same time to a high degree the trust of the entire astronomical world".[20]

Schumacher did not suspect that before long he would have to play this important role. The first hint that Schumacher might be a suitable candidate can be found in a letter from Sept. 1819 from Lindenau to Schumacher where referring to Zach's "Correspondance Astronomique" he writes: "Do you really feel like taking over the publishing of such a journal in the German language?".[21] But by the beginning of 1820 Lindenau thought that Olbers was a desirable candidate, too.[22] A bit later they seem to have concentrated on Schumacher. Lindenau argued as follows: "Since Schumacher combines a great literary erudition with diligence I might think that he is well fitted for such a job."[23] After the summer of 1820 many negotiations must have been conducted behind the scenes among the astronomers as well as between Schumacher and the officials of the Danish King's government. As a clear result in March 1821 Schumacher was officially requested to found an

[19]Olbers-Nachlaß, Staats- und Universitätsbibl. Bremen, Briefwechsel Schumacher-Olbers, Ol 1 ff., 3.9.1819 ("Es muß ein Mann von erstem Rufe sein, sonst fällt alles bald zusammen.")

[20]Bessel-Nachlaß, Archiv der Berlin-Brandenb. Akademie der Wiss. Berlin, Nr. 287, Lindenau, 1.3.1820 ("der neben der Gründlichkeit und Vielseitigkeit mathematisch-astronomischer Bildung, zugleich auch das Vertrauen der ganzen astronomischen Welt in hohem Grade besitzt")

[21]Schumacher-Nachlaß, Lindenau.

[22]Bessel-Nachlaß, Nr. 287, Lindenau, 1.3.1820.

[23]Lindenau an Gauß, 7.7.1821. Niedersächs. Staats- u. Univ.-Bibl. Göttingen, Cod. Ms. Gauß 101, Briefe A (Lindenau V), Brief Nr. 208 ("Da Schumacher mit einer grossen litterarischen Bekanntschaft einen grossen Fleiss verbindet, so möchte ich glauben, dass er zu einen solchen Geschäft gut geeigenschaftet sey".)

astronomical journal (see figure 3). This request was made by Johann Sigismund von Mösting. Mösting was a highly educated person. Having studied jurisprudence in Copenhagen, he was the director of the Danish Reichsbank in 1813. Soon thereafter he became minister of finance, president of the Chamber of Finance and for many years the Prime Minister of the Danish Kings, chancellor of the Dannebrog-Order, in 1838 director of the King's Library and, last but not least, he was a friend and promoter of astronomy as well.[24]

About Mösting's intention to found an astronomical journal we know first from Schumacher's information to Gauß on March 27, 1821. Unfortunately, the 1211 preserved letters from Mösting to Schumacher from 1821 till 1845 started on June 12, 1821.[25] The mentioned "request" by Mösting, probably in the form of a King's order, indicated to Schumacher that he should "publish an astronomical newspaper in Altona with approximately one printed sheet per week serving the most lively communication among astronomers".[26] In the preface to the first volume of *Astronomische Nachrichten* dated Altona September 1821 Schumacher wrote: "By higher support I am able to offer this journal to astronomers and mathematicians as a means of quick circulation of individual observations and short news".[27] Schumacher had sent his colleagues a similar text already in June, 1821, inviting them to collaborate.

At that time Schumacher was still formally a professor of astronomy in Copenhagen and he received his salary at least till 1845.[28] But in connection with the foundation of the *Astronomische Nachrichten* he was allowed to have Altona as his permanent residence and further to suspend his duties at the university. This privilege must have been a basic precondition for him. For the foundation and later the maintenance of the journal money was paid from the King's budget: "By publishing astronomical news and treatises our Minister of Finance wants to promote the rapid publication of scientific works and therefore really covers all expenses", Schumacher wrote to Encke

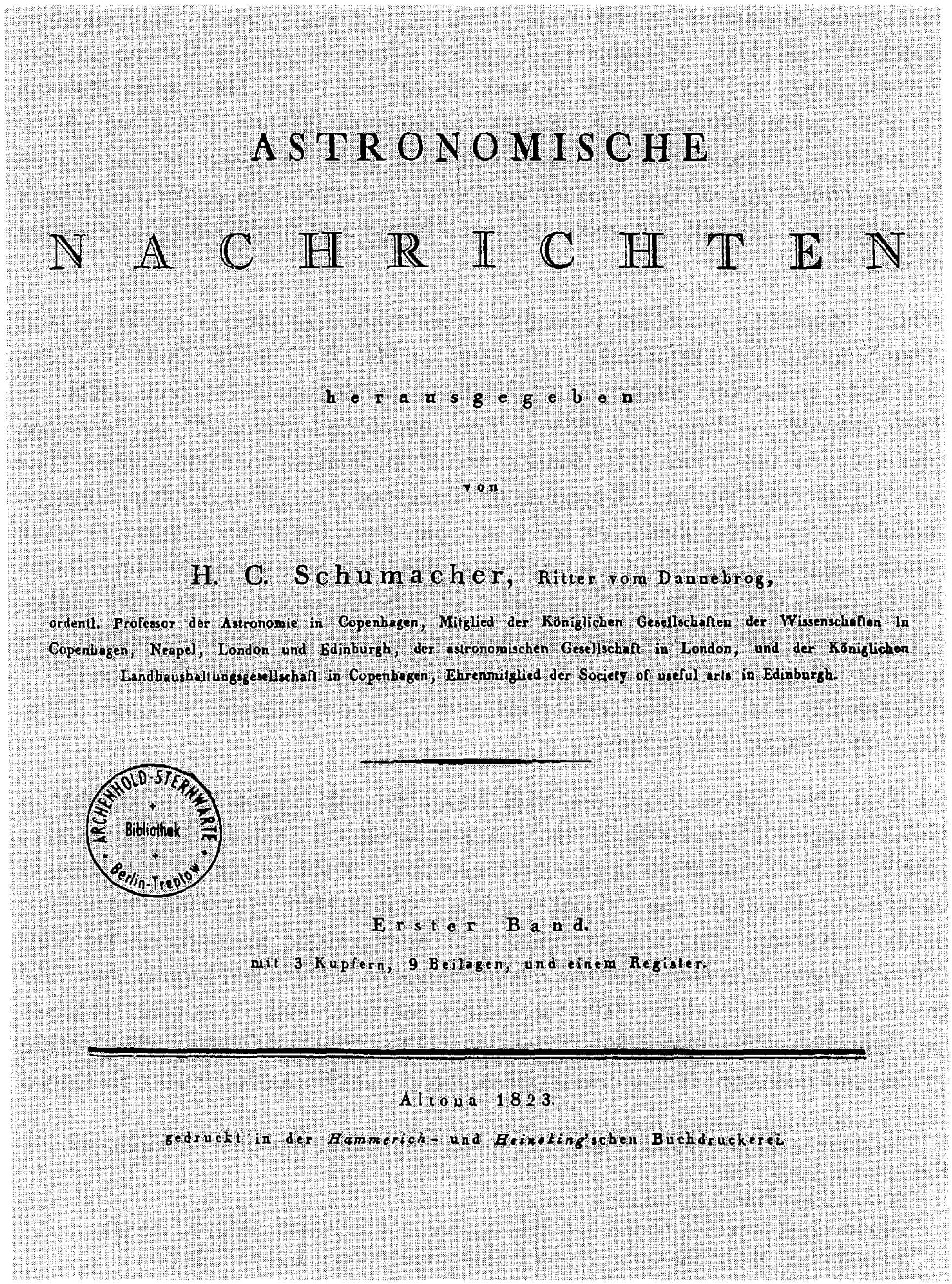
[24] Jørgensen, Harald: Johan Sigismund v. Mösting. *Dansk biografisk Leksikon*, vol. 10. København 1982, 300–302

[25] Schumacher-Nachlaß, Mösting

[26] Briefwechsel zwischen Gauss und Schumacher, vol. 1. Altona 1860, 27.3.1821

[27] Schumacher, H.C.: Vorwort. *Astronomische Nachrichten* 1 (1823), 1–2

[28] Københavns Universitet 1479–1979, vol. XII.1. Det matematisk-naturvidenskabelige Fakultet. København 1983, 63



ASTRONOMISCHE

NACHRICHTEN

herausgegeben

von

H. C. Schumacher, Ritter vom Dannebrog,

ordentl. Professor der Astronomie in Copenhagen, Mitglied der Königlichen Gesellschaften der Wissenschaften in Copenhagen, Neapel, London und Edinburgh, der astronomischen Gesellschaft in London, und der Königlichen Landhaushaltungsgesellschaft in Copenhagen, Ehrenmitglied der Society of useful arts in Edinburgh.

Erster Band.

mit 3 Kupfern, 9 Beilagen, und einem Register.

Altona 1823.

gedruckt in der *Hammerich*- und *Heineking*'schen Buchdruckerei.

Figure 3: *Astronomische Nachrichten*, title page of volume 1

on January 26 1822.[29] For the year 1849 there is the information that Schumacher received an annual financial aid of 640 Reichsbankthaler in order to cover the expense for the publication of the *Astronomische Nachrichten.*[30]

During the first decades the publication of the *Astronomische Nachrichten* was possible only because of the generous support by Frederik VI and after his death in 1839 by Christian VIII. It can be considered as an extraordinary example of international promotion of science. Looking back to history it is difficult to find another time and place where it would have been possible to publish permanently an astronomical journal. To underline this I would like to refer to the circumstances under which the *Astronomisches Jahrbuch* had been published in the Prussian capital. In 1774 the Berlin Academy founded an annual with a literary part because it was considered to be useful for the communication between astronomers. Very soon, however, the Academy no longer wanted to bear the financial risk and retreated from the project. Therefore Bode had to run it as a completely private enterprise. It was extremely difficult to find a publisher for the supplement editions containing scientific articles because the financial profit was doubtful.[31]

Johann Sigismund von Mösting (1759–1843) was the mediator in the project between the Danish King and Schumacher. He was bound to Schumacher by ties of deep friendship and gratitude. This is attested by the great number of letters exchanged between them where they addressed each other with the rarely used "Dear friend". They often wrote about very private matters, but also about political affairs in Altona, revealing interesting insights into Schumacher's living conditions. On July 27, 1827, for instance, Schumacher reported about an incident in Altona with a police officer von Aspern: "For the calm inhabitants of Altona this turmoil had the unpleasant effect that the police have almost disappeared. Beggars rush into the houses and I guess that thieves will not refrain from taking advantage of the situation. I barricade myself every night, as well as I can with my chronometers and instruments in my house and I will try to defend the fortress."[32]

[29]Encke-Nachlaß, Archiv der Berlin-Brandenb. Akademie der Wiss. Berlin, Schumacher, 26.1.1822

[30]Schumacher, H.C., an J. Collin, 3.3.1849, quotation Nielsen, Axel V., H.C. Schumacher, 296

[31]Herrmann, Dieter B.: Die Entstehung, 23–24

[32]Rigsarkivet. Privatarkiver J.S. Mösting, No. 6023, 27.7.1827 ("Für die ruhigen Einwohner Altonas hat dieser Tumult die unangenehmen Folgen, daß die Polizei fast ganz darniederliegt. Die Bettler drängen sich jetzt in die Häuser, und ich vermuthe die Diebe

Mösting did not play the role of the willing financier but encouraged Schumacher to undertake the important and difficult publication of the journal. Nevertheless, it seems to be a bit exaggerated when Schumacher on April 10, 1821 mentioned to Gauß that "everybody will believe that it is my idea, but in fact it comes from our minister of finance and is not imposed on him".[33] In the first volume of the *Astronomische Nachrichten* Schumacher wrote as follows: "With the sincerest gratitude for the great measure of support for my scientific plans I feel the rare happiness to live under a King like Frederik, loving and protecting in a King's manner the exact sciences, and under a minister like Mösting, full of the same love, putting the great plans of his King into practice."[34] It was mainly Mösting who presented Schumacher's petitions and reports to the King and hurried to send the confirmation to Altona. Schumacher also had good relations with other officials of the court such as Johan Gunder Adler (1784–1852), Royal Secretary of the cabinet and secret state councillor.[35]

But the relation to the court was kept mainly through Mösting. Schumacher knew his ideas to be in good hands there. Most probably it is thanks to Mösting that a number of the most important astronomers working at German observatories became Knights or even Commanders of the Dannebrog-Order, as for instance Bessel, Olbers, Hansen and Gauß. It was Schumacher's unofficial duty to explain to his colleagues how to wear the "decorations" and to write the letter of thanks to Mösting as the Chancellor of the Order and the King.[36] Christian as well as Frederik allowed Schumacher to suggest who among his colleagues deserved to be decorated.[37] One may have different opinions as far as decorations and medals are concerned, but the attitude of the Danish Court reflects the high esteem for scientific research hardly to be found at any time in any other country.

In this connection the medal endowed by Frederik VI for the discovery of

werden auch nicht unterlassen, auf die Conjunctur zu speculirn. Ich verschanze mich jede Nacht, nach besten Kräften mit meinen Chronometern und Instrumenten in meinem Hause und werde die Festung zu vertheidigen suchen.")

[33] Briefwechsel zwischen Gauss und Schumacher, vol. 1, 10.4.1821

[34] Schumacher, H.C.: Zusatz des Herausgebers. *Astronomische Nachrichten* **1** (1823), 376

[35] Schumacher-Nachlaß, J.G. Adler; Rigsarkivet. Privatarkiver J.G. Adler, Nr. 5008

[36] Briefwechsel zwischen Gauss und Schumacher, vol. 3, 382–383, 390–391; Olbers-Nachlaß, Ol 17, 26.3.1825; the Dannebrog-Ridders-Ordens Statuter in: *De Kongelige Danske Ridderordener og Medailler*, vol. 1. København 1950, 94–102

[37] Privatarkiver J.G. Adler, 4.6.1840

comets by telescope should be mentioned,[38] (see figure 4). The "Gold medal of 20 ducats" confirmed by Christian VIII in April of 1840 had a special dedication: "The medal will be dedicated to the person who first discovers a comet invisible by the naked eye but visible by telescope at the time of discovery and with unknown time of orbit".[39] As had been the case with the *Astronomische Nachrichten*, an international effect was intended. On the one hand the origin of the discoverer was of no importance, on the other hand Frederik had formed an international jury consisting of Schumacher and Francis Baily in London. For the case that the two could not agree a referee was appointed. After the death of Olbers in 1840 Carl Friedrich Gauß became the referee, chosen by Christian among Schumacher's proposals: Bessel, Gauß and Herschel.[40] The medal was designed in 1834 by the Danish artist Christen Christensen[41] under supervision of the important classical sculptor Bertel (Alberto) Thorvaldsen.

The endeavours to intensively promote science in Copenhagen, of which the examples mentioned are only a few among many, were honoured with gratitude by astronomers. In 1823 Johann Franz Encke, successor of Lindenau at the Seeberg-Observatory near Gotha and later outstanding director of the Berlin Academy Observatory, exclaimed: "Where else than in Denmark can one nowadays find such a sense among the rulers to heavily support really useful work?"[42] The astronomers expressed their gratitude to the King's minister of finance in a special way: In their classical works about the topography of the moon in 1837 Wilhelm Beer and Johann Heinrich Mædler named a lunar crater after him.[43] It is to be seen as "Mösting" A and B; a second object was named "Schumacher". Moreover, Christian VIII in 1840 was honoured as the first foreign ruler to be elected member of the Royal

[38] Schumacher, H.C.: Abbildung der Cometen-Medaille. *Astronomische Nachrichten* **11** (1834), 137f.

[39] Schumacher, H.C.: Allerhöchste Bestätigung der Cometen Medaille. *Astronomische Nachrichten* **17** (1840), 241

[40] Briefwechsel zwischen Gauss und Schumacher, vol. 3, 365

[41] Dansk Kunstnerleksikon, vol. 1. København 1994, 502–503; *Allgemeines Lexikon der bildenden Künstler* **6** (1912), 537

[42] Encke, Johann Franz: Auszug aus einem Briefe ... an den Herausgeber. *Astronomische Nachrichten* **1** (1823), 375

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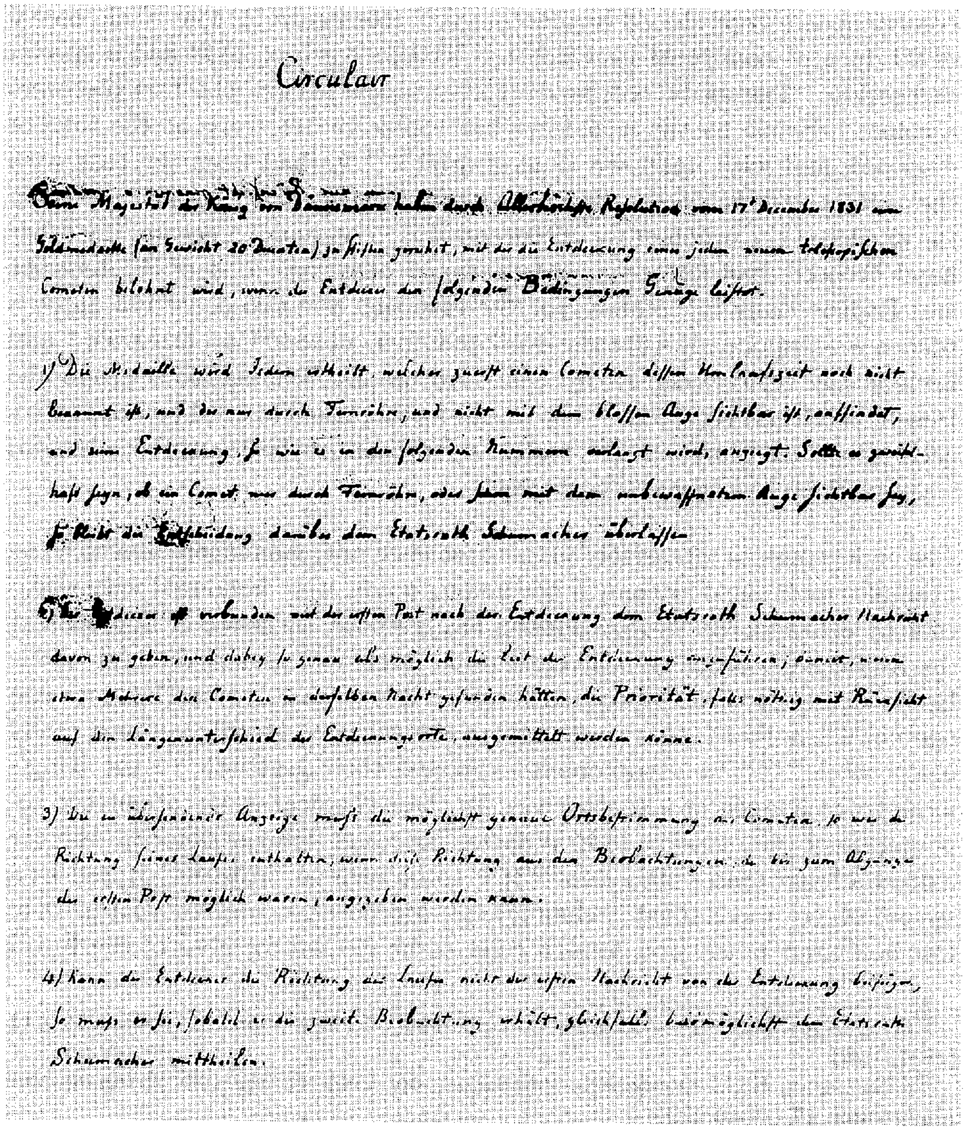

Circulair

Seine Majestät der König von Dänemark haben durch Allerhöchste Resolution vom 17ᵗ December 1831 eine Goldmedaille (am Gewicht 20 Ducaten) zu stiften geruhet, mit der die Entdeckung eines jeden neuen telescopischen Cometen belohnt wird, wenn der Entdecker den folgenden Bedingungen Genüge leistet.

1) Die Medaille wird Jedem ertheilt, welcher zuerst einen Cometen dessen Umlaufszeit noch nicht bekannt ist, und der nur durch Fernröhre, und nicht mit dem blossen Auge sichtbar ist, auffindet, und seine Entdeckung, so wie es in den folgenden Nummern verlangt wird, anzeigt. Sollte es zweifelhaft seyn, ob ein Comet nur durch Fernröhre, oder schon mit dem unbewaffneten Auge sichtbar sey, so bleibt die Entscheidung darüber dem Etatsrath Schumacher überlassen.

2) Der [illegible] ist verbunden mit der ersten Post nach der Entdeckung dem Etatsrath Schumacher Nachricht davon zu geben, und dabey so genau als möglich die Zeit der Entdeckung anzuführen, damit, wenn etwa Mehrere den Cometen in derselben Nacht gefunden hätten, die Priorität, falls nöthig mit Rücksicht auf den Längenunterschied der Entdeckungsorte, ausgemittelt werden könne.

3) Die zu übersendende Anzeige muss die möglichst genaue Ortsbestimmung des Cometen so wie die Richtung seines Laufes enthalten, wenn diese Richtung aus den Beobachtungen, die bis zum Abgange der ersten Post möglich waren, angegeben werden kann.

4) Kann der Entdecker die Richtung des Laufes nicht der ersten Nachricht von der Entdeckung beifügen, so muss er sie, sobald er die zweite Beobachtung erhält, gleichfalls baldmöglichst dem Etatsrath Schumacher mittheilen.

Figure 4a

Figure 4a,b: "Circulair" related the foundation of the medal for the discovery of comets by telescopes by Frederik VI., 17.12.1831, with a postscript by Schumacher to Encke (Encke-Nachlaß, Archiv der Berlin-Brandenb. Akademie der Wiss. Berlin, Schumacher, 6.1.1832)

5) Ob eine Entdeckung als constatirt zu betrachten sey, oder nicht, bleibt der Entscheidung des Etatsraths Schumacher überlassen.

6) Die Zuerkennung der Medaille geschieht ein halbes Jahr nach der Entdeckung.

7) Auf Astronomen in andern Welttheilen kann nur Rücksicht genommen werden, wenn ihre Anmeldung vor Ablauf des Termins dem Etatsrath Schumacher zu Händen kommt. Nach Ablauf dieses in No 6 bestimmten Termins wird überhaupt keine Reclamation mehr angenommen.

Indem ich die Ehre habe Ihnen diese, von Sr. Majestät Allerhöchst bestimmten, Bedingungen mitzutheilen, empfehle ich mich Ihrem geneigten Andenken.

Altona 1832. Januar 6. H. C. Schumacher.

[illegible]

Altona 1832 Jan 10.

Schumacher

[illegible]

Figure 4b

Astronomical Society in London.[44]

Schumacher's *Astronomische Nachrichten* — the only international journal in the field of astronomy with its first 31 years edited by Schumacher himself — made the observatory in Altona the centre of international relations between astronomers. The success was immense as immense as the amount of work for the publisher! In a letter to George Biddel Airy from 1845 Schumacher speaks of about 1500 letters per year he had to draft and write himself.[45] This seems not to be an exaggeration. In the estate in the Berlin State Library alone nearly 10 000 letters to Schumacher from more than 750 authors can be found. Shortly before the *Astronomische Nachrichten* were started Bohnenberger mentioned to Schumacher: "I am extremely pleased that thanks to your astronomic news and articles astronomers will get into closer relations with each other again."[46]

These expectations were truly fulfilled. The *Astronomische Nachrichten* published with support of the Danish Kingdom, became a centre of international communication in the field of astronomy. All astronomers of any importance published in this journal whether they came from Germany, France, England, Russia, Italy, the Netherlands, the United States, Denmark, of course and so on. Thanks to his connections through correspondence Schumacher became an international "bureau of conference", a scientific "news agency". He received information and spread it via his *Astronomische Nachrichten* or by letters to his colleagues. In a letter to Jonas Collin (1776–1861), deputy in the Ministry of Finance, Schumacher wrote: "Altona now has become the centre of astronomic relations recognised even by England and France".[47] And the famous John Herschel, secretary of the Royal Astronomical Society in London, concluded in a letter to the Danish King of March 13, 1840 that Schumacher's journal, was "one of the most remarkable and influential astronomical works, which have ever appeared and which, while operating more beneficially on the progress of its Science than any similar work of modern times", has "made Your Majesty's city of Altona ... the astronomical centre of the civilised world".[48] Nothing more has to be said.

[44]Privatarkiver J.G. Adler, 25.2.–4.6.1840, incl. the letter from J. Herschel to Christian VIII, 13.3.1840, and its German transl. by Schumacher

[45]Nielsen, Axel V., H.C. Schumacher, 274

[46]Schumacher-Nachlaß, J.G.F. Bohnenberger, 2.10.1821

[47]Nielsen, Axel V., H.C. Schumacher, 291

[48]Privatarkiver J.G. Adler, 27.5.1840

The *Astronomische Nachrichten* survived many difficult situations: First around 1848/50 the differences between Denmark and Germany concerning the status of Holstein;[49] later the first and second World Wars, and finally the transfer of the centres of astronomical research from Middle Europe. In 1998 the journal appeared in its 319th volume. The editorial bureau is at the Astrophysical Institute in Potsdam.[50]

Appendix

Schumacher's application for a scholarship to visit foreign observatories, Altona 1808 April 26, Rigsarkivet København. Fonden ad usus publicos 1810–1812 (fig. 2).

An die Königliche Direction des Fonds ad usus publicos

Heinrich Christian Schumacher Doctor der Rechte bittet unterthänigst um Unterstützung in seinen astronomischen Studien durch Bewilligung des Reisestipendii zu einer astronomischen Reise.

Die Bitte mit der ich mich zu nahen wage scheint so kühn, daß ich ohne Gefahr unbescheiden zu scheinen nicht die Entwickelung ihrer Motive übergehen darf, Motive die aus einer kurzen Darstellung meiner Lage hervorgehen.

Von Jugend an, durch Neigung zu den mathematischen Studien hingezogen, mußte ich nachher aus fremden Rücksichten, mich mit dem Rechte beschäftigen, aber nur um bald mit desto grösserem Eifer, mich zu den früheren Untersuchungen zu wenden. Ein überwältigtes Hinderniß beschleunigt, statt zu hemmen.

Kaum war es mir vergönnt aus der reinen Theorie in die Anwendungen zu treten, als vor allen die Astronomie mich anzog. Wenn andere Wissenschaften theils auf dem ietzigen Zustand der Ausbildung, theils sogar auf menschlichen Schwächen, und moralischer Unvollkommenheit gegründet, ihr ephemeres Daseyn mit diesen Veränderlichen selbst wechseln, oder verlieren; so kann sie in der Natur gegründet, mit ihr von gleichem Umfange, gleicher Würde, nur mit dem Weltall veralten, und vergehen. Ihre leuchtende Bahn wird nur durch das Unendliche begränzt. Denn wie die Naturwissenschaften in absteigender Linie, die Organisation bis zum Atom verfolgen, so geht sie in aufsteigender von der Erde zur Sonne, von der Sonne zu ihrer Sterngruppe, von dieser zum Sternenheer.

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[49] Schwarz, Oliver; Strumpf, Manfred: Peter Andreas Hansen und die astronomische Gemeinschaft. *Beiträge zur Astronomiegeschichte* 1 (1998), 141–154 (Acta Historica Astronomiae; 1)

[50] Present attempts to work scientifically with the Berlin estate of Schumacher so far have failed because of the lack of funding. Allow me to propose the idea to put this into practice as a German-Danish joint project, in Schumacher's spirit.

In der Ruhe des Landes, nach vollendeten academischen Jahren, beschäftigte ich mich ausschließlich mit ihrem theoretischen Theile, zur Praxis führte nachher mich mein Freund der Professor Pfaff in Dorpat. Mit ihm beobachtete ich ein Jahr hindurch, das nur zu schnell verging.

Dann, ohne günstige Aussichten in Frankreich zu benutzen, fest überzeugt mein Glück sey an mein Vaterland gebunden, kehrte ich zurück voll Hoffnung nicht die verlassenen juristischen Studien wieder ergreifen zu dürfen. Dennoch scheint mich iezt die Nothwendigkeit dahin zurückzuführen, da Mangel des Vermögens mich verhindert eine künftige Anstellung in meinem Fache abzuwarten. Die einzige Hoffnung mit der ich iezt meinen Muth belebe, beruht auf eine gnädige Gewährung meiner Bitte um Unterstützung, bis sich eine für mich passende Anstellung ergeben sollte.

Um nun die Zeit bis dahin so nützlich wie möglich zu verwenden, wünschte ich die vornehmsten Sternwarten zu besuchen um alles kennen zu lernen, was iezt zur Vervollkommnung des beobachtenden Apparats geschehen ist, und zugleich mich mit den ersten Astronomen in nähere Verhältnisse zu setzen, Verhältnisse die mir bey einer künftigen Anstellung von dem grössten Nutzen seyn würden.

Ich wage also meine Bitte mich durch gnädige Unterstützung in den Stand zu setzen, die ersten Sternwarten besuchen zu können

unterthänigst Altona den 26st. April 1808. Heinrich Christian Schumacher.

Viète's Generation of Triangles

Otto B. Bekken *

Caspar Wessel's main aim in his *On the Analytical Representation of Direction* is from given directed line segments to form others by algebraic operations and to form the product of two lines in the same plane as the positive unit:

- As regards length, the length of the product should be the product of the length of the factors,
- and "the directional angle of the product, or its deviation from the positive unit is the sum of the directional angles of the factors."

As Branner and Voje Johansen have recently discovered (see [Wessel (1999), 44–49]), Wessel used complex numbers to represent line segments in his surveying reports as early as 1787 — ten years before he wrote his theoretical memoir on the subject. Where he got his inspiration for this, we do not know. His only reference on these matters reads "nobody else has treated it. An exception might be Master Gilbert in Halle, whose prize memoir on Calculus Situs may contain some explanation on this subject." [Wessel (1999), 103] All attempts to find this prize memoir by Gilbert have so far failed.[1]

I will here only recall some material from two papers by Francois Viète (1540–1603) and his collaborator Alexander Anderson (1582–1619). T.R. Witmer's translation of 1983, which is our source, was based on Frans van Schooten's 1646 edition of Viète's manuscripts. In this context, these papers by Viète have been brought forward by Glushkov, by Bashmakova and Slavutin, by Bashmakova and by Itard (see the bibliography below).

In Viète's *Isagoge in Artem Analyticem* we find a section entitled The Genesis of Triangles with his proposition XLVI and the accompanying figures

*Kristiansand, Norway

[1] However, see Kirsti Andersen's argument on p.72 of [Wessel (1999)]

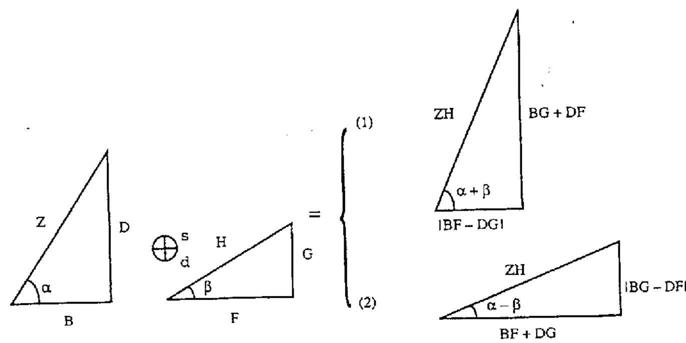

Figure 1: "To construct a third right triangle from two right triangles"

(see figure 1). His reasoning, which is verbal, can be symbolized through

$$\begin{aligned}(ZH)^2 &= Z^2H^2 = (B^2+D^2)(F^2+G^2)\\ &= B^2F^2 + D^2G^2 + B^2G^2 + D^2F^2\\ &= B^2F^2 \mp 2BFDG + D^2G^2 + B^2G^2 \pm 2BGDF + D^2F^2\\ &= (BF \mp DG)^2 + (BG \pm DF)^2\end{aligned}$$

Only the sides of the new triangle are presented in his third triangle generated from two right triangles by either of these methods. Moreover, a right triangle generated by the first method is called a *synaeresic* (taken together) triangle, and by the second method a *diaeresic* (taken apart) triangle, "for reasons set out in the proper place", says Viète.

This proper place seems to be his *Ad Angularium Sectionum Analyticen Theoremata* which opens with two theorems

Theorem I

If there are three right triangles the acute angle of the first of which **differs** from the acute angle of the second by the acute angle of the third, the first being the largest of these, the sides of the third will have the likenesses of the **diaeresic** triangle made from the first and the second.

Theorem II

If there are three right triangles and the acute angle of the first of these **plus** the acute angle of the second is equal to the acute angle of the third, the sides of the third will have the likenesses of the **synaeresic** triangle made from the first and the second.

The acute angle is the angle between the hypotenuse and the base, and the figure above has been amended by adding symbols to these angles. The demonstrations given by Anderson are based on Euclidean arguments, but can easily be interpreted as proofs of the trigonometric formulas for the sine and the cosine of the difference and the sum of two angles.
Viète's theorems say that if we want the acute angle of the product to be *the sum* of the acute angles of the factors, then we must use the *synaeresic* product of triangles with

$$\text{base} \quad BF - DG, \quad \text{and perpendicular} \quad BG + DF,$$

and if we want the acute angle of the product to be *the difference* of the acute angles of the factors, then we must use the *diaeresic* product of triangles with

$$\text{base} \quad BF + DG, \quad \text{and perpendicular} \quad BG - DF.$$

The reader skilled in complex number multiplication, can see the analogy to the real and imaginary parts of the product of the complex numbers $B + D\sqrt{-1}$ and $F + G\sqrt{-1}$ in the synaeresic case, and the product of $B + D\sqrt{-1}$ with the complex conjugate $F - G\sqrt{-1}$ in the diaeresic case. The similarity between Viète's product of triangles and Wessel's product of directed line segments is also quite apparent, but whether Wessel had access to or had seen these papers by Viète, this we do not know.

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Argand and the Early Work on Graphical Representation: New Sources and Interpretations

Gert Schubring *

The aim of the present paper is to investigate the works of some of the mathematicians who dealt with the graphical representation of the complex numbers independently of Wessel and in particular with the most famous of them: Argand. However let me begin by mentioning some of the basic concepts in Wessel's paper in order to establish connections with the ongoing discussion in the mathematical community at large and more specifically in France.

Wessel was not concerned with epistemological problems associated with the idea of negative quantities in the French discussion — rather, he extracted from it two basic notions and rearranged them: the notion of *être numérique* and the notion of *être spécifique* or *qualité* of a given quantity. Whereas these two notions had been treated as separate concepts in the French discussions of the 18th century, Wessel forged them together — renaming them as *line segments* and *directions of lines.*

His innovative procedure was to enlarge the notion of direction. He considered the concept of simply opposed directions like positive and negative directions as well-known and envisaged going beyond the restriction to directions along the same line, extending the concept to directions in the entire plane and even in the sphere. As Wessel explains:

> if one takes the operations in a wider sense, and does not, as before, restrict them to be used only on segments of the same or opposite direction, but extends their formerly restricted concept somewhat, so that it becomes applicable not only in the former cases, but also in infinitely many more cases (Wessel 1999, 95).

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An example of a generalization proposed by Wessel based on this approach is the *perpendicular unit.*

My intention is not to analyse the application of these generalizations but to discuss the point raised by Valentiner, one of the editors of the French version, in 1897. Valentiner expressed his astonishment about the innovations achieved by mathematical "outsiders" in these words:

> il est étonnant qu'un homme puisse composer un ouvrage aussi remarquable que celui qui nous occupe, après avoir dépassé la cinquantaine, sans avoir jamais, ni avant ni après, produit aucune oeuvre scientifique (Valentiner 1897, V).

Almost the same remark can be made about most of the inventors of the graphical representation of complex numbers before Gauß's publication of 1831. In fact, historians of mathematics frequently wonder about the simultaneity of these inventors around 1800 and about their common pattern of marginality to the mathematical communities, and of amateurism.

My intention is to contribute somewhat to an understanding of this mystery. Two authors who provide some elements for illuminating the issue are Argand and Buée.

1 Argand

Argand, the author of the booklet *Essai sur une manière de représenter les quantités imaginaires dans les constructions géométriques,* is usually identified as Jean Robert Argand, who lived from 1768 to 1822 and is reported to have been a bookkeeper in Paris (teneur de livres). As we will see, these few known data seem to be doubtful.

Argand's text, allegedly published in 1806, did not remain unknown like Wessel's, but was discussed beginning in 1813/14, with Argand himself participating in the discussions. These debates took place publicly in the *Annales de mathématiques pures et appliquées*, the first journal which specialised in mathematics, published by J. D. Gergonne at Montpellier. Yet, the public of this journal was a restricted one, as the dominant Parisian mathematicians did not participate in it.[1] Like Wessel, Argand begins with

[1]Lacroix, for example, did not communicate the fact that Buée's paper had been published in 1806 to Gergonne and his journal but rather to a colleague of Gergonne's. Lacroix's note became known to the *Annales* (vol. 4, 1814, 367) only by this intermediary.

reflections on oppositely directed quantities and uses the analogous conceptual differentiation of *rapport numérique* and *rapport de direction.* He goes on to ask if one can generalize these concepts to imaginary quantities, given that one cannot assign them a geometrical representation on one line as is the case for two opposed, so-called positive and negative directions. Thus, he looks for a geometrical construction in the same plane. He proposes such a construction in the plane by interpreting the proportion

$$1 : -1 = -1 : 1$$

as yielding the mean proportional. By this approach, Argand transformed the proportion $1 : -1 = -1 : 1$, which up until his time had been the touchstone for every approach to understand negative numbers, into the cornerstone of a new theory. Argand is therefore a nice illustration of Lakatos's model of conceptual development: former monsters become the definitional foundation for innovations. Having identified the notions of *grandeur absolue* and of *direction* as constituting his conceptual basis, he investigates whether it would be possible to combine them so as to assign a place to the imaginary quantities within the conceptual field of positive and negative quantities ("une place dans l'echelle des quantités positives ou négatives (Argand 1874, 6).) His answer was to exploit the basic proportion in a geometric way:

> En y réfléchissant, il a paru qu'on parviendrait à ce but si l'on pouvait trouver un genre de grandeurs auquel pût s'allier l'idée de direction, de manière que, étant adoptée deux directions opposées, l'une pour les valeurs positives, l'autre pour les valeurs négatives, il en existât une troisième telle, que la direction positive fût à celle dont il s'agit comme celle-ci est à la direction négative (ibid.)

In expanding on this approach, Argand did not restrict himself to perpendicular lines, to imaginary quantities. Hence, he instead constructed all directions in a given plane geometrically and thus achieved the construction of complex numbers as well. Argand gave as the general algebraic form for complex numbers

$$\pm a \pm b\sqrt{-1} \quad \text{(ibid., 12)}$$

and showed, using figure 1, the corresponding lines with their direction. Leading mathematicians seemed — during this period — not to be very

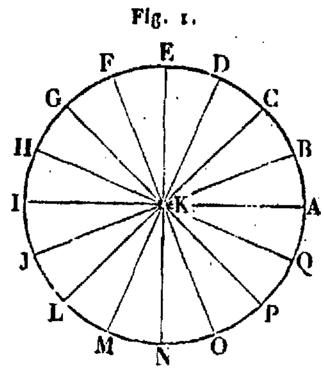

Figure 1: Argand 1874,7

interested in these foundational efforts. A revealing example is presented by Adrien Marie **Legendre** (1752–1833). As is well known, Argand's ideas attracted some public interest for the first time when Jacques Frédéric **Français** (1775–1833), an Alsatian mathematician, published an article in Gergonne's *Annales* in 1813 where he referred to the ideas of an unknown author:

> Je dois [...] à la justice de déclarer que le fond de ces idées nouvelles ne m'appartient pas. Je l'ai trouvé dans une lettre de M. Legendre à feu mon frère [François Joseph Français, 1768–1810], dans laquelle ce grand géomètre lui fait part (comme d'une chose qui lui a été communiquée, et comme objet de pure curiosité) du fond de mes définitions 2^{e} et 3^{e}, de mon théorème I^{er}, du corollaire 3^{e} de mon théorème II^{e} [...]. Je désire que la publicité que je donne aux résultats auxquels je suis parvenu puisse déterminer le premier auteur de ces idées à se faire connaitre, et à mettre au jour le travail qu'il a fait lui-même sur ce sujet (Français 1813a, 71).

As is likewise well known, this appeal induced Argand to enter into the debate and to show that he was the author.

I have been lucky enough to find this letter from Legendre to Français the elder brother. It is a letter dated 2 November 1806; in its final part, Legendre reports to Français that an unknown person had given a *mémoire* to him. Legendre was astonished by the quality of this paper and by the lack of ambition of its author.

> Il y a des gens qui cultivent les sciences avec assez de succès sans être connus et sans courir pour la renommée. Dernièrement j'ai vu un jeune homme qui m'a engagé à lire un travail qu'il avait fait sur les imaginaires; il ne m'expliquait pas très bien son objet, mais il me faisait entendre qu'il regardoit les quantités dites *imaginaires* comme aussi réelles que les autres, et qu'il les representait par des lignes. J'ai témoigné d'abord bien des doutes à l'auteur, cependant j'ai promis de lire son mémoire. J'y ai trouvé contre mon attente, des idées assez originales, fort bien présentées, appuyées de connaissances de calcul assez profondes, et enfin qui conduisent à des conséquences fort exactes telles que la plupart des formules de trigonométrie, le théorème de Cote, etc. Voici une esquisse de ce travail qui vous intéressera peut-être et qui vous fera juger du reste.[2]

Legendre then explained Argand's approach for the geometric construction of imaginary and complex quantities and went on to outline two of Argand's applications:
— trigonometric formulas such as

$$\cos(a+b) = \cos a \cos b - \sin a \sin b,$$

— Cotes's theorem (also called de Moivre's formula):

$$\cos(na) + \sqrt{-1}\sin(na) = (\cos a + \sqrt{-1}\sin a)^n.$$

Legendre declared himself not to be interested in this as a research subject but appealed to Français to further develop these ideas:

> Je ne rends ici qu'une petite partie de ses idées, mais vous y suppléerez et peut-être vous trouverez comme moi qu'elles sont assez singulières pour mériter attention. Au reste je vous les abandonne simplement comme objet de curiosité et je ne me chargerois pas de les défendre.

It seems that without this letter from Legendre to Français, Argand's paper would neither have been discussed by contemporary mathematicians nor would it ever have become publicly known. In fact, there is no proof that this paper was published in 1806. The only indication of this date is given by its appearance on the title page of the printed book. This is not decisive, for three reasons:

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[2] In my talk, I showed a copy of this letter, which belongs to a private collection.

— The booklet was not deposited at the *Dépot Legal* at Paris as prescribed by the law for published books, as my research at this office has shown.

— In all my searches I was unable to identify an original version in a French or a foreign library. All the copies in libraries are either the 1874 second version published by Hoüel or later reprints. And the 1874 edition is made on the basis of a copy which Argand had sent to Gergonne.

  As I have learned recently from Luigi Pepe (Ferrara), there is, however, at least a second copy of the first printing. It is in the possession of Jean-Luc Verley (Paris), who, as a bibliophile mathematician, possesses an impressive collection of old books. Curiously enough, an inspection of this copy proved that it belonged to Gergonne, too![3]

— Legendre's letter gives a supplementary argument: Legendre wouldn't have communicated Argand's ideas to Français and invited him to pursue and further develop them if the paper had already been published or if publication were imminent. Moreover, according to the rules of the Paris *Institut*, only unpublished manuscripts were allowed to be fully reported upon in this Academy. Thus if Argand wanted to have a chance of such a report, his paper had to remain a manuscript. In fact, Argand himself mentioned in 1813 that Legendre examined, in 1806, "mon manuscrit" (Argand 1813b, 133).

The only thing which one can say with certainty is that the paper was **printed** somewhere between 1806 and 1813, without an indication of the author; it was, however, only privately distributed[4] and not **published**. Even in 1813, it did not attain the character of a real publication, as the only known copies all belonged to Gergonne (see above). Although Argand had offered, in his 1813 paper in the *Annales*, to sell copies of his *Essai* (Argand 1813b, 133),[5] apparently no other contemporary mathematician

[3]I am very grateful to Jean-Luc Verley for his cooperation and assistance in this research. There is a manuscript note in his book which states that this copy had belonged to Gergonne and that it was used for Hoüels reprint. Since Verley's copy does not bear Argand's dedication to Gergonne of 1813 (written on the page with the printer's data, opposite to the title page), which is reproduced in the reprint, it is clear that this dedication was taken by Hoüel from a second copy originally belonging to Gergonne.

[4]As Argand affirmed himself in 1813, he had distributed the printed version only in a "très-petit nombre" (Argand 1813b, 133), probably to some friends. In this passage he characterized his paper as only being an "écrit" (ibid.).

[5]On the title page of the copy which he dedicated to Gergonne, he had crossed out the

contacted Argand or ordered the booklet. Even Français, most interested in the text, did not address Argand and rather preferred to borrow a copy from Gergonne (cf. Français 1814b, 222). It was only due to Hankel's revival of interest in Argand's original paper (Hankel 1867, 82) that Hoüel was motivated to search for it and to reprint it.

Besides the extraordinary history of Argand's *mémoire*, his biography offers at least as many questions. In fact, Legendre's letter has caused me to question all the biographical information available on Argand, which is actually quite scarce. I began to wonder about the fact that Legendre calls the paper's author "un jeune homme". Legendre was 54 years old in 1806 and the Argand of the standard sources already 38 years old. Can one believe that Legendre would, at age of 54 years, call a man of 38 years a "young man"? This is at least doubtful, the more so since Legendre's first sentence seems to imply that he rated Argand as capable of a scientific career if he had been interested in it.

Almost all the available biographical information on Argand is due to Hoüel who undertook some research on the matter for his 1874 edition. All this information is based on Hoüel's assumption that Argand originated in Geneva (Hoüel 1874, ix). Hoüel gave no justification for this claim, and one can only guess that he came to this assumption by induction: Ami Argand (1750–1803), an inventor active in physics and chemistry, known for the construction of a lamp, who had lived a certain time at Paris, was in fact born in Geneva. This hypothesis led Hoüel to address colleagues at Geneva and to ask them to conduct biographical research. They came up with the information on birth date, first names and profession of a certain Jean-Robert Argand (ibid., xv-xvi). Although they added a cautionary remark about the identity ("C'est très probablement l'auteur du Mémoire de Mathématiques en question", ibid.), their information has been taken up to now as pure truth, in all related historical publications and in all biographical dictionaries.[6] The article on Argand in the *Dictionary of Scientific Biography*, for

distributor's address and indicated his own so that one should order copies of the booklet there.

[6]Only once, a doubt has been raised by an anonymous person (again!) in the journal *L'intermédiaire des Chercheurs et Curieux*: in 1875, one "T. de L." remarked that one has the biographical data of this Jean-Robert Argand, "mais on n'a pas la certitude qu'il s'agisse là de l'auteur de l'*Essai sur une manière de représenter les quantités imaginaires* [...]" (8e année, 1875, 424). He received two answers: the first declared Ami/Aimé Argand to be the *Éssai*'s author (9e année, 1876, 688) and the second only repeated

instance, shows no hesitations or doubts and even reports that the "verification of the dates of his birth and death is given by H. Fehr in the *Intermédiaire des mathématiciens*" in 1902. An inspection of this source shows, however, that Fehr had started from the conviction of correctness of Hoüel's hypothesis and just checked in the "Archives de l'État de Genève" the death date of that Jean-Robert Argand born in Geneva.[7]

What objective information about Argand do we have? Unfortunately, we do not even have any primary evidence as to Argand's first name: his letters as printed by Gergonne show only his last name. Also the tables of contents of Gergonne's *Annales* cite him by his last name only. The only objective information is his address in 1813 in Paris. Unfortunately again, as a result of the fights of the *communards* in Paris 1870/71, most of the registers of the administration of Paris are lost and it is therefore impossible to infer anything about the citizens living there from an address alone. The death registers were also burnt.[8] Since historians of the nineteenth century failed to do adequate biographical research, it will now be quite complicated to obtain better information. One can, at least, infer from Legendre's letter that in 1806 Argand was unknown in the scientific community. Moreover, it is clear from his immediate reactions in 1813 and 1814 that he had easy and regular access to scientific journals. Apart from this we can only state — in terms recalling medieval history — that Argand "flourished" in 1806, 1813, and 1814.

As regards his profession, there are two clues which point in the same direction. Firstly, the address for distribution as given in the original printing is the shop of a clockmaker. And secondly, the very first paper which

Hoüel's affirmations (ibid., 715). These sources are mentioned in the (relatively) new: *Dictionnaire de Biographie Française*, t. III, 1939, 504.

[7] *L'Intermédiaire des mathématiciens*, 9(1902), 74. Fehr had answered to a query by G. Eneström in 1900 in this journal; Eneström had remarked that the death date of Argand was unknown. This unique journal functioned in an analogous way to the electronic-mail newsgroups of today. The journal "L'intermédiaire des chercheurs et curieux" was a somewhat parallel journal addressing all the sciences and the arts. As I have learnt from the Archive d'État of Geneva, Fehr had more information at his disposal than he published later in his answer to *L'Intermédiaire*: the Archive had communicated to him in 1901 — quoting from *the acte de décès* submitted to the Geneva registration office in 1822 by the *mairie* of the tenth Paris *arrondissement* — that Jean-Robert Argand had been an employee at the hospital of the *Garde Royale*. Had Fehr been afraid that this additional information would falsify this person's authorship?

[8] Communication by the Direction des services d'Archives de Paris.

Argand had sent for publication — his 1813a, written in February 1813, occasioned by a problem announced in the *Annales* in 1812, and published several months before Français's paper (1813a) which opened the debate about Argand's *Essai* — discusses technical constructions of artisans from an expressly theoretical point of view. Argand shows himself in this paper as closely familiar with all the details of technical production, in particular of portable thermometers in the form of clocks, and as knowledgeable in recent scientific publications. For example, he quotes Laplace's *Système du Monde* in order to criticize certain shortcomings of practitioners (Argand 1813a, 39). In his conclusion, Argand pleads against the trial-and-error practices of artisans, which devalue the work of even the highly skilful, and pleads for guidance by theoretical principles. His claim to have achieved better practical results by such guidance (ibid., 41) suggests that Argand was a scientifically oriented technician, based in the Parisian clock industry.

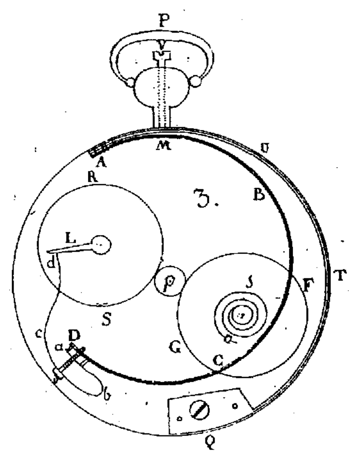


Figure 2: Figure of Argand's construction of a portable thermomenter (Argand 1813a, Planche I)

It is useful to try to create a reconstruction of the events around Argand's *Éssai*. A fairly probable one is the following. In the autumn of 1806, Legendre was approached by Argand, who tried to outline the main results in his manuscript to him in a direct conversation. Legendre responded with skepticism as to the method and its applications. Upon leaving, Argand urged Legendre to read his manuscript. Legendre had not retained the name of this man and assumed that the manuscript would show the name of its author. When Argand had left, Legendre realized that the paper indicated neither the address nor the name of the author. Upon reading the *Éssai*, Legendre noticed its quality, he waited for a further visit from its author, but the author did not appear again. In order to end his own involvement with these conceptions he wrote the report to Français in the letter of 2 November 1806. Since Legendre firmly asked not to be bothered with discussions on this paper, neither the elder nor later the younger Français dared to ask him about the paper or its author. On the other hand, Argand — apparently a shy man — abstained from publishing his paper, due to Legendre's uninterested and sceptical reaction. Only the quite indirect reception of his ideas via the brothers Français induced Argand to organize a later printing where he arranged for the date of its composition to be put on the title page.

2 Adrien-Quentin Buée

The third author productive around the same period is Adrien-Quentin Buée (1748–1826), a French Catholic Priest — a "prètre réfractaire" as the French during the Revolution called those priests who refused to give their oath on the Constitution. Buée fled in 1792 to England and returned to France in 1813. His paper *Mémoire sur les quantités imaginaires* was read in 1805 at the Royal Society and was published in 1806.

Buée's paper is remarkable in its systematic evaluation of the earlier French discussion on the nature of negative and imaginary quantities. He achieves the establishment of a conceptual connection between the two basic concepts of length or absolute value and of direction which had been separated so systematically in France during the 18th century.

Buée's achievements are likewise important for the conceptual development of the negative numbers and for the graphical representation of the complex numbers.

The important step in Buée's approach is that he clearly distinguishes between the two different meanings of the signs plus and minus, namely as

— signs of operations, on the one hand, and

— signs of qualities of the quantities themselves, on the other. As he notes:

> Des Signes "+" et "−"
>
> Ces signes ont des significations opposées.
>
> Considérés comme signes d'opérations arithmétiques, "+" et "−" sont les signes, l'un de l'addition, l'autre de la soustraction. Considérés comme signes d'opérations géométriques, ils indiquent des directions opposées (Buée 1806, 23).

Buée interpreted this sign of quality in geometrical terms, as *direction*, while he attributed *length* to an arithmetical meaning. His important step was not restricted, however, to this conceptual clarity of distinguishing between what he called *arithmetical operation* and *geometrical operation*, rather his decisive step was to propose to *unite* both operations:

> Lors donc qu'on réunit ces deux opérations, on fait réellement une opération *arithmético-géométrique*,

Buée immediately applied his approach to the investigation of imaginary quantities and hence expanded the applicability of the novel arithmetico-geometrical operations:

> Je mets en titre, *Du signe* $\sqrt{-1}$, et non *De la quantité ou De l'unité imaginaire* $\sqrt{-1}$; parceque $\sqrt{-1}$ est un signe particulier joint à l'unité réelle 1, et non une quantité particulière. C'est un nouvel adjectif joint au substantif ordinaire 1, et non un nouveau substantif (ibid., 27).
>
> Ainsi $\sqrt{-1}$, est le signe de la PERPENDICULARITÉ, dont la propriété caractéristique est, *que tous les points de la perpendiculaire sont également éloignés de points placés à égales distances, de part et d'autre de son pié.* Le signe $\sqrt{-1}$, exprime tout cela, et il est le seul qui l'exprime (ibid., 28).

This novel approach where the perpendicular direction is treated in a manner analogous to the two traditional opposed directions on a line, is well explained by Buée's following figure (see Figure 3).
Like Wessel and Argand, Buée was no professional mathematician. As a Catholic priest exiled in Britain, he engaged in publishing political-religious tracts. Except for his paper on the imaginaries, his sole other scientific paper is on physics. And as Valentiner did in the case of Wessel, one can wonder how an author of political-religious tracts could arrive at such a level of *clarté* regarding the foundations of mathematics.

In order to further complicate the matter one can add that there was even a fourth inventor, active in about the same period: Henri-Dominique **Truel**.

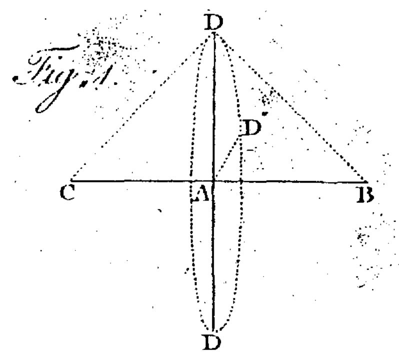


Figure 3: Figure 1 (Buée 1806, planche annexée)

Truel's existence and activity are asserted by Cauchy, who attributed the priority of the discovery of the graphical representation to him. Truel, who is not known otherwise, was called "un savant modeste" by Cauchy, who reported that Truel had found the greatest part of Buée's and Argand's results by 1786 but that he had concealed them in his manuscripts. By 1810, Truel had communicated his research to a marine engineer, Augustin Normand, at Le Havre, and it seems that Cauchy learned via Normand — during his engineer's service at Cherbourg — of Truel's work and of this graphical representation in particular (Cauchy 1847/1938, 175).

3 François Daviet de Foncenex

Fortunately enough, Buée has given us a clue as regards the source of his innovation. I have to confess that I noticed this hint quite late since this is a remark at the end of his paper; maybe other authors missed it entirely since it is mentioned nowhere in the literature. This indication is given as a "Postscriptum":

> Since I wrote this mémoire, I have read — in the first volume of the Turin Academy Transactions a paper by M. Foncenex with the title: *Reflèxions sur les Quantités Imaginaires* where one finds the following paragraph (Buée 1806, 83; my translation, G. S.).

He quoted this paragraph 6 completely. Due to its importance for our investigation, I will quote it completely, too:

6. Si l'on réflechit sur la nature des racines imaginaires, qui comme on sait impliquent contradiction entre les données, on concevra évidemment qu'elles ne doivent point avoir de construction Géomètrique possible, puisqu'il n'est point de maniére de les considérer, qui lève la contradiction qui se trouve entres les données immuables par elles mêmes.

Cependant pour conserver une certaine analogie avec les quantités négatives, un Auteur dont nous avons un cours d'algébre d'ailleurs fort estimable a prétendu les dévoir prendre sur une ligne perpendiculaire à celle où l'on les avoit supposé, si par exemple (pl. I. Fig. I) on devoit couper la ligne $AB = 2a$ de façon que le rectangle des parties $x \times (2a - x)$, fut égal à quantité $2a^2$ on trouveroit $x = a \pm \sqrt{(-a^2)}$, pour trouver donc cette valeur de x, qu'on prenne sur la ligne AB, la partie $AC = a$ partie réelle de la valeur de x, & sur la perpendiculaire ED les CE, CD aussi $= a$, on aura les points D, E qui resolvent le problême en ce que $AD \times DB$, ou $AE \times EB = 2a^2$, mais puisque les points E, & D sont pris hors de la ligne AB, & qu'une infinité d'autres points pris de même, auroient aussi une propriété semblable, il est visible, que si cette construction ne nous induit pas en erreur, elle ne nous fait absolument rien connoître, c'est cependant là un des cas ou elle pourroit paroître plus spécieuse, car le plus souvent on ne voit absolument pas comment le point trouvé pourroit résoudre la question, quelques changemens qu'on se permit dans l'énoncé du problême.

Les racines imaginaires n'admettent donc pas une construction géomètrique, & on ne peut en tirer aucun avantage dans la résolution des problèmes: on devroit par conséquent s'attacher à les écarter autant qu'il est possible des équations finales, puisque prises dans quel sens que ce soit, elles ne peuvent pas résoudre la question, comme les racines négatives, dont toute la contradiction consiste dans leur maniére d'être à l' égard des positives (Foncenex, 1759, 122–123).

Who was this Foncenex? François Daviet de Foncenex (1733/4–1799) was an officer, living at Turin, interested in the sciences and in engineering. He has published — besides the paper on imaginaries — several papers on physics and technology. He is said to have been a friend of Lagrange during Lagrange's stay at Turin and one even reports that Foncenex' 1759 paper expressed thoughts of Lagrange.

This sixth paragraph leads to at least three questions:

— an analysis of its mathematical meaning,
— the reception of this part of the paper,
— the identification of the algebra textbook mentioned at the beginning of the 6th section.

As regards the analysis of the text, one should note beforehand that it is *not* mentioned in the historical literature — not even by Cajori who is the only one to extensively discuss other parts of the *mémoire*.[9]

Looking concretely at the text, it must be said that it is ambiguous and even contradictory. This character becomes visible only by considering the figure which is printed in another part of the volume:

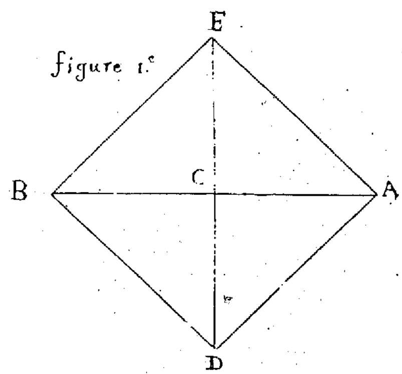


Figure 4: Foncenex 1759, fig. 1: planche annexée

On the one hand, Foncenex clearly shows how one can construct the imaginary quantities geometrically, and visualizes this construction by means of this figure. In fact, transforming the equation

$$x \times (2a - x) = 2a^2$$

for x immediately yields:

$$x = a \pm \sqrt{a^2 - 2a^2} = a \pm ia.$$

In the text, Foncenex assures us that the construction does not lead to errors.

[9]The only exception is Pieper, who just mentions Foncenex's interdiction verdict (Pieper 1984, 214).

On the other hand there is the commenting text before and after the constructive part. This commentary sets up signs saying that such numbers are "off limits!" For instance, in the introductory comment, Foncenex gives an epistemological argument:

> Evidently, one understands that imaginary roots do not admit a possible geometric construction

and in the concluding comment:

> imaginary roots do not, **hence**, admit a geometrical construction, and one cannot infer any advantage in resolving problems by using them: one has therefore to pay careful attention to eliminate them, as fas as it is possible, from the **final** equations (my emphasis, G. S.).

The mathematical legitimation which Foncenex gave for his refutation is rather weak: He concedes that the construction is correct but argues that one learns nothing new from it. And he adds that one can construct an infinity of other points with analogous properties.

This analysis of the sixth section already leads to an at least partial answer to the second question: Among the readers of Foncenex's *mémoire*, there certainly were some who became more convinced by the force of the visualization of a geometrical construction and who did not let themselves be deterred by epistemological interdictions. It is quite probable that one might find, among such readers, marginal figures who as autodidacts might have been less permeated by the dominant epistemology than those who adhered professionally to the mathematical norms.

And one has to know that the readers of Foncenex's *mémoire* were numerous: Foncenex had written this paper to investigate the difference of opinion between d'Alembert and Euler about the admissibility and the meaning of logarithms of negative quantities (cf. Youschkewitsch, Taton in: Euler 1980).[10] I cannot enter here into a presentation of this year-long controversy which reveals highly illuminating epistemological dimensions of basic mathematical concepts. However I should mention that they ended this controversy by exhaustion, without either of them convincing the other. The fact that two famous mathematicians had not been able to reach a consensus

[10] The strong reception of Foncenex's paper is due to d'Alembert's paper of 1761 where he made his controversy with Euler publicly known for the first time. In an appendix to this paper written already in 1752, d'Alembert extensively discussed Foncenex's arguments.

about basic concepts, attracted the attention of a great many people during the second half of the eighteenth and at the beginning of the nineteenth century who tried to develop their own solution. The problem seemed to be accessible by "common sense" and attracted, hence, in particular the interest of amateurs. Almost everyone who worked on the problem and subsequently wrote about it stumbled on Foncenex' *mémoire* and read it.[11]

I can now pass on to the third question: who is the author not named by Foncenex who published a highly esteemed algebra textbook where the construction of imaginary roots on a perpendicular line was proposed?

Foncenex's paper led me to assume that he had an author of his own time in mind. Consequently, I consulted a great many algebra textbooks of his time and of the entire first half of the eighteenth century; I consulted even general textbook series for mathematics since they contain parts on algebra, too. I detected, however, no author who had discussed or allowed such geometrical constructions. My subsequent evaluation of Robin Rider's bibliography of publications on algebra gave no better result (cf. Rider 1982).

4 John Wallis

The only solution seems to be that Foncenex alluded to John Wallis's algebra treatise, published in 1685 in English and in 1693 in Latin, the latter in a somewhat extended form. This first proposal for a geometric construction is relatively well known so that I will just recall the structurally important points.

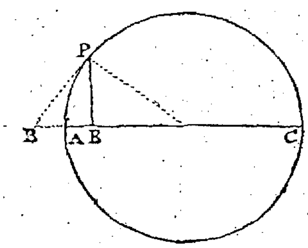
On the one hand, Wallis denied there that quantities less than zero can exist. On the other hand, he added that such a supposition is nevertheless not useless and not absurd; in fact, Wallis continued to freely develop operations with negative quantities (Wallis 1693, 286).

Using this conceptual basis, Wallis was the first to admit a geometrical construction of imaginary roots by interpreting them as mean proportionals:

[11] Buée, e.g., protested against Foncenex's principle of excluding imaginary roots (Buée 1806, 85). Buée's claim to have read Foncenex's paper only after completing his own one seems to be a "Schutzbehauptung": In quoting Foncenex's paragraph no. 6, he refers to his own — quite analogous — figure; Buée has not only renamed the points within that quote according to his own figure but also tacitly corrected a misprint in Foncenex's original text.

Firstly, he explained the well known fact that $\sqrt{bc}$ is the mean proportional between $+b$ and $+c$, and then he formally extended this interpretation to $\sqrt{bc}$ as mean proportional between $-b$ and $-c$. Eventually, he rather postulated than demonstrated that $\sqrt{-bc}$ can also have the signification of the mean proportional between $+b$ and $-c$. Likewise, he interpreted $\sqrt{-bc}$ as the mean proportional between $-b$ and $+c$ (ibid., 287).

Wallis exemplified his algebraic argumentation by several geometrical constructions. As an introduction to his examples, he even seems to have been the first who visualized the quadrants of the plane with their respective signs, by a graphic (ibid.). Then, he showed how one can interpret trigonometric lines as the geometric construction of some imaginary quantity. For example, he showed, after having chosen lines a and b in a circle conveniently ($AB = +b, BC = +c$), that the tangents can be represented as the mean proportional $\sqrt{-bc}$ (ibid., 288):



Verbi gratia; Si prorſum ab A ſumatur AB=+b, & prorſum adhuc (in eadem recta) BC=+c; ſitque AC (=AB+BC=+b+c) diameter circuli: erit ſinus rectus, ſeu media proportionalis, BP=√+bc.

Sin retrorſum ab A (adeoque cum contrario ſigno) ſumatur —AB=—b; &, à B prorſum, BC=+c; manente eadem circuli diametro AC=—AB+BC =—b+c: erit Tangens, ſeu media proportionalis, BP=√—bc.

What was the effect of this textbook and in particular the impact of this method for interpretation? I could find no trace of a discussion or a reception by contemporaries of Wallis or of later authors of the first half of the eighteenth century. This silence is the more astonishing since Wallis had expressed this approach in an earlier letter. In a letter of 6 May 1673, addressed to John Collins (1625–1683), (who was acting as secretary of the *Royal Society)*, Wallis explained his interpretation of imaginary roots as "mean proportionals" in the context of a discussion on Cardan's rules for solving higher degree equations.

As Wallis reported, he had earlier on had scruples as "too young an algebraist to innovate without example". But since he had become "more venturous" in the meantime, he "had several projects" for "designing geometrically" imaginary roots (Rigaud 1841, 578). It seems that Collins did not raise objections to Wallis's argumentation.

It seems, therefore, that Wallis was the first to propose a geometric construction of imaginary quantities but that he remained without an echo and an impact in his time, or even later on. Foncenex's discussion means a new beginning of related reflections.

5 Wenceslaus J.G. Karsten

As a concluding element, I will present a little-known but highly instructive example of a direct impact of Foncenex's *mémoire*, one which gives a much more sophisticated geometric construction than those discussed up to now.

It is a paper on the logarithms of negative quantities, by W.J.G. Karsten (1732–1787), a mathematics professor at one of the smallest German universities, at Bützow, when he published its first version in 1768. Karsten was later professor at Halle, then ranking as the second university in Germany, when he published a revised version in 1786.

In this paper, which referred to Foncenex, Karsten, too, discussed the possibility and legitimacy of a geometrical construction of imaginary quantities. He asserted, firstly, that an algebraically impossible (i.e. imaginary or complex) quantity does not admit a geometrical construction. He continued nevetheless his reflection, by introducing the distinction that if an algebraically impossible quantity is given in the form $b\sqrt{-1}$ one can construct the possible factor b geometrically. Karsten remarked that it would be sufficient to bear in mind that the geometric quantity thus constructed is not identical with the quantity sought (Karsten 1786, 379).

He went on to give, as an example, a particular relation between a hyperbola and a circle. Karsten defined an equilateral hyperbola by the equation $x^2 - y^2 = 1$ and the related circle by $x^2 + z^2 = 1$, with $y = z\sqrt{-1}$ (ibid., 380, see figure 5) . In this case, for $x = 0$ the ordinate is imaginary: $y = \pm\sqrt{-1} = \pm 1\sqrt{-1}$, and putting $CE = CF = 1$ yields $y = \pm CE\sqrt{-1}$ and $\pm CE = -y\sqrt{-1}$; in general, all values y of the hyperbola for x between $+1$ and -1 are imaginary. And Karsten explained that one is able in this way to draw ("zeichnen") the respective ordinates for all these x: these ordinates are in themselves ("an sich") possible quantities, but they appear as imaginary quantities here in the calculus since they cannot intersect the hyperbola and are hence imaginary, regarded as ordinates of the hyperbola. Moreover:

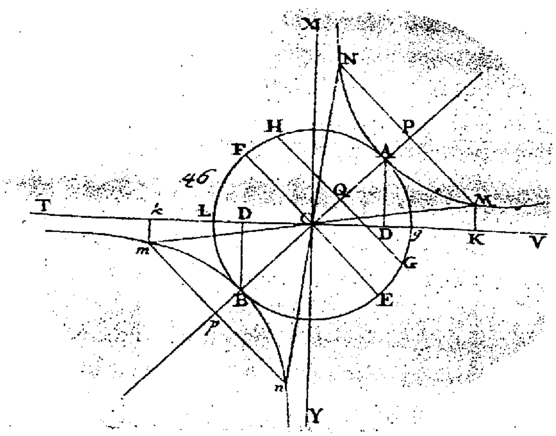


Figure 5: Fig. No. 46

> All ordinates of this circle are imaginary ordinates of the hyperbola, since $z = \pm y\sqrt{-1}$; but also vice-versa, all ordinates of the hyperbola are imaginary ordinates of the circle, since $y = z\sqrt{-1}$. As a consequence, the circle is an imaginary part of the hyperbola, as the hyperbola is an imaginary part of the circle. (Karsten 1786, 380–381; my translation, G. S.).[12]

And Karsten, who was aware of the periodicity of the circle, even showed that there is an infinity of such correspondences (ibid., 381 ff.).

Resuming his discussion, Karsten asserted that the geometric design of an algebraic formula can never lead to consequences which are different from those obtained by algebraic methods (ibid., 385). Remarkably, by this point Karsten had skipped over the distinction between 'possible' and 'impossible' algebraic terms.

Cajori seems to have been the only one who has discussed Karsten's paper. He commented in the following terms that he could see no reception:

> It looks very much as if transactions of academies had been in some cases the safest places for the concealment of scientific articles from the scientific public (Cajori 1913, 111).

[12] I am quoting here from the revised later version. Although profoundly revised, the example of this construction with the hyperbola and the circle had remained unchanged as compared to the first version (Karsten 1768, 96–98).

One has to add that Cajori did not know that Karsten had organized a second printing of his paper in 1786, which certainly did not remain unnoticed.

Actually, we have not studied sufficiently the impact of the enormous number of foundational reflections by mathematicians who are not regarded as great mathematicians on the evolution of the thinking of the greater mathematical community.[13] The foundational work of marginal contributors prepared well the passage from an epistemology favouring a geometrical relation to real-world existence to an epistemology favouring internal systemic coherence. It is quite paradoxical that this passage paved its way by reinforcing the geometrical legitimation of abstract mathematical objects. Foncenex's proposal to admit imaginary quantities only as auxiliary means during the calculation process but not in the final solution provoked the eventual breakthrough of their general admission as algebraic concepts.

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[13] One further indication for the wide diffusion of the idea of geometrical representation is a review in the Göttingische gelehrte Anzeigen of 6 March 1806 by Tobias Mayer, physics professor at the Göttingen University, where he asked for concrete modes of geometrical construction for imaginary roots. As a source, he pointed to a paper by Heinrich Kühn of 1750 in the Petersburg Academy Transactions. Despite its promising title, it is a rather confused paper since the author attributes to the same area of, say a square, a positive or a negative value — depending on in which quadrant it is situated — and deduces from negative area values that the corresponding side lengths are imaginary (Kuehn 1753).

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Inexplicable? The status of complex numbers in Britain, 1750–1850

Adrian Rice *

"The history of algebra shows us that nothing is more unsound than the rejection of any method which naturally arises, on account of one or more apparently valid cases in which such method leads to erroneous results. Such cases should indeed teach caution, but not rejection: if the latter had been preferred to the former, negative quantities, and still more their square roots, would have been an effectual bar to the progress of algebra, which would have been confined to that universal arithmetic of which Newton wished it to bear the name: and those immense fields of analysis ... would have been not so much as discovered, much less cultivated and settled." [De Morgan 1842b, 566]

1 Introduction

The period from the mid-18th to the mid-19th century is generally regarded as the time when complex numbers were finally accepted as legitimate algebraic objects by the mathematical community. It is also widely viewed that it was the work of Carl Friedrich Gauss (1777–1855) which was primarily responsible for bringing these numbers into the mathematical mainstream, with his proofs (in 1799, 1815, 1816 and 1849) of the fundamental theorem of algebra, as well as his description of their geometrical representation in

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1831, serving as the final impetus to their eventual acceptance. [Lewis 1994, 725]

However, while this is basically true, especially for countries at the heart of the mathematical mainland of western Europe, such as France and Germany, the story of the acceptance of complex numbers was a little different in the British Isles.[1] So, although the general recognition of complex numbers dates from around the same time in Britain as on the rest of the continent, there were noticeable differences in the underlying causes. One of these was the relatively isolated position of British mathematicians arising from their rejection of the continental approach to the calculus and related topics.[2] Thus, while still in touch with developments in mainland Europe, mathematicians in Britain were inevitably slower in assimilating results from the continent than might otherwise have been the case.

The country's peripheral geographical position, together with the effects of a prolonged war with France, also resulted in the slight delay and limited availability of continental publications (especially obscure ones) reaching its shores. Consequently, if this paper neglects to draw detailed attention to the work of, for instance, Wessel, Argand or Grassmann, it is because their British contemporaries would have known little or nothing of them — although in this regard, they were far from unique. For example, it should be said that in cases such as that of Grassmann, whose work was almost unintelligible to contemporary mathematicians, many of his compatriots were equally unacquainted with his work. [Lewis 1977]

One final factor was the distinctive intellectual environment in Britain at this time, with its emphasis on wide-ranging amateurism rather than professional specialisation, in which mathematical questions were often intimately connected to more general philosophical problems. Thus the British debate over complex numbers has been interpreted as part of "a three-quarters-of-a-century dialogue on general terms and sound reasoning in which major British thinkers intricately interwove mathematical and philosophical insights". [Pycior 1984, 438] In this view, the change in predominant philosophy among British scientists in this period, from Locke to Berkeley to

[1] By this, as well as by the word "Britain", I refer to the countries of England, Wales, Scotland and Ireland. The latter was officially incorporated as part of Britain by the Act of Union in 1801, although it had been effectively under British rule for centuries.

[2] Although this isolation was not as complete as earlier historians have suggested. [Guicciardini 1989, vii–viii]

Kant, resulted in a corresponding shift in mathematical point of view towards, among other things, complex numbers.

The purpose of the present article is to survey these changing British attitudes during this time, a subject which, while it has been the subject of much recent research, still awaits a definitive history. It will therefore proceed as follows. Following a brief summary of the early history of complex numbers (§2), it will review the peculiarly British position towards them in the period up to around 1830, in order to determine precisely how they eventually came to be accepted in Britain (§§3–4). Sections 5 and 6 will then highlight the largely forgotten but influential work on imaginary numbers of two British mathematicians, John Warren and John Thomas Graves, the details of which have been essentially overlooked by recent historians of mathematics. The influence of this work is traced in §7, via the familiar story of the birth of quaternions, concluding with a survey of how the notion of "inexplicable quantities" subsequently came to be extended.

2 The first 200 years

The early history of imaginary and complex numbers is well documented. Their first recorded use is by Girolamo Cardano (1501–1576) in his *Ars Magna* of 1545. There he poses the problem of dividing 10 into two parts such that their product is 40, obtaining the solutions $5+\sqrt{-15}$ and $5-\sqrt{-15}$. However, his attitude towards them was hardly progressive. Indeed, given contemporary reservations about the validity of negative numbers in algebra, it is hardly surprising that Cardano describes the use of these new forms as involving "mental tortures" [Cardano 1968, 219].

It was Descartes who, in *La Géométrie* (1637), introduced the term "imaginary" [Smith and Latham 1954, 175] to describe such entities,[3] although he himself did not regard them as actual numbers. From the mid-17th century onward, however, mathematicians appear to have had fewer qualms about imaginaries and made frequent use of them. Newton believed that complex roots were helpful for determining which problems have real or explicable solutions. He said: "it is just that the Roots of Equations should be often

[3] Albert Girard, in 1629, referred to them as "solutions impossibles" — the phrase "impossible numbers" was widely employed during the eighteenth and early-nineteenth centuries. It was Gauss who introduced the term "complex" in 1831. [Gauss 1876, 102]

impossible, lest they should exhibit the cases of Problems that are impossible as if they were possible." [Newton 1728, 193] Yet their epistemological status remained unclear, as reflected in Leibniz's description of them as "that amphibian between being and not-being, which we call the imaginary root of negative unity" [Leibniz 1858, 357].

Not surprisingly, considering their ambivalent status, by the early 18th century, results concerning negative and complex numbers were the cause of heated debates among many of the leading mathematicians. One of the most vigorous discussions concerned the nature of their logarithms. [Cajori 1913, 35–47, 75–84] During this period, Leibniz and the Swiss mathematician Johann Bernoulli put forward contrasting opinions on the subject, the latter proving for example that

$$\pi = \frac{2 \log \sqrt{-1}}{\sqrt{-1}}.$$

Related results soon followed which quickly established a connection between logarithms, exponentials and trigonometric functions. In 1714, the Englishman Roger Cotes (1682–1716) was able to show that

$$x\sqrt{-1} = \log(\cos x + \sqrt{-1}\sin x),$$

which of course is equivalent to Euler's later result

$$e^{x\sqrt{-1}} = \cos x + \sqrt{-1}\sin x.$$

Another theorem which originated in England at this time was the following, which Euler was also able to re-formulate and prove:

$$(\cos x \pm \sqrt{-1}\sin x)^n = \cos nx \pm \sqrt{-1}\sin nx, \quad n \in \mathbb{R}.$$

This result is implicitly used in the work of the French emigré Abraham de Moivre (1667–1754), although he never stated it or attempted a proof. [Schneider 1968]

The question of imaginary logarithms was finally resolved by Euler in 1749. By writing a complex number $x = a + b\sqrt{-1}$ as $r(\cos\theta + \sqrt{-1}\sin\theta)$, where $r = e^c$, he obtained

$$x = e^c(\cos\theta + \sqrt{-1}\sin\theta) = e^c e^{\sqrt{-1}(\theta \pm 2n\pi)}.$$

Thus

$$\log x = c + \sqrt{-1}(\theta \pm 2n\pi), \quad n \in \mathbb{N} \text{ or } n = 0.$$

He therefore concluded that, when x is positive and real, one value of the logarithm will be real, but for negative and imaginary values of x, all logarithms will be imaginary. [Euler 1749] But despite this successful resolution, no one was any clearer as to what complex numbers actually were, or whether it was mathematically valid to use them.

Moreover, because of the anti-intuitive nature of the results obtained by their use, even those who admitted their value in mathematical research were liable to make mistakes. For example, although Euler correctly calculated that $\sqrt{-1}^{\sqrt{-1}} = e^{-\pi/2} = 0.20787957\ldots$, he omitted to mention the other values this number could be. Another error, made by Bernoulli and D'Alembert, was that $(-a)^2 = a^2$ implied $\log(-a)^2 = \log a^2$. This led them to the result that $2\log(-a) = 2\log a$, or $\log(-a) = \log a$. [Woodhouse 1801, 114]

The resulting mixture of correct and erroneous conclusions concerning negative and complex numbers served only to intensify the confusion. As late as 1815, the British mathematician Charles Hutton reported that "the arithmetic of these imaginary quantities has not yet been generally agreed on" [Hutton 1815, I, 675], contrasting Euler's claim that $\sqrt{-2} \times \sqrt{-3} = \sqrt{6}$ with William Emerson's[4] belief that $\sqrt{-a} \times \sqrt{-b} = \sqrt{-ab}$. "And thus," he concluded, "most of the writers on this part of algebra are pretty equally divided." [Hutton 1815, I, 675]

3 The British debate 1750–1830

The unease with negative and imaginary numbers was especially strong in Britain. [Pycior 1997] The primary concern was that the lack of a proper definition of negative numbers, and hence imaginary quantities, rendered them meaningless. John Wallis (1616–1703), for example, in his *Arithmetica Infinitorum* (1655) had expressed the view that negatives were not only greater

[4]Emerson (1701–1782), an English mathematician, was later described as "the writer of many works, which had considerable celebrity: but he was as much overrated as Thomas Simpson was underrated" [De Morgan 1847, 78].

than zero but actually larger than infinity, a view also held by Euler over a century later. [Kline 1972, 253, 593] Yet, throughout the 18th century, negative numbers were also variously defined as "quantities less than nothing" and "quantities obtained by the subtraction of a greater quantity from a lesser". [Pycior 1981, 28] Since it was almost universally acknowledged that there existed no adequate justification for either negatives or complex numbers, it is hardly surprising that opposition to their employment was eventually expressed.

This came in the form of *A dissertation on the use of the negative sign in algebra*, published in 1758 by Francis Maseres (1731–1824), a Fellow of Clare College, Cambridge and the Royal Society of London. In this work he advocated the complete rejection of all negative numbers as solutions to equations or algebraic processes. His purpose, as he explained in the preface, was to "remove from some of the less abstruse parts of Algebra, the difficulties that have arisen therein from the too extensive use of the Negative Sign, and to explain them, without considering the Negative Sign in any other light than as the mark of the subtraction of a lesser quantity from a greater". [Maseres 1758, i]

He went on to demonstrate how to evade such difficulties by separating those equations with negative roots from the rest and only admitting their positive roots. In Maseres' algebra, therefore, there was no place for negative (and therefore also imaginary) roots to any form of equation since "they serve only, as far as I am able to judge, to puzzle the whole doctrine of equations, and to render obscure and mysterious things that are in their own nature exceeding plain and simple ... " In his opinion, "It were to be wished therefore that negative roots had never been admitted into algebra, or were again discarded from it". [Maseres 1758, 34]

Although the *Dissertation* was somewhat radical in its emphatic denial of the validity of negative concepts, and although Maseres' extreme opinions were not shared by the majority of his contemporaries, it should not be assumed that he or his work were ignored or derided. On the contrary, Maseres was a competent and respected mathematician who, two years later, would compete (albeit unsuccessfully) for the prestigious Lucasian chair of mathematics at Cambridge. [Pycior 1997, 306–7] Indeed, the fact that a man of his intellectual stature was prepared to make such a bold statement added impetus to the need felt among the British to justify the obscure notions of negatives and imaginaries.

Towards the end of the century, Maseres was joined in his attack of these "impossible" quantities by another Cambridge mathematician, William Frend (1757–1841),[5] whose *Principles of algebra*, published in two volumes in 1796 and 1799, endorsed the abridged interpretation of the subject. In the *Principles*, Frend criticised foregoing algebra texts, including both Newton's *Arithmetica Universalis* (1707) and Maclaurin's *Treatise of Algebra* (1742), for attempting to define negatives by resorting to analogy, such as financial debts or directed lines: "when a person cannot explain the principles of a science without reference to metaphor, the probability is, that he has never thought accurately upon the subject". [Frend 1796, x]

Supporting Maseres' contention that negative and imaginary numbers were "a parcel of algebraick quantities, of which our understandings cannot form any idea" [Maseres 1778, 947], Frend branded such concepts as "jargon, at which common sense recoils", adding that "like many other figments, it finds the most strenuous supporters among those who love to take things upon trust, and hate the labour of a serious thought". [Frend 1796, xi] His chief concern was that if these poorly-defined concepts were used in algebra, even valid reasoning could produce incorrect results: "from false notions, falsehood must necessarily flow, if the reasoning employed upon them has been properly conducted". [Frend 1798, 3]

As a mark of his endorsement of Frend's work, Maseres provided an appendix on the solution of cubic and quartic equations. Here he observed that the treatment of this subject by previous authors "has been made the subject of much mysterious and fantastick reasoning, (or, perhaps, I ought rather to say, *discoursing*, since it deserves not to be called *reasoning*,) concerning negative and impossible quantities ... All these writers have ... treated this subject with an astonishing degree of obscurity, and almost as if they had been contending with each other which should treat it most obscurely". [Frend 1796, 253–4] In consequence, he claimed, "the Science of Algebra, or Universal Arithmetick, has been disgraced and rendered obscure and difficult, and disgusting to men of a just taste for accurate reasoning". [Maseres 1800, lv]

While Maseres and Frend may have been in the minority with their belief in the necessity of excluding negatives and imaginaries from algebra, they were very much in harmony with the majority of the mathematical community

[5] For a biography of Frend, see Knight 1971.

in their diagnosis of the problem. In the 75 years following Maseres' initial attack, a number of works by prominent British mathematicians responded to the anti-negative argument. Indeed, the last half of the 18th century and the opening third of the 19th saw the question of negative and imaginary numbers occupy a major place in the discussions of British mathematicians, philosophers and men of science. However, the overall consensus was that, although a satisfactory definition was still absent, their utility as indispensable mathematical tools meant that they could not be abandoned.

One of the first to come to their defence was the Scottish mathematician John Playfair (1748–1819) who, in a paper of 1778, presented his readers with the following paradox: "If the operations of this imaginary arithmetic are unintelligible, why are they not also useless?" [Playfair 1778, 321] His answer was that although imaginary numbers, and the operations performed on them, have no direct meaning themselves, they act as "notes of reference to others which are significant". [Playfair 1778, 326] Thus, although the results obtained through them are justified by argument from analogy, this does not impair their validity. Rather, the use of negatives and imaginaries serves as a means of deducing meaningful results when no other method is available. "For this reason, many researches concerning it, which in themselves might be deemed absurd, are nevertheless not destitute of utility." [Playfair 1778, 342]

In the following decade, William Greenfield, professor of rhetoric at the University of Edinburgh, entered the controversy. Although he conceded that "the Method of negative quantities ... is supported, rather by induction and analogy, than by mathematical demonstration," [Greenfield 1788, 134–5] he was opposed to the idea of their exclusion from mathematics. While agreeing that "the whole business of algebra might be carried on without the consideration of the negative roots," he stressed that since their admission "evidently affords so great elegance and universality to algebraical solutions; to find our author [Maseres] gravely declaring that he can see no advantage in it, is perfectly astonishing". [Greenfield 1788, 136] Indeed, Greenfield goes on to rebuke Maseres for failing to "exert his industry and ingenuity, rather to confirm than to destroy; rather to demonstrate, how far we might rely on the method of negative quantities, than to overturn at once so great a part of the labours of the modern algebraists". [Greenfield 1788, 136]

The debate continued into the 19th century with two works by a young

Cambridge mathematician, Robert Woodhouse (1773–1827); a paper published by the Royal Society in 1801 and a textbook entitled *The Principles of Analytical Calculation* which followed two years later. In the first of these, Woodhouse attempted to demonstrate why results obtained by means of imaginary numbers were necessarily true. To do this, he discarded Playfair's appeal to analogy, preferring to justify imaginaries by stressing the validity of their formal laws of operation as opposed to the meaning of the individual characters operated upon. As he argued, "a conclusion expressed by certain characters and signs, if general, must be true in each particular case that presents itself, on assigning specific values to the signs". [Woodhouse 1801, 93]

Thus, he asserted that since $(a+b)(c+d)$ and $ac+ad+bc+bd$ are algebraically equivalent for all real a, b, c and d, replacing the real quantities b and d with the imaginary numbers $b\sqrt{-1}$ and $d\sqrt{-1}$ would lead to the conclusion that $(a + b\sqrt{-1})(c + d\sqrt{-1})$ and $ac + ad\sqrt{-1} + cb\sqrt{-1} - bd$ are also equivalent forms. But, he said, these latter expressions are "not proved equivalent, but put so, by extending the rule demonstrated for the signs of real quantities to characters that are insignificant". [Woodhouse 1801, 93] Similarly, he claimed, just as $(a+b)^x$ was regarded as being equivalent to $a^x+xa^{x-1}b+\ldots$, so $(a+b)^{x\sqrt{-1}}$ was likewise equivalent to $a^{x\sqrt{-1}}+x\sqrt{-1}a^{x\sqrt{-1}-1}b+\ldots$ "not by any proof, but by the extension of a rule". [Woodhouse 1803, 8–9]

Thus, if either negative or imaginary quantities were to be made the subjects of mathematical demonstration, "it must be in consequence of some arbitrary rule". [Woodhouse 1803, 7] But since these rules had already been established for general terms, regardless of their meaning, he could therefore conclude that "although the symbol $\sqrt{-1}$ be beyond the power of arithmetical computation, the operations in which it is introduced are intelligible, and deserve, if any operations do, the name of reasoning".[6] [Woodhouse 1801, 108]

An alternative defence was offered by the French expatriate Adrien-Quentin Buée in 1806. Unlike Woodhouse, however, he emphasised the need for meaning in algebra. Dividing it into "universal arithmetic" and a "mathe-

[6] It is interesting to note that, despite his vigorous defence of the use of complex numbers in algebra, in a letter to Maseres, dated November 16, 1801, Woodhouse writes: "Till the doctrines of negative and imaginary quantities are better taught than they are at present taught in the University of Cambridge, I agree with you that they had better *not* be taught". [De Morgan 1842a, 147]

matical language", where the symbols + and − were also characterised by the vaguely-defined notion of "quality", Buée returned to Playfair's philosophy of analogy to make his case. Thus "when one says that a negative quantity is smaller than zero ... it is not the *quantity* which is smaller than *zero*; it is the *quality* which is inferior to *nullity*. For example, if my debts exceed my assets, I am poorer than if I had neither assets nor debts". [Buée 1806, 25] While this paper contained some original ideas on the meaning and interpretation of both negative and imaginary quantities (including geometric representation), it was "presented in a very vague and unscientific form ... [and] accompanied by many other attempts which were either imperfect or altogether erroneous". [Peacock 1830, xxvii]

It was in a critique of Buée's memoir, published 30 years after his initial foray, that Playfair made a second contribution to the debate. Rejecting all intervening explanations, he insisted that while the utility of imaginaries was certain, neither the numbers themselves nor the operations performed on them could be assigned any meaning. He illustrated his point by considering the operation of dividing by an imaginary number: "what is meant by dividing by an impossible quantity, or telling how often an impossible quantity contains another; if the quantity be impossible, to multiply or divide by it, or to make it the subject of any arithmetical operation, must be impossible also. The operations performed with the symbols are therefore destitute of meaning; they are as imaginary as the symbols themselves." [Playfair 1808, 315]

And so the debate rumbled on for the next twenty years, with additional articles and textbooks adding further opinions to those already mentioned. For the most part, however, these contained mainly reiterations or minor variations of views already aired [Bonnycastle 1820, II, 24–25n; Hutton 1815, I, 675, II, 93]. The final significant contribution came from Davies Gilbert (1767–1839) in his valedictory address as President of the Royal Society in 1830. Although not a practising mathematician, Gilbert was able to offer some perceptive remarks on the current status of imaginaries in mathematics. He referred to them as "Creations merely of arbitrary definition, endowed with properties at the pleasure of him that defines them". [Gilbert 1831, 93] Their use in mathematics is thus perfectly justified since, although an imaginary quantity has only potential existence while the $\sqrt{\ }$-symbol is attached to the -1, "it stands ready to exist in energy whenever that sign is removed". [Gilbert 1831, 94]

The year of Gilbert's address marks the end of three-quarters of a century of dispute over the status, meaning, interpretation and validity of negative and imaginary numbers. This was not because interest in the subject had waned or a consensus had been reached, but because of the publication of a new work which offered a new approach to the practice of algebra and effectively rendered the question of the legitimacy of complex numbers immaterial. That work was *A Treatise on Algebra* by George Peacock (1791–1858), the first publication to propose a purely symbolic approach to the subject — although, as we shall see, some aspects of the new philosophy were not entirely novel.

4 Peacock's symbolism: a new approach?

The algebraic work of George Peacock[7] represented the most significant contribution to the (British) debate thus far. This is largely to be found in two main sources. The first was his aforementioned *Treatise on Algebra*, published in 1830, of which a second edition, substantially revised and extended, appeared in two volumes in 1842 and 1845. His other principal publication was a book-length "Report on the recent progress and present state of certain branches of analysis", which he presented at the third meeting of the British Association for the Advancement of Science in 1833. With these two works, Peacock initiated a different, more abstract, algebraic methodology, which fundamentally altered the way the subject was perceived in Britain.

In common with recent authors on the subject, Peacock was in full agreement with the claims of Maseres and Frend regarding the inadequate justification of negatives and imaginary numbers in mathematics. However, he was also adamant that any restriction which limited algebra merely to a form of universal arithmetic would be equally unsatisfactory. This, he explained, was because of the "great multitude of algebraical results and propositions, of unquestionable value and of unquestionable consistency with each other, which were irreconcilable with such a system, or, at all events, not deducible from it, ... and which made it necessary to consider negative and even impossible quantities as having a real existence in algebra, however vain might be the attempt to interpret their meaning". [Peacock 1833, 190–1]

[7] Like Maseres, Frend and Woodhouse, Peacock was a Cambridge man, being a fellow and tutor of Trinity College, Cambridge, and later the University's Lowndean professor of mathematics.

The root of the problem, he decided, was that "Algebra has always been considered as merely such a modification of Arithmetic as arose from the use of symbolical language, and the operations of one science have been transferred to the other without any statement of an extension of their meaning and application". [Peacock 1830, vi] This had led to the erroneous practice of using arithmetical symbols in algebra as if they represented the same operations with fewer limitations. The result was that, by appealing to a basis in arithmetic, mathematicians had enlarged the scope of these operations without adequate justification. In other words, they had taken a special case and used it to justify a generalisation.

Peacock's solution was to divide algebra into two distinct subjects, arithmetical and symbolical. Arithmetical algebra was basically the system of universal arithmetic insisted upon by Maseres and Frend. Here, symbols could only represent positive real numbers, "whether abstract or concrete, whole or fractional, and the operations to which they are subject are assumed to be identical in meaning and extent with the operations of the same name in common Arithmetic". [Peacock 1842–5, II, 1] Symbolical algebra, on the other hand, was a far more abstract system "which regards the combinations of signs and symbols *only*, according to determinate laws, which are altogether independent of the specific values of the symbols themselves". [Peacock 1830, vi]

In this second system, the symbols could be not only "the general representatives of numbers, but of every species of quantity". [Peacock 1830, ix] Moreover, it was now possible to assume that operations such as subtraction or extracting square roots were possible in all cases, thus enabling negative and imaginary quantities to be perfectly legitimate within this system. The emphasis now, then, was not on what symbols such as $-a$ or $\sqrt{-1}$ actually meant but under what laws they operated. Thus, for Peacock, symbolical algebra was a subject based not on a generalisation of arithmetical concepts, but on a series of arbitrary assumptions envisaged as "a means of evading a difficulty which results from the application of arithmetical operations to general symbols". [Peacock 1830, ix] In short, given that the question posed was to legitimise negatives and imaginaries in algebra, Peacock had answered, not by attempting to clarify the meaning of such entities, but by redefining what was meant by algebra itself.

Yet, although Peacock's views and ideology were certainly different to what had gone before, it has been successfully argued that he was probably influenced by previous (unpublished) work by Charles Babbage [Dubbey 1977; 1978, 93–130], and that both Peacock and Babbage can be seen to be building on ideas previously laid down by Woodhouse [Becher 1980; Nagel 1935, 446]. In turn, Woodhouse can be said to owe an intellectual debt to the writings of George Berkeley, while Babbage was deeply affected by the philosophy of Dugald Stewart. [Pycior 1984, 434-38; Sherry 1991, 47–54] Indeed, looking back, one can even see the germ of a symbolic philosophy in Playfair's paper of 1778: "In algebra again every magnitude being denoted by an artificial symbol, to which it has no resemblance, is liable, on some occasions, to be neglected, while the symbol may become the sole object of attention." [Playfair 1778, 319]

In his separation of algebra into two distinct parts, Peacock's approach also displays the influence of Buée's paper of 1806. Indeed, by virtue of his earlier division of algebra into "universal arithmetic" and a more general "mathematical language", Buée was later recognised as being "though in an imperfect manner, ... the first formal maintainer of that exposition which remove[d] the long standing difficulty [of negative and imaginary numbers]". [De Morgan 1842a, 151] But Buée's work, flawed though it undoubtedly was, exercised influence not only on the innovative work of Peacock with regard to algebra, but also on a different attempt to justify imaginary numbers, this time by means of geometry.

5 Warren and the geometrical approach

The earliest hint at the possibility of a two-dimensional representation of complex numbers was in John Wallis's *Treatise of Algebra* of 1685. There, he had demonstrated a procedure for determining the real coefficients of the complex roots of quadratic equations, by means of two perpendicular axes, one for the reals and one for the imaginaries — although this latter was not necessarily the y-axis. [Wallis 1685, 266–73] However, while correct, Wallis's method, and others formulated during the 18th century, failed either to exert any influence on further developments or to persuade the mathematical community of the validity of complex numbers.

Thus, despite its great merit, when the Norwegian Caspar Wessel (1745–1818) published his paper "On the analytic representation of direction" in 1799, it received virtually no attention until its republication nearly a hundred years later. A similar fate befell the work of Jean-Robert Argand. While his *Essai sur une manière de représenter les quantités imaginaires dans les constructions géométriques* (1806) did provoke some discussion on the subject in the French journal *Annales des mathématiques* around 1813–15, its immediate effect was otherwise entirely negligible.[8]

Not surprisingly, therefore, neither Wessel's paper nor Argand's *Essai* had any influence on the debate in Britain. Indeed, it is almost certain that none but a tiny fraction of British mathematicians were aware of the existence of either work, and still fewer had actually read them.[9] In Britain, it was Buée's paper (coincidentally published in the same year as Argand's work) which appears to have been the first to offer a new approach to the geometrical representation of complex numbers. Like Argand, Buée interpreted the symbol $\sqrt{-1}$ as representing perpendicularity in geometry. Thus passing from a point x on the real axis to a point $-x$ on the same line could be achieved by rotating the point through a right angle and then repeating the process, which would be equivalent to multiplying x by $\sqrt{-1}$ and then again by $\sqrt{-1}$, to obtain $-x$.

However, as has been mentioned, the clarity of the explanation and the rigor of some of Buée's arguments left a good deal to be desired, being accompanied by much that was unintelligible or simply wrong, such as a "proof" that $(\sqrt{-1})^n = n\sqrt{-1}$. [Buée 1806, 67–71] Indeed, in the preface to his *Treatise*, Peacock pointed out that "even in those cases in which his conclusions were correct, his reasonings were insufficient to establish them". Nevertheless, he still acknowledged the influence of the paper "as having first directed my attention to this subject". [Peacock 1830, xxvii]

It would appear, however, that Buée's influence did not extend much further, since the next significant work to be published on the subject made no reference to him (or indeed any other previous writers on the subject) at all. This work, *A treatise on the geometrical representation of the square roots of negative quantities* (1828) was written by John Warren (1796–1852), yet another Cambridge mathematician, this time from Jesus College.[10] The

[8]See the paper by Gert Schubring in this volume.

[9]De Morgan listed Argand's *Essai* among the works on this topic "which I have either not seen, or cannot immediately obtain" [De Morgan 1849, vi], while Hamilton admitted that he knew of it "only by Dr. Peacock's mention of it in his Report [of 1833]" [Hamilton 1853, 31]

[10]Warren's explanation for the absence of any citation to Buée was that he was "not

book was original in many ways, not least in its notation. Warren expressed a line of length a, inclined to the real line at an angle A ($< 360°$), as $\genfrac{}{}{0pt}{}{a}{0}$. More generally, $\genfrac{}{}{0pt}{}{a}{p}$ denoted a line of length a inclined at an angle of $A + p.360°$. So that, "if $(\genfrac{}{}{0pt}{}{a}{p})^{m/n} = b$, b is inclined to unity at an angle $= \frac{m}{n}(A + p.360°)$." [Warren 1828, 29]

Using this convention, Warren was able to prove that both roots of -1 represented anticlockwise rotations of 90° and 270°, respectively:

$$\sqrt[2]{-1} \text{ has two values, viz. } \binom{-1}{0}^{1/2} \text{ and } \binom{-1}{1}^{1/2};$$

$$\begin{array}{llcl}
\text{Now} & -1 \text{ is inclined to unity at an angle} & = & 180°, \\
\therefore & \binom{-1}{0}^{1/2} \ldots\ldots\ldots\ldots & = & \frac{1}{2}(180°) = 90° \\
\text{and} & \binom{-1}{1}^{1/2} \ldots\ldots\ldots\ldots & = & \frac{1}{2}(180° + 360°) = 270°
\end{array}$$

$$\begin{array}{ll}
\text{[Hence] if} & \binom{-1}{0}^{1/2} \text{ be represented by } +\sqrt{-1}, \\
& \binom{-1}{1}^{1/2} \text{ will be represented by } -\sqrt{-1}.
\end{array}$$

[Warren 1828, 59–60]

Taking this concept further still, he demonstrated that if $c = e\binom{1}{1}^n$, for $e > 0$ and $0 < n < 1$, then when $n < \frac{1}{4}$, c would be of the form $+a + b\sqrt{-1}$; similarly, for $\frac{1}{4} < n < \frac{1}{2}$, $c = -a + b\sqrt{-1}$, and so on. [Warren 1828, 64] Warren's *Treatise* thus gave the most comprehensive rationale to date (in English) for the geometrical representation of complex numbers; and while there were omissions,[11] it was praised for having "completely succeeded in giving an interpretation of the roots of unity, when attached to symbols which denote lines in Geometry". [Peacock 1830, xxvii]

The work of John Warren, though virtually unheard of outside Britain, and largely forgotten within twenty years inside, was otherwise significant in two respects. Firstly, it would later influence Hamilton in his research on algebraic triples and hence provide a starting point from which the theory of quaternions would develop. Secondly, it marks the genesis of a change in

aware of the existence of M. Buée's paper till November 1827, when my treatise was in the press". [Warren 1829a, 251]

[11]The *Treatise* contained no interpretation of numbers of the form $(a + b\sqrt{-1})^{m+n\sqrt{-1}}$, an oversight not realised by Warren until he read C.V. Mourey's *La vraie théorie des quantités négatives et des quantités prétendues imaginaires* (Paris, 1828).

attitude amongst British mathematicians — an attitude completely shared by Peacock — in which the justification of complex numbers is only part of a broader framework which is more concerned with how mathematics can be extended once their use is permitted.

Indeed, if anything, Warren's work was less an attempt to justify "impossible" numbers than a bid to demonstrate their geometric utility. In fact he rarely used the word "impossible" to describe complex numbers, since "by the word 'impossible' no impossibility is necessarily implied, but on the contrary, ... the quantities called impossible have a real existence, and are capable of geometric representation". [Warren 1829b, 340] This progressive attitude characterised much of British work on complex numbers from this point onwards. In British eyes, thanks to Warren and Peacock, the veracity of their use in both algebra and geometry was now established.[12] The question now was to discover in what ways the subject could be extended. The first area considered was that of logarithms.

6 A new logarithmic controversy

As was seen in §2, Euler had successfully solved the contentious problem of imaginary logarithms in 1749. This had shown that the general logarithm is dependent on an arbitrary non-negative integer. However, the late 1820s and early 1830s saw a minor reprise of the controversy in Britain. It began with research undertaken by a young Irish mathematician by the name of John Thomas Graves (1806–1870). A lawyer by profession, Graves had studied mathematics at Trinity College, Dublin, and while there had formed a strong and lasting friendship with fellow student William Rowan Hamilton (1805–1865). For the next forty years, their regular correspondence would act as a mutual source of stimulation for each other's mathematical work.

In 1826, Graves obtained a result that in the most general case of a logarithm of a complex number taken to a complex base, two integers are necessary

[12]Not all British mathematicians were immediately convinced, however. Taking the example of Augustus De Morgan, Helena Pycior points out that he "seems to have fully recognised only in the mid-1830s that the geometrical representation of the complex numbers made a sound basis for legitimation of the imaginaries and justified the symbolical approach itself". [Pycior 1983, 220] For a discussion of the reservations expressed by De Morgan, Frend, William Whewell and others with regard to symbolic algebra, see Pycior 1982.

to ascertain the solution. After lengthy consultation with Hamilton, Graves submitted the result to the Royal Society for publication in their *Philosophical Transactions* in 1828, but the referees (who included George Peacock) remained unconvinced by the proof. The problem was that Graves' argument was long, circuitous and very difficult to follow. Had it not been for the intervention of Hamilton on his friend's behalf, the paper would certainly have been rejected. [Hankins 1980, 262] Thanks to his persuasion, it finally appeared in 1829 under the title "An attempt to rectify the inaccuracy of some logarithmic formulæ".

The crux of Graves' argument ran as follows: Starting from the premises

$$f(\theta) = \cos\theta + \sqrt{-1}\sin\theta \tag{1}$$
$$f(\theta) = 1 + \sqrt{-1}.\theta + \cdots + \tfrac{(\sqrt{-1}.\theta)^n}{1.2\ldots n} + \cdots \tag{2}$$
$$f(x\theta) = \text{ a value of } \{f(\theta)\}^x \tag{3}$$
$$f^{-1}f(\theta) = 2n\pi + \theta \tag{4}$$
$$f\{x(2n\pi+\theta)\} = \{f(\theta)\}^x, \text{ where } n \text{ is an integer,}^{13} \tag{5}$$

he let $y = a^x$ and

$$a = f(\theta). \tag{6}$$

Therefore, by (5),

$$a^x = \{f(\theta)\}^x = f\{x(2n\pi+\theta)\}.$$

But, by (4) and (6),

$$2n\pi + \theta = f^{-1}f(\theta) = f^{-1}(a).$$

Therefore

$$y = a^x = f(xf^{-1}(a)). \tag{7}$$

Re-arranging gave

$$\log_a y = x = \frac{f^{-1}(y)}{f^{-1}(a)}.^{14} \tag{8}$$

[13] Graves credits premises (1) and (2) to Euler, (3) and (4) to de Moivre, and (5) to Louis Poinsot (1777–1859), although he admits that his knowledge of the latter's research "is derived not from the original Essays, but from abstracts of their contents given in the *Dublin Philosophical Journal*". [Graves 1829, 183]

From (2),

$$f(-\sqrt{-1}) = 1 + 1 + \cdots + \frac{1}{1.2\ldots n} + \cdots = e.$$

Therefore, by (4),

$$f^{-1}(e) = 2n\pi - \sqrt{-1}.$$

So the logarithms of k^2 (positive) were given by

$$\log_e(k^2) = \frac{f^{-1}(k^2)}{2n\pi - \sqrt{-1}}. \tag{9}$$

Then, by a series of elaborate substitutions, Maclaurin expansions and further manipulation, Graves arrived at the expression

$$f^{-1}(k^2) = 2m\pi + 2\sqrt{-1}\left\{\frac{1-k^2}{1+k^2} + \cdots + \frac{1}{2n+1}\left(\frac{1-k^2}{1+k^2}\right)^{2n+1} + \cdots\right\},$$

where m is an integer, which equals $2m\pi$ when $k^2 = 1$. Thus, he said, by (9), if $k^2 = 1$, the logarithms of unity will be given by $\frac{2m\pi}{2n\pi - \sqrt{-1}}$.

The announcement of this result provoked a series of publications both supporting and opposing it. Overseas, work by the French and German mathematicians Alexandre Vincent and Martin Ohm arrived at the same conclusion.[15] At home, however, Peacock, in his Report to the British Association of 1833, continued to express his doubts about its validity [Peacock 1833, 266–7n; Cajori 1913, 178] Support came once again from Hamilton who, at the Association's meeting in Edinburgh the following year, defended his friend's theory, offering a proof which confirmed the expression Graves had obtained. [Hamilton 1834; Hankins 1980, 391–6]

Objections still lingered, however, although less about the final result than the methods Graves had used to obtain it. Doubts as to the soundness

[14]Note that, by (4), there are an infinite number of possible values here, since $f^{-1}(y) = f^{-1}f(xf^{-1}(a)) = 2n\pi + xf^{-1}(a)$. We must assume that Graves is considering the case where $n = 0$.

[15]Vincent, a professor at the Royal College at Reims, published results in 1832 which corroborated those obtained by Graves. [Vincent 1832] The results of Ohm (a professor at Berlin), which actually pre-dated those of Graves, were published in volume two of his *Versuch eines vollkommen consequenten Systems der Mathematik* in 1823; although British mathematicians only learned of them via the second edition. [Ohm 1829]

of the proof were expressed by Augustus De Morgan (1806–1871), professor of mathematics at University College London, in 1836. Pointing out that Graves had used the premise $f(\theta) = \cos\theta + \sqrt{-1}\sin\theta$ to prove that $\{f(\theta)\}^x = f(xf^{-1}(a))$, De Morgan observed that "this is equally true where $f(\theta) = e^{c\theta}$, c having any value whatever, as appears sufficiently if the common processes of Algebra be true". [De Morgan 1836a, 62n]

Graves responded by issuing an alternative (and much shorter) proof, this time designed "to show that 1 is *among* the values of $e^{\frac{2m\pi}{2n\pi-\sqrt{-1}}}$." [Graves 1836, 281] This time his premises were the standard Maclaurin expansions for $\cos x$ and $\sin x$, together with the assumptions that

$$\begin{aligned}&\left\{1+\frac{x}{1}+\frac{x^2}{1.2}+\frac{x^3}{1.2.3}+\cdots\right\}\times\left\{1+\frac{y}{1}+\frac{y^2}{1.2}+\frac{y^3}{1.2.3}+\cdots\right\}\\ = \;& 1+\frac{x+y}{1}+\frac{(x+y)^2}{1.2}+\frac{(x+y)^3}{1.2.3}+\cdots\end{aligned}$$

that

$$e^x = 1^x\left(1+\frac{x}{1}+\frac{x^2}{1.2}+\frac{x^3}{1.2.3}+\cdots\right)$$

and that

$$1^x = \cos(2\omega x\pi)+\sqrt{-1}\sin(2\omega x\pi), \quad \text{for } \omega = 0, \pm1, \pm2, \pm3, \ldots .$$

From these, it immediately followed that

$$e^x = \left\{1+\frac{\sqrt{-1}.2\omega x\pi}{1}+\frac{(\sqrt{-1}.2\omega x\pi)^2}{1.2}+\cdots\right\}\times\left\{1+\frac{x}{1}+\frac{x^2}{1.2}+\cdots\right\}.$$

Therefore

$$\begin{aligned}e^x = 1 \;&+\; \frac{(\sqrt{-1}.2\omega\pi+1)x}{1} + \frac{(\sqrt{-1}.2\omega\pi+1)^2x^2}{1.2}\\ &+\; \frac{(\sqrt{-1}.2\omega\pi+1)^3x^3}{1.2.3} + \cdots .\end{aligned}$$

Letting $x = \frac{2m\pi}{2n\pi-\sqrt{-1}}$ (where m and n are both integers) resulted in the expansion

$$\begin{aligned}e^{\frac{2m\pi}{2n\pi-\sqrt{-1}}} = 1 \;&+\; \left(\frac{2\omega\pi-\sqrt{-1}}{2n\pi-\sqrt{-1}}\right)\frac{\sqrt{-1}.2m\pi}{1}\\ &+\; \left(\frac{2\omega\pi-\sqrt{-1}}{2n\pi-\sqrt{-1}}\right)^2\frac{(\sqrt{-1}.2m\pi)^2}{1.2}+\cdots .\end{aligned}$$

So, when $\omega = n$,

$$\begin{aligned} e^{\frac{2m\pi}{2n\pi-\sqrt{-1}}} &= 1+\frac{\sqrt{-1}.2m\pi}{1}+\frac{(\sqrt{-1}.2m\pi)^2}{1.2}+\frac{(\sqrt{-1}.2m\pi)^3}{1.2.3}+\cdots \\ &= \cos(2m\pi)+\sqrt{-1}\sin(2m\pi) \\ &= 1. \end{aligned}$$

This appeared to satisfy De Morgan, who declared "I see no further objection to Mr. Graves's system, but quite the contrary ... the extension may be highly useful, and I am happy to bear testimony to the ingenuity which suggested it, and the talent with which it has been carried out".[16] [De Morgan 1836b, 253] This left Peacock to be convinced, a feat which was achieved in 1837 by a young Scottish mathematician Duncan Farquharson Gregory (1813–1844).[17] In the first edition of his *Treatise*, Peacock had asserted the existence of logarithmic values common to positive and negative numbers [Peacock 1830, 569–70]; but in a paper published in 1837, after providing a further proof of Graves' formula, the young Gregory went on to prove the fallacy in Peacock's argument. [Gregory 1837, 134–5] Peacock's second edition of 1845 duly contained an acknowledgement of his mistake and another proof of the Graves result. [Peacock 1842–5, 444–5]

Given this eventual acceptance of Graves' generalisation, it is not unnatural at this point to ask why it is rarely (if ever) used. After all, modern texts on complex function theory employ only the Eulerian definition of the logarithm of a complex number, and make no reference to any extended formulation. The reason is simple: not only was the generalised version unnecessarily complicated, but it was also superfluous — the standard Eulerian logarithmic formulæ are perfectly sufficient to solve exponential equations in complex numbers. In short, no real advantage could be gained from using Graves' system in preference to that of Euler.

Nevertheless, the extension of the theory of imaginary logarithms gives an indication of the renewed interest in Britain at this time in complex numbers, whose existence and legitimacy were now no longer in question. But this interest extended further; indeed "from the 1820s through the 1840s

[16]Graves and De Morgan were later colleagues at University College, when Graves served as professor of jurisprudence between 1839 and 1843.

[17]A great-great-grandson of the great 17th-century mathematician James Gregory (1638–1675).

the study of the foundations of algebra became almost a British monopoly". [Hankins 1980, 249] In this respect, as we shall see, the work of both Warren and Graves was significant not merely in its own right as innovative mathematical research, but also as a catalyst which would eventually lead to one of the most significant mathematical discoveries of the 19th century.

7 "New inexplicables": the search for triples

The inspiration (at least in part) behind Graves' work on logarithms had been the idea that there might be other imaginary numbers. Although Hamilton was sceptical that new imaginaries could be found in that area, his interest in complex numbers was aroused by reading Warren's *Treatise* in 1829 (at Graves' instigation). [Hankins 1980, 262] Together with the prompting of Graves, it was this work which suggested to him the possibility of extending the representation of complex numbers from a plane to three-dimensional space. As he later recalled, "To suggestions from that Treatise I gladly acknowledge myself to have been indebted". [Hamilton 1853, 31] Thus began the search for the new algebraic entity of "hypercomplex" numbers capable of three-dimensional geometrical representation.

Although involved in the same area of research as those so far mentioned (Peacock, Graves, De Morgan, Gregory), Hamilton's philosophy of the subject was entirely different.[18] As he said to Graves in 1835, "we belong to opposite poles in Algebra; since you, like Peacock, seem to consider Algebra as a 'System of Signs and of their combinations,' somewhat analogous to syllogisms expressed in letters; while I am never satisfied unless I think that I can look beyond or through the signs to the things signified". [Graves 1882–9, II, 143] For an opponent of Peacock's formal approach such as Hamilton, algebra was too significant to be reduced to the mere manipulation of arbitrary symbols. Thus, while agreeing that complex numbers were mathematically sound, he maintained that they had yet to be given *meaning.*

[18]Because of his shifting attitudes towards algebra, it has been argued that De Morgan's views on the foundations of the subject both differed from [Hankins 1980, 249–50], and moderately agreed with [Pycior 1983, 223–4; Nagel 1935, 461] those of Hamilton. Similarly, it has been shown that Hamilton's initial opposition to symbolical algebra softened in later years. [Koppelmann 1971, 226; Hankins 1980, 310–11]

Of course, for Peacock and his followers, one could be sure that the results obtained in symbolical algebra were both true and meaningful. If they had been derived by means of a consistent set of rules of combination and operation, then they could be viewed as correct within that system. However, in order that the results obtained were *meaningful*, Peacock imposed the arbitrary restriction that, the operations of symbolical algebra must obey the same fundamental rules as those of arithmetic, including what are now known as the commutative and distributive laws.[19] He called this criterion the "Principle of the Permanence of Equivalent Forms".[20] [Peacock 1830, 104, 1842–5, II, 59]

Hamilton's priorities were completely different. Whereas Peacock played down the meaning of the individual symbols, emphasising the rules under which they operated, Hamilton aspired to give meaning to the algebraic objects to be sure of obtaining interpretable results. Thus, his view of algebra was "a science of meaningful symbols governed by necessary principles stemming from intuition". [Pycior 1981, 41] In this approach, he showed the influence of the philosophy of Immanuel Kant, who, in his *Critique of Pure Reason* (1781), had stated that the intuitive notion of time served as the basis for both arithmetic and algebra. From this primitive intuition, Hamilton sought to define the concept of real numbers, and hence the foundations of a meaningful algebra.

He did this in a lengthy paper (written in sections between 1833 and 1835, and published in 1837) entitled the "Theory of conjugate functions, or algebraic couples; with a preliminary and elementary essay on algebra as the science of pure time". By basing number on the notion of steps in time from an arbitrary point of origin, Hamilton was able to easily define both positive and negative numbers, since a negative number value would simply

[19] The terms commutative and distributive were introduced by François-Joseph Servois (1767–1847) in 1814. The laws were used by Peacock in his *Treatise*, but it was Gregory who was the first to mention them by name in an English work. [Gregory 1838, 211] The associative law, although implicitly present in preceding works, was first stated by Hamilton in 1843. [Hamilton 1967, 114]

[20] Peacock's statement of this principle varied slightly in the two versions of his *Treatise*. The 1830 version reads as follows: "Whatever form is Algebraically equivalent to another, when expressed in general symbols, must be true, whatever those symbols denote. Whatever equivalent form is discoverable in arithmetical Algebra considered as the science of suggestion, when the symbols are general in their form, though specific in their value, will continue to be an equivalent form when the symbols are general in their nature as well as in their form." [Peacock 1830, 104]

represent a time-step in the opposite direction to that of a positive number. However, that still left the difficult problem of giving a meaningful definition of complex numbers.

Hamilton solved this by defining number *couples* (a, b), where a and b were two real numbers,[21] or moments in time. Defining operations such as

$$\begin{aligned} (a,b) \pm (x,y) &= (a \pm x, b \pm y), \\ (a,b).(x,y) &= (ax - by, ay + bx), \\ \frac{(a,b)}{(x,y)} &= \left(\frac{ax+by}{x^2+y^2}, \frac{bx-ay}{x^2+y^2}\right), \end{aligned}$$

he was able to show that, not only do they give consistent results, but that any real couple (a, b) is exactly equivalent to the complex number $a+b\sqrt{-1}$ in its mode of operation. Thus, by substituting these ordered couples of (meaningful) real numbers for complex expressions, the whole question of giving meaning to imaginary quantities was made redundant. But by the time Hamilton published his theory of number couples, the justification of imaginaries was no longer of importance; indeed "it was not clear that Hamilton had created anything more than a new representation of what was already known". [Hankins 1980, 275] British mathematicians now not only accepted complex numbers: they wanted to find more!

The endeavour now underway in Britain was to discover if there were number *triples* (a, b, c) which would do for three-dimensional geometry what Hamilton's couples and the standard complex numbers could do for the two-dimensional case. Clearly, such an extension of the theory of complex numbers would be a far more general and, most importantly, applicable algebraic subject. Yet, as De Morgan recognised, "an extension to geometry of three dimensions is not practicable until we can assign two symbols, Ω and ω, such that $a + b\Omega + c\omega = a_1 + b_1\Omega + c_1\omega$ gives $a = a_1, b = b_1$ and $c = c_1$: and no *definite* symbol of ordinary algebra will fulfil this condition." [De Morgan 1839, 177]

Once again, Hamilton's interest in triples was periodically maintained throughout the 1830s and 1840s by John Graves, who himself made sev-

[21] Although Gauss appears to have had the notion of couples as early as 1831, there is no evidence to suggest that Hamilton was aware of this. He seems to have obtained the idea from Cauchy's *Cours d'analyse* (1821), which had included the statement that "every imaginary equation is merely the symbolic representation of two equations between real quantities". [Cauchy 1882–1938, vol.2, 3]

eral investigations into representing lines in space by means of triples, and attempting to multiply them together. One of these systems, which Graves developed in 1836, was independently discovered ten years later by his younger brother Charles.[22] This gave a satisfactory geometrical interpretation of numbers of the form $a + bi + cj$ $(i \neq j, i^2 = j^2 = -1)$, but while most of the usual laws of algebra held, multiplication was not distributive. [Hamilton 1853, 37–9] Hence, for formalists such as the Graves brothers, this system was not regarded as satisfactory.

Meanwhile, De Morgan was also developing his own theories regarding triples and their geometrical representation. In a paper read to the Cambridge Philosophical Society in 1839, he hinted at a use for the cube roots of unity as geometrical operators: "in passing from x to $-x$ by two operations, we make use in ordinary algebra of one particular solution of $\phi^2 x = -x$, namely $\phi x = \sqrt{-1}.x$. An extension to three dimensions would require a solution of the equation $\phi^3 x = -x$, containing an arbitrary constant, and leading to a function of triple value, totally unknown at present." [De Morgan 1839, 177]

Although this theory was never fully developed, one of the by-products of De Morgan's paper was that it prompted Hamilton to re-embark on the quest for a sound system of triples. [Hamilton 1853, 41] Since the early 1830s, he had formulated various triple systems, which for various reasons had failed to produce unequivocal results regarding multiplication. In particular, they failed to obey the rule that the product of two non-zero numbers must also be non-zero — known as the product law. His latest attempt was to prove more fruitful, although, since there already exist a number of excellent accounts of the eventual outcome of this research, [e.g. van der Waerden 1985, 179–83; Hankins 1980, 293–300] it is only necessary to give a basic outline of the main details. The first problem came from multiplying two triples together. Hamilton obtained

$$\begin{aligned} & (a + bi + cj)(x + yi + zj) \\ = \quad & (ax - by - cz) + (ay + bx)i + (az + cx)j + (bz + cy)ij. \end{aligned}$$

This was obviously not a triple, unless the ij-term could somehow be accounted for. Also, the *norm* of $a+bi+cj$ (obtained by multiplying $a+bi+cj$

[22] Charles Graves (1812–1899), professor of mathematics at the University of Dublin, and later Bishop of Limerick.

by $a - bi - cj$) was expected to be $a^2 + b^2 + c^2$ since, by analogy to ordinary complex numbers, multiplying complex conjugates $a + bi$ and $a - bi$ gave $a^2 + b^2$. But in fact it was $a^2 + b^2 + c^2 - 2ijbc$. Attempts involving $ij = 1$, $ij = -1$ and $ij = 0$ all failed to produce satisfactory results, until he let $ij = -ji$ and defined this as a new unknown: "I made therefore $ij = k, ji = -k$, reserving to myself to inquire whether k was $= 0$ or not". [Hamilton 1967, 107]

Then, on 16 October 1843, he had a sudden idea of what this k could be: "there dawned on me the notion that we must admit, in some sense, a *fourth dimension* of space for the purpose of calculating with triplets; or, transferring the paradox to algebra, must admit a *third* distinct imaginary symbol k, not to be confounded with either i or j, but equal to the product of the first as multiplier, the second as multiplicand, and therefore I was led to introduce *quaternions* such as $a + ib + jc + kd$, or (a, b, c, d)." [Hamilton 1967, 108] By *defining* k to be a third imaginary quantity, so that $k^2 = -1$, $ik = -ki$ and $jk = -kj$, he was now able to quickly deduce the fundamental formulae of his new algebra:

$$i^2 = j^2 = k^2 = -1, \quad ij = -ji = k, \quad jk = -kj = i, \quad ki = -ik = j.$$

It thus immediately followed that the norm of a quaternion was

$$\begin{aligned}(a + bi + cj + dk)(a - bi - cj - dk) &= a^2 + b^2 + c^2 + d^2(-k^2) \\ &\quad - bd(ik + ki) - cd(jk + kj) \\ &= a^2 + b^2 + c^2 + d^2,\end{aligned}$$

and that the product of any two quaternions $(a + bi + cj + dk)$ and $(w + xi + yj + zk)$ would also yield another quaternion:

$$\begin{aligned}&(aw - bx - cy - dz) + (ax + bw + cz - dy)i \\ &+ (ay - bz + cw + dx)j + (az + by - cx + dw)k.\end{aligned}$$

Hamilton's quaternions had arisen from his search for a coherent system of number triples capable of representing lines in three-dimensional space. The unexpected inclusion of a fourth term, while ensuring a consistent algebra, made the geometrical interpretation of quaternions somewhat harder. While the imaginary (or "vector") parts i, j and k could be said to represent three perpendicular lines, Hamilton interpreted the real part as representing "*extra-spacial*, or simply SCALAR direction",[23] [Hamilton 1967, 359] in other words, direction on a uni-dimensional line, or a concept independent of space, such as time.

The "discovery" of quaternions came as a surprise to Hamilton, but more so to his British contemporaries. This was for two main reasons. Firstly, since the products of two distinct imaginaries depended on the order in which they were multiplied, the commutative law for multiplication was violated. This meant that he had created the first fully consistent generalised number system in which the laws of arithmetic no longer held in their entirety. Thus Peacock's Principle of the Permanence of Equivalent Forms had been proved to be no obstacle to the formation of a meaningful system of symbolic algebra.

The second reason concerned Hamilton's "creation" of new algebraic objects; this was the first time that mathematical entities had been deliberately constructed, as opposed to being discovered or evolving from pre-existing concepts. As John Graves said: "There is still something in the system which gravels me. I have not yet any clear view as to the extent to which we are at liberty arbitrarily to create imaginaries, and to endow them with supernatural properties." [Graves 1882–9, II, 443] De Morgan was less surprised than Graves at the origin of new imaginary quantities; indeed, in a paper completed just nine days before Hamilton's discovery, he had expressed the opinion that "new inexplicables might, and perhaps would arise" before long. [De Morgan 1843, 142] Yet even he was taken aback at the idea of "*imagining* imaginaries". [Graves 1882–9, II, 475]

Hamilton's work on complex numbers had initially begun as an attempt to place them on a sound intuitive foundation but had quickly developed into a search for higher order concepts. It eventually resulted in the discovery/creation of a new and surprising species of number: one that was made up of four distinct components and one whose laws of combination did not permit one of the most basic ideas in elementary algebra. It is somewhat ironic in fact that for a mathematician whose chief priority was to base the

[23]Notice the first use of the word *scalar*. Although Hamilton also coined the term *vector* (in the modern sense) to denote the directed line segment represented by the imaginary quaternion component, the phrase "radius vector" had been in use for over a century, and was still employed at this time [e.g. Warren 1828, 137–8; De Morgan 1839, 186].

concept of number on intuition, Hamilton was responsible for one of the most counter-intuitive algebraic creations in the history of mathematics.

8 After quaternions

The discovery of quaternions heralded a new era in the history of complex and imaginary numbers. Not only were they now widely accepted by British mathematicians, but it was also realised that further imaginaries were possible, and that meaningful and consistent systems of hypercomplex numbers were entirely conceivable within the framework of symbolic algebra. The realisation that mathematicians were at liberty to create new algebras by *inventing* algebraic elements and defining legitimate operations upon them was not lost on those who had previously adhered to Peacock's permanence principle.

Among the most active of these were John and Charles Graves and Augustus De Morgan. Although for Hamilton, the question of three-dimensional geometrical representation had been answered by quaternions, these three continued to try to establish a workable system of triples by exploiting the new freedom. In a paper written in October 1844, one year after Hamilton's discovery, De Morgan gave his most extensive treatment to date of the subject by constructing five distinct systems of triples. While distinguishing his triple systems from Hamilton's quaternion one,[24] De Morgan credited Hamilton's work "for the idea of inventing a distinct system of unit-symbols, and investigating or assigning relations which define their mode of action on each other". [De Morgan 1844, 241]

Unlike Hamilton, De Morgan was not dissuaded by a lack of symmetry in his algebras. He wrote: "Sir William Hamilton seems to have passed over triple Algebra altogether on the supposition that the modulus,[25] if any, of $a\xi+b\eta+c\zeta$ [where $\xi^2=\xi$, $\eta^2=\zeta^2=-\xi$] must be $\sqrt{a^2+b^2+c^2}$ ... but it is

[24] In an addendum, De Morgan added that "having since I read this paper in proof, examined Sir W. Hamilton's system of quaternions, I may state that, in my view of the subject, it is not *quadruple*, but *triple*, since every symbol is explicable by a line drawn in space". [De Morgan 1844, 254]

[25] i.e. the square root of the norm. The "law of the moduli" holds if the norm of the product equals the product of the norms, i.e. $(a_1^2+a_2^2+\cdots+a_n^2)(b_1^2+b_2^2+\cdots+b_n^2)=c_1^2+c_2^2+\cdots+c_n^2$. In 1898, Adolf Hurwitz (1859–1919) proved that this law could only hold for $n=1,2,4$ or 8. [van der Waerden 1985, 185]

by no means requisite that the modulus should be a symmetrical function of a, b, and c." [De Morgan 1844, 242] He then went on to illustrate this belief with a series of various triple systems, all of which had extremely irregular moduli. But this drawback was not the only "imperfection" in his systems. Commenting on a case in which $P = a + b\eta + c\zeta$, where $\eta^2 = \zeta^2 = \eta\zeta = -1$, he remarked "I did not at first see that though this will give $PP' = P'P$, it will not give $P''(P'P) = (P''P')P$, except in particular cases". [De Morgan 1844, 249]

Yet while the abandonment of symmetrical moduli and the associative law of multiplication in De Morgan's systems was apparently no more imperfect than Hamilton's disregard for the commutative law in his, the former remained fundamentally flawed due to their failure to adhere to the product law, a defect which was not present in the latter. The same applied to the various attempts by the Graves brothers, who constructed similar systems in the months following the birth of quaternions [e.g. Graves 1847].

But the search for new complex systems was not limited to triples. In fact, it was in a higher dimension that the first successful complex number system after quaternions was discovered. As early as December 1843, John Graves constructed a consistent algebra of complex numbers with eight components which obeyed the law of the moduli. [Young 1848, 338–41; Hamilton 1967, 648–56] This system of *octaves* or *octonions* consisted of the real element 1, and seven imaginaries, $i_1, i_2, i_3, i_4, i_5, i_6, i_7$, obeying the following rules of multiplication:

$$
\begin{aligned}
&i_1^2 = i_2^2 = i_3^2 = i_4^2 = i_5^2 = i_6^2 = i_7^2 = -1, \\
&i_1 = i_2 i_3 = i_4 i_5 = i_7 i_6 = -i_3 i_2 = -i_5 i_4 = -i_6 i_7, \\
&i_2 = i_3 i_1 = i_4 i_6 = i_5 i_7 = -i_1 i_3 = -i_6 i_4 = -i_7 i_5, \\
&i_3 = i_1 i_2 = i_4 i_7 = i_6 i_5 = -i_2 i_1 = -i_7 i_4 = -i_5 i_6, \\
&i_4 = i_5 i_1 = i_6 i_2 = i_7 i_3 = -i_1 i_5 = -i_2 i_6 = -i_3 i_7, \\
&i_5 = i_1 i_4 = i_7 i_2 = i_3 i_6 = -i_4 i_1 = -i_2 i_7 = -i_6 i_3, \\
&i_6 = i_2 i_4 = i_1 i_7 = i_5 i_3 = -i_4 i_2 = -i_7 i_1 = -i_3 i_5, \\
&i_7 = i_6 i_1 = i_2 i_5 = i_3 i_4 = -i_1 i_6 = -i_5 i_2 = -i_4 i_3.
\end{aligned}
$$

This system was also independently created in one of the earliest papers by the young Cambridge mathematician Arthur Cayley (1821–1895). [Cayley 1845] In a subsequent article, published two years later, Cayley observed that "if $i_\alpha, i_\beta, i_\gamma$ be any three of the seven [imaginary] quantities which do *not* form a triplet, then $(i_\alpha i_\beta).i_\gamma = -i_\alpha.(i_\beta i_\gamma)$. Thus, for instance, $(i_3 i_4).i_5 = -i_7.i_5 = -i_2$; but $i_3.(i_4 i_5) = i_3.i_1 = i_2 \ldots$ ". Graves' and Cayley's octaves[26] were therefore neither commutative nor associative, "which is still a wider departure from the laws of ordinary algebra than that which is presented by Sir W. Hamilton's quaternions". [Cayley 1847, 257–8]

From this point, a variety of new systems of hypernumbers began to emerge. Hamilton created a new system of *biquaternions* (quaternions with complex coefficients) in 1844, although he noticed that they failed to obey the product law. [Hamilton 1853, 650] A generation later, another young Cambridge-trained mathematician, William Kingdon Clifford (1845–1879), produced another form of number, which he also termed biquaternions, although his definition was very different from that of Hamilton.[27] [Clifford 1873] These satisfied the product law but not the associative. The introduction of "Clifford algebras" five years later gave a generalisation for systems with 2^n elements $1, i_1, i_2, \ldots, i_{2^n-1}$, where $i_a^2 = -1$ and $i_a i_b = -i_b i_a$ for $a \neq b$. [Clifford 1878] But Clifford, although British, had been more influenced by recent German work on hypernumbers, particularly that of Hermann Günther Grassmann (1809–1877), than by his contemporary countrymen.

This provides an indication that, by the middle of the century, research into higher-order complex numbers was no longer dominated by British mathematicians. Indeed it was to be elsewhere, especially in Germany, that the major advances in this area were achieved in the second half of the 19th century. This work culminated in Adolf Hurwitz's 1898 proof that the only linear associative algebras which satisfy the product law are the real numbers, complex numbers, real quaternions and Clifford's biquaternions. [Kline 1972, 793] Thus it was finally confirmed that a triple system of the kind originally envisaged by Hamilton, De Morgan and the Graves brothers was impossible.

While the latter half of the 19th century witnessed profound developments on the algebraic implications of quaternions, similar extensions were also being made to the geometrical side of the subject. Through the work of Scottish mathematicians Peter Guthrie Tait (1831–1901) and James Clerk

[26] Because Cayley was the first to publish his findings, octaves are known today as *Cayley numbers*.

[27] Clifford's biquaternions are of the form $p + \omega q$, where p and q are real quaternions, ω commutes with every real quaternion, and $\omega^2 = 0$ or $\omega^2 = 1$.

Maxwell (1831–1879), the three-dimensional properties of quaternions were adapted for use in physics. These ideas were developed by Oliver Heaviside (1850–1925) in England and Josiah Willard Gibbs (1839–1903) in the United States to found the subject of vector analysis, one of the most valuable tools in modern applied mathematics. [Crowe 1967] Thus, by the end of the century an algebraic system derived from imaginary numbers would be employed to model the real world.

In the space of one hundred years, complex numbers had gone from being concepts of uncertain meaning and dubious mathematical validity to universally recognised mathematical entities of acknowledged importance. As the basis of far-reaching extensions to both algebra and geometry, they acted as a catalyst for the vast developments in those fields in the latter part of the 19th century. But as far as British mathematicians of the mid-19th century were concerned, all this lay ahead. While the future of complex numbers in mathematics was secure, the outlook was still uncertain. But to those who advocated the more risk-averse approach which had characterised much of the British mathematics of the late-18th and early-19th centuries, Augustus De Morgan had the following words [Richards 1987, 28]: "The motto which I should adopt against a course which seems to me calculated to stop the progress of discovery would be contained in a word and a symbol — remember $\sqrt{-1}$."

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Bellavitis's Equipollences Calculus and his Theory of Complex Numbers

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Abstract

The aim of this contribution is to present a detailed analysis of Bellavitis's work. In particular, we will examine Bellavitis's theory about the geometrical nature of complex numbers and the connections with his equipollences calculus. Afterwards, we will examine some aspects of the resulting theory of algebraic equations.

Introduction

Although Bellavitis's analysis of the theory of complex numbers was of secondary importance compared to what other mathematicians had done a little before him on this subject, it was the geometrical representation of the complex numbers that led him to make the equipollences calculus. Moreover, during the first half of the XIX century, in Italy it was he who took most interest in the "nature" of complex numbers. The aim of the present contribution is to go through some essential features of the equipollences calculus and to look at some interesting links between this calculus and the theory of complex numbers. Synthetically, we can say that Bellavitis's goal was to carry out Lazare Carnot's program regarding the position calculus and hence to establish a parallelism between algebraic calculus and geometrical calculus in the wake of the barycentric calculus.

Giusto Bellavitis was born in 1803 at Bassano del Grappa, near Padua, where he studied. He worked as a clerk at the municipality of Bassano,

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until 1843, when he was called to teach at the Vicenza "liceo". Throughout this period he maintained close contact with the mathematical circle at the University of Padua. In 1845 Bellavitis became a full professor of descriptive geometry at Padua University. His colleagues included such interesting Italian mathematicians as Conti, Minich and Turazza. Moreover, Bellavitis later became rector and a senator. When he died in 1883 he left approximately two hundred works mainly on geometry, in particular the geometrical calculus (equipollences calculus) and its applications and on the theory of complex numbers.[1]

1 The sources which inspired Bellavitis

Bellavitis's intellectual itinerary was rather singular, but at the same time emblematic of a certain cultural atmosphere present in Italy during the first half of the XIX century. As a starting point for his research he took themes connected with Lazare Carnot's *Géométrie de Position*[2] (1803) (analogous, for instance, to J.F.Français[3]). However, he was also interested in the problems which were being discussed between the end of the XVIII and the first half of the XIX century regarding the geometrical representation of complex numbers. In 1847 he wrote:[4]

> The geometrical representation of the imaginary numbers, which had been almost completely forgotten when I drew the equipollences method from it some years ago, has now become the object of several essays in the *Philosophical Magazine.*

However, from a theoretical point of view, the equipollences calculus comes first, as this method justifies the theory of complex numbers. So we will begin with the equipollences calculus. In 1876[5] Bellavitis reviewed the works

[1]Regarding the analysis of Bellavitis's work we have already devoted some papers (see P.Freguglia [1991], [1994] and with G.Canepa [1991]) and a book (see P.Freguglia [1992]).

[2]The word "equipollence" is drawn from the *Géométrie de Position*, but with a different meaning, see p.83 of L.Carnot [1803].

[3]See J.F.Français [1813].

[4]See "Saggio sull'algebra degli immaginari", G.Bellavitis [1847], p.249, "La rappresentazione geometrica degli immaginari, che era quasi affatto dimenticata quando alcuni anni or sono io ne traevo il metodo delle equipollenze, forma ora oggetto di frequentissime comunicazioni nel *Philosophical Magazine*".

[5]See G.Bellavitis [1876] pp. 469–473.

of those mathematicians (Buée, Argand, Français, Mourey, Warren) who, before him, had analysed the "nature" of complex numbers. The author whom Bellavitis considered to have influenced him most was Buée[6], who in an essay published in *Philosophical Transactions* in 1806 had suggested a geometrical representation of the imaginary numbers which was afterwards used as a basis for Bellavitis's approach. Buée's fundamental idea is well summarised in this short passage drawn from his essay:[7]

> So $\sqrt{-1}$ is the sign of perpendicularity, whose characteristic is that every point belonging to the perpendicular is equally far from [two] points placed at the same distance from either side of the foot (of the perpendicular). The sign $\sqrt{-1}$ expresses all this and it is the only sign that does this.

It seems that for Buée the nature of $\sqrt{-1}$ (imaginary unit) is purely geometrical and it has no other meaning.

As a matter of fact the main concern for mathematicians such as Buée, Français, Argand, etc. was to find the "true" nature, the "true" essence of the imaginary numbers. Argand,[8] for instance, saw the imaginary unit as a geometrical mean between the real positive unit and the real negative unit, that is:

$$-1 : \sqrt{-1} = \sqrt{-1} : 1 \quad \text{or} \quad -1 : -\sqrt{-1} = -\sqrt{-1} : 1.$$

But we must not forget that the "true" theory of ratios was traditionally a geometrical theory.

The accepted view gave an epistemologically acknowledged foundation only to the positive real numbers, which were connected to Euclidean geometry

[6]See A. Buée [1806]. Bellavitis says: "In reality, it was while I was studying a geometrical representation of imaginary numbers, which Buée had proposed, that I had the first idea (1832) about the equipollences method [. . .]" ("Fu veramente considerando una rappresentazione geometrica degli immaginarii proposta da Buée che a me venne (1832) la prima idea del metodo delle equipollenze [. . .]"), see G.Bellavitis [1876], p.58. See also Schubring's paper in this volume.

[7]See A. Buée [1806] p.23, "Ainsi $\sqrt{-1}$ est le signe de perpendicularité, dont la propriété caractéristique est, que tous les points de la perpendiculaire sont également éloignés de points placés à égales distances, de part et d'autre de son pié. Le signe $\sqrt{-1}$ exprime tout cela, et il est le seul qui l'exprime".

[8]See J.R.Argand [1806] p.6.

(theory of quantities). Because of this none of the negative roots of algebraic equations were accepted because they disagreed with the synthetic Euclidean geometrical interpretation. In this respect, it is interesting to make a comparison with D'Alembert's article "Negatif" in the *Encyclopédie* and with his essay "Mémoire sur les quantités négatives", which is in the VIII book of his *Opuscules mathématiques*. These papers were well known to Lazare Carnot and also, indirectly, to Bellavitis. Now we turn to examine the equipollences method.

2 The equipollences calculus

In his principal work, written in 1854, Bellavitis motivated the theory as follows[9]:

> This method [of the equipollences] satisfies Carnot's wish to find an algorithm which represents at the same time the length and the position of the various parts of a figure; we therefore have, directly, elegant easy graphic solutions for geometrical problems. The equipollences method includes as particular cases the method of parallel or polar co-ordinates, the barycentric calculus etc. [...]. He who knows the principles of Carnot's *Géométrie de Position*, will easily follow me [...].

The equipollences calculus is a geometrical algebraic structure analogous to that of a vector space where the multiplication and division operations (between vectors) are defined. Bellavitis never considers the possibility of an axiomatic theory. His position is strongly heuristic. He starts from the definition of parallelism (23rd definition [ορος] of the Ist book of Euclid's *Elements*) and from the following principle (which in this theoretical attitude is not a theorem):

[*P.1*]: "two angles which have parallel sides are equal" (see Figure 1).

[9]See G.Bellavitis [1854] p.2, "Questo metodo [delle equipollenze] soddisfa un desiderio del Carnot di trovare un algoritmo che rappresenti nello stesso tempo e la grandezza e la posizione delle varie parti della figura; ne risultano quindi per via diretta eleganti e semplici soluzioni grafiche dei problemi geometrici. Il metodo delle equipollenze comprende come casi particolari i metodi delle coordinate parallele o polari, il calcolo baricentrico ecc. [...]. Chi sia abituato ai principi della *Géométrie de Position* di Carnot troverà facile seguirmi [...]".

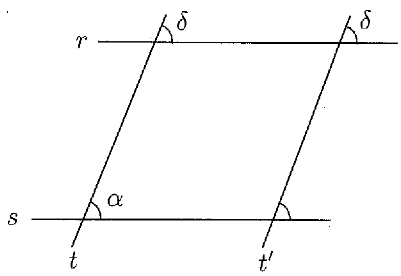

Figure 1:

Of course this principle is equivalent to Euclid's proposition (theorem) I,29: if $r \,||\, s$ and $t \,||\, t'$ then $\alpha = \delta$.[10] Thus for Bellavitis two oriented segments AB and CD are called *equipollent* if they have the same length and the same direction. We will denote this fact by writing as follows:

$$AB \simeq CD.$$

Instead of calling AB an "oriented segment", which is the term used by Bellavitis, we will often call AB a "vector applied in a point", which is a more modern mathematical term. Now in the plane, if a horizontal straight line is given, Bellavitis defined for an oriented segment AB the *inclination* operator as follows. Consider a point O and a fixed straight line OR; then the *inclination* operator can be defined by means of the relations (see Fig. 2):

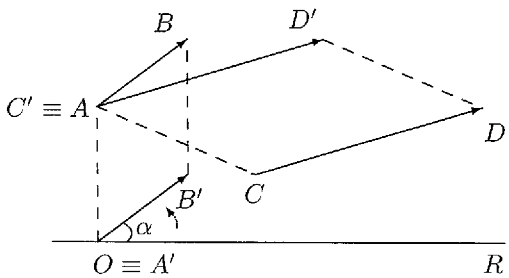

Figure 2:

$\mathrm{inc}\, AB = \mathrm{ang}\, B'A'R$ which means the angle $B'A'R$ oriented in an anti-clockwise direction. Here $A'B' \simeq AB$ and $A' \equiv O$. It follows that $\mathrm{inc}\, OR =$

[10] We must observe that Euclid's I,29 is logically equivalent to Euclid's Vth postulate.

0 and ang $BAD' =$ inc AB − inc CD. Bellavitis gives the other following fundamental principles:

[*P.2*] inc BA = inc $AB \pm 180°$

[*P.3*] (*the rule of parallelogram*) $AB + BC \simeq AC$

[*P.4*] $AB + 0 \simeq 0 + AB \simeq AB$ (existence of vector 0 which has length equal zero and no direction)

[*P.5*] $AB + BA \simeq 0$ or $AB \simeq -BA$.

From $AB + BC \simeq AC$ we have $AB + BC + CA \simeq 0$ or $AB \simeq AC - BC$.

Bellavitis also implicitly considers the composition + to be associative and commutative.

He gives this following definition:

$AB \simeq tCD$, where $t \in \mathbb{R}$, if AB and CD have the same direction. However the length and the orientation of tCD depend on t.

Moreover Bellavitis defines:

i) $AB \simeq BD \cdot EF$ if gr $AB = (\text{gr}\, CD) \cdot (\text{gr}\, EF)$ and inc AB = inc BD + inc EF, where gr AB means length of AB.

ij) $BD \simeq AB/EF$ if gr $BD = (\text{gr}\, AB)/(\text{gr}\, EF)$ and inc BD = inc AB − inc EF.

With regard to the orientation of AB, every ambiguity is solved by taking each oriented segment to its respective equipollent which has its origin in the point O of the fixed straight line. The operator inc — as we can easily observe — has the same formal behaviour as the operator log. Moreover, as is well known, the equipollences calculus is a geometrical plane calculus which presents some difficulties if we want to expand it to three dimensions. Bellavitis gave some canons which are helpful for facilitating the calculus. These canons (twelve in all) are in reality similar to lemmas (preliminary theorems). The fifth canon deals with the conjugate $A'B'$ of an oriented segment AB ($A'B' \simeq \text{cj}\, AB$) that is its mirror image in OR (see Fig.3).

Clearly $\text{gr}(\text{cj}\, AB) = \text{gr}\, AB = \text{gr}\, A'B'$, $\text{inc}(\text{cj}\, AB) = -\,\text{inc}\, AB = \text{inc}\, A'B'$.

cj is like a 180 degrees overturning operator which carries the semiplane ABC on to the semiplane $A'B'C'$. The notion of a *conjugate* of an oriented segment is, of course, suggested by the analogous notion relating to complex numbers. Bellavitis also examines the behaviour of the operator γ which turns an oriented segment AB by 90 degrees, that is:

$$\gamma AB \simeq A'B' \quad \text{if and only if } AB \text{ is perpendicular to } A'B'.$$

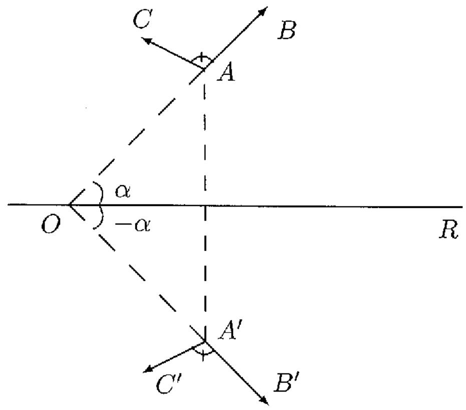

Figure 3:

The author says that γ behaves in calculus as $\sqrt{-1}$ (which he calls "ramuno", an abbreviation of "radice di meno uno", or in English "root of minus one"). Bellavitis states that all the elementary geometrical theorems (and also those relating to projective geometry, such as Desargues's theorem (plane case) about homological triangles) can be proved by the equipollences calculus. Bellavitis writes:[11]

> In elementary geometry the method of equipollences can be used [...] to deduce from a few easily demonstrable principles all the theorems which we use to teach, and many others besides [...].

To prove Desargues's theorem about homological triangles we need only the:

[Th. 2.1] Second canon. *If the terms of a binomial equipollence have different inclinations, then every term of it is zero.*

That is, let us suppose that $mIL \simeq nMN$ (binomial equipollence), where IL and MN are different from zero. We know that if $\mathrm{inc}\,IL \neq \mathrm{inc}\,MN$ then mIL and nMN are not equipollent. But we suppose $mIL \simeq nMN$, therefore $IL \simeq MN \simeq 0$.

The statement, which Bellavitis gives, regarding Desargues's theorem on homological triangles is the following: "If the vertexes of two triangles ABC

[11] See G.Bellavitis [1843] p.5, "Nella geometria elementare il metodo delle equipollenze può servire [...] a dedurre da pochissimi principi di facile dimostrazione tutti i teoremi che soglionsi insegnare, oltre molti altri [...]".

and $A'B'C'$ are in a straight line with the fixed point S, then the intersection points T, U, V of their corresponding sides are also in a straight line"[12] (see *Appendix* Fig.10 and ibid. §.A.1 for the proof). On the other hand, to prove Pythagoras's theorem (see *Appendix* §.A.2) we need[13] the next two canons. The third canon deals with the following situation:

$$LM + MN + NL \simeq 0.$$

[Th. 2.2] Third canon. *If two terms of a trinomial equipollence have equal inclinations, the third term (if we decide to transpose all three terms to only one member) will have an inclination which will differ 180 degrees from that of the other two and its length will be equal to the sum of the length of the first two terms.*

The proof of this canon is trivial.

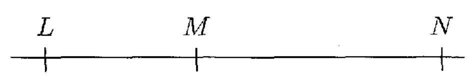

Figure 4:

[Th. 2.3] Fourth canon. *If we compare the terms of a trinomial equipollence with the terms of the identical equipollence:*

$$LM + MN + NL \simeq 0.$$

and we ascertain $\operatorname{inc} ML + \operatorname{inc} LN = 2 \operatorname{inc} MN$ *(and the three inclinations are unequal) then it follows that* $\operatorname{gr} LM = \operatorname{gr} NL$*, and vice versa: if* $\operatorname{gr} LM = \operatorname{gr} NL$ *then* $\operatorname{inc} ML + \operatorname{inc} LN = 2 \operatorname{inc} MN$.

Proof. In Fig. 5 let $ML \simeq L'N$ and $L'M + MN + NL' \simeq 0$ and $\operatorname{inc} ML = \operatorname{inc} L'N = \beta$, $\operatorname{inc} ML' = \delta$ and $\operatorname{inc} MN = \tau$. Let it be given that

(*hyp.*): $$\delta + \beta = 2\tau.$$

If we substitute $\tau = \alpha + \beta$, $\delta = \alpha + \beta + \alpha_1$ into the hypothesis we have

$$\beta + \alpha + \beta + \alpha_1 = 2(\alpha + \beta),$$

[12] See G.Bellavitis [1854] pp. 20–21.

[13] See G.Bellavitis [1854] pp.13 and foll.

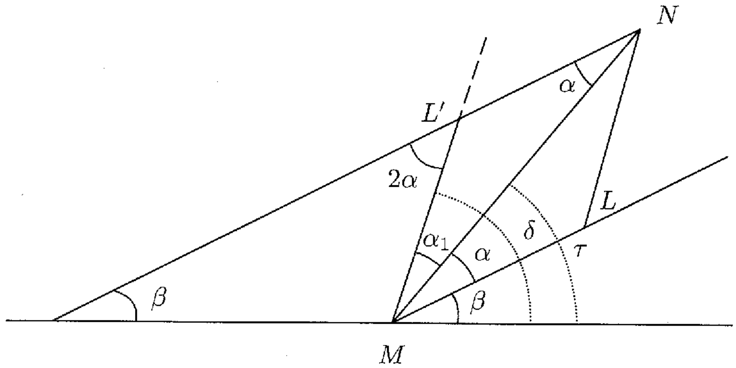

Figure 5:

so that $\alpha + \alpha_1 = 2\alpha$ that is $\alpha = \alpha_1$ and hence:

(*th.*): $$\mathrm{gr}\, ML' = \mathrm{gr}\, L'N.$$

Conversely, assume:

(*hyp.*): $$\mathrm{gr}\, ML' = \mathrm{gr}\, L'N$$

then $\alpha = \alpha_1$ so that $\delta = 2\alpha + \beta$. By adding β to the two members of the last expression we have:

$$\delta + \beta = 2\alpha + 2\beta$$

but $\tau = \alpha + \beta$ and hence:

(*th.*): $$\delta + \beta = 2\tau.$$ □

Epistemologically these demonstrations of theorems from elementary geometry using equipollences calculus offer a certain interest. It is a new way to see things and an anticipation of the modern ideas of geometrical algebra, where the main concept of vectorial space has a central role.[14] Indeed, with Bellavitis's help we have a calculus which governs all geometry and which enables us to find the solutions to geometrical problems.

[14]For instance see Emil Artin *Geometric Algebra* (1957), Interscience Publishers, Inc., USA, John Wiley and Sons, 3rd ed. 1964.

3 The theory of complex numbers

In "Saggio sull'algebra degli immaginari" Bellavitis asserts:[15]

> [...] the [geometrical] representation of imaginary quantities was often used by Analysts long before I deduced my method of equipollences from it; Cauchy also utilises it a lot, but always as a tool to express more clearly certain situations relating to imaginary quantities, but not for their sole definition: instead I am going to take it as the true definition and from it I deduce the properties of imaginary numbers.

Bellavitis clearly believed that the geometrical road was the main one to be followed in approaching the foundation of algebra. According to Bellavitis the algebra of real numbers also has its basis in the geometry of the straight line. Bellavitis considered the algebraic calculus to be a useful representative simplification. Thus by means of the equipollences calculus Bellavitis algebraized the complex numbers. First we must say that a complex number is represented by an oriented segment in a plane.[16] So addition and subtraction are operations like those between two oriented segments. Also for multiplication (and division) Bellavitis applies the equipollences calculus, but in this case for a better geometrical agreement he gave a geometrical construction where the length of the resulting oriented segment is graphically determined.[17] This construction is completely analogous to the one given by Argand.[18]

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[15]See G.Bellavitis [1847] p.247, "[...] la rappresentazione [geometrica] delle quantità immaginarie, fu data da parecchi Analisti molto prima che io ne deducessi il mio metodo delle equipollenze; anche Cauchy la adopera non rade volte, ma sempre come un mezzo di esprimere più chiaramente qualche circostanza relativa alle quantità immaginarie, non già come l'essenziale ed unica definizione delle medesime: io invece la prenderò come la vera definizione e da essa dedurrò le proprietà degli immaginari".

[16]Bellavitis gives also the trigonometrical representation of a complex number (see G.Bellavitis [1847] p.260). He writes:

$$z\surd^{u} = OM = OP + OQ = z\cos u + z\sin u \surd,$$

where $\surd = i$ and $u \in \mathbb{Q}$ is the number (whole or fractional) of right angles $(\pi/2)$. Obviously, $\surd^{u} = \cos u + \surd \sin u$, for instance when $u = 1$ we have: $\surd^{1} = \cos(\pi/2) + \surd\sin(\pi/2) = \surd$.

[17]Of course the subtraction and the division are deduced respectively from the addition and the multiplication.

[18]See J.R.Argand [1806] p.21.

The powers are iterations of the product. For the cases of the square and of the cube Bellavitis gave an explicit geometrical construction.

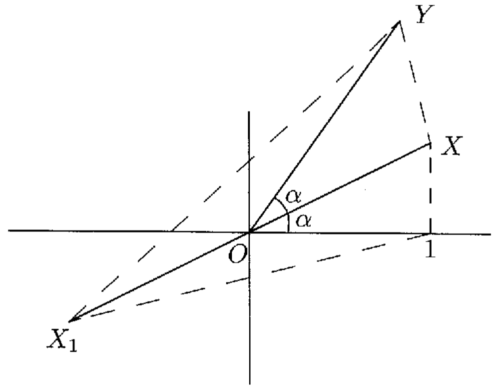

Figure 6:

— *Square case*: where $OX_1 \doteq -OX$; $OX_2 \doteq OY$ means $\mathrm{gr}\, OY = (\mathrm{gr}\, OX)^2$, $\mathrm{inc}\, OY = 2\,\mathrm{inc}\, OX \pm 2k\pi$.[19] The triangle OXY is made similar to triangle $O1X$ and it is possible graphically to determine the point Y. We have (for the lengths):

$$O1 : OX = OX : OY \quad \text{with } O1 = 1 \tag{1}$$

hence:

$$\mathrm{gr}\, OY = (\mathrm{gr}\, OX)^2$$

Moreover, if we pose $OX_1 \doteq -OX$, that is $\mathrm{gr}\, OX_1 = \mathrm{gr}\, OX$ (and $\mathrm{ang}\, X_1O1 = \mathrm{ang}\, X_1OY$) we have from (1):

$$O1 : OX_1 = OX_1 : OY.$$

Hence $\sqrt{OY} \doteq OX, OX_1$ or $\sqrt{OY} \doteq \pm OX$.

As we can see, Bellavitis was concerned to find the lengths of oriented segments (the moduli of complex numbers) by methods of Euclidean synthetic geometry. The constructive geometrical features are prominent in Bellavitis's work.

[19]The behaviour of values of k is according to De Moivre's formula.

Finally we want to discuss what Bellavitis calls the *fundamental theorem* (or *fundamental canon*) for the equipollences calculus. This canon — as we will see — plays an essential role in the theory of complex numbers. It is not difficult to demonstrate this theorem.

[Th. 3.1]. *"In the equipollences [...] we can do all operations and all calculations which would be lawful in the case of equations, and the resulting equipollences are always correct".*[20]

Besides we have the following corollary:[21]

[Th. 3.2]. *"If any relation among the distances of points of a straight line is found and expressed by an equation, we also have a corresponding relation among the points of a plane which transforms the equation into an equipollence".*

For example (see Fig.7), for any position of A, B, C, D and E on a straight line, where $EB = CD$, we have:

$$AE \cdot BC = AB \cdot BD - AC \cdot CD. \tag{2}$$

If we put $AE = x$, $EB = CD = a$, $BC = y$ and we substitute it into (2) then we have the identity:

$$x \cdot y = (x + a)(y + a) - (x + y + a)a.$$

In the plane this corresponds to the following equipollence (3):

$$AE \cdot BC \doteq AB \cdot BD - AC \cdot CD \tag{3}$$

(see Fig.7).

Bellavitis's interpretation of these theorems is that the algebraic properties of the complex numbers (oriented segments in a plane) and those relating to the real numbers (oriented segments in a straight line) are the same.

[20] See G.Bellavitis [1876]. The results were already found in 1832 (see G.Bellavitis [1832]).

[21] See G.Bellavitis [1876] p.455, "[...] trovata tra le distanze dei punti di una retta una relazione qualunque espressa in equazione, si ha una relazione tra i punti di un piano mutando l'equazione in equipollenza".

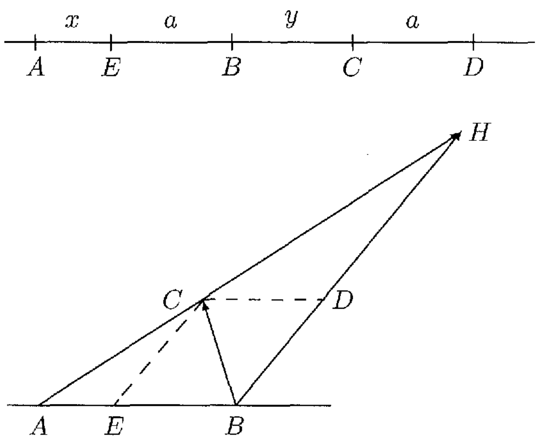

Figure 7:

4 Algebraic equations

In § II (entitled "About algebraic equations") of his essay on the algebra of imaginary numbers (1847), Bellavitis examined some applications of his theory of complex numbers to the theory of algebraic equations. His treatment is based on geometrical constructions, taking the general equation of fourth degree as an example. Afterwards he studies the numerical solutions of equations. But Bellavitis's essential and partially original contribution concerns the following. He states:[22]

> For any value of the unknown X we will build the polynomial $Y = D + CX + BX^2 + AX^3 + X^4$ by drawing the DC_1 equal and parallel to the straight line [oriented segment] which is expressed by the product CX; similarly, C_1B_1, B_1A_1, A_1Y are equal and parallel to those given by the other monomials BX^2, AX^3, X^4. (The polygon $ODC_1B_1A_1Y$ of the figure [see Fig.8] corresponds to the case where $X = 1$, so that

[22]See G.Bellavitis [1847] p.265, "Per un qualunque valore dell'incognita X si costruirà il polinomio $Y = D + CX + BX^2 + AX^3 + X^4$ tirando la DC_1 eguale e parallela alla retta espressa dal prodotto CX; similmente le C_1B_1, B_1A_1, A_1Y egauali e parallele a quelle espresse dagli altri termini BX^2, AX^3, X^4. (Il poligono $ODC_1B_1A_1Y$ della figura [see Fig.8] corrisponde al caso $X = 1$, sicchè $DC_1 \doteq OC, C_1B \doteq OB, B_1A_1 \doteq OA, A_1Y \doteq O1$, esprimendo con il segno $\doteq$ la condizione di due rette di essere uguali, parallele e dirette per lo stesso verso). Al variare di X variano anche i punti C_1, B_1, A_1, Y; e se X è una radice dell'equazione il punto Y cade in O".

$DC_1 \doteq OC$, $C_1B \doteq OB$, $B_1A_1 \doteq OA$, $A_1Y \doteq O1$ where the sign $\doteq$ expresses the condition that two straight lines [oriented segments] are equal, parallel and going in the same direction). When X changes value C_1, B_1, A_1, Y also change values; and if X is a root of the equation then the point Y coincides with O.

First he says that it is necessary to distinguish a formal algebraic equation, for instance

$$x^4 + ax^3 + bx^2 + cx + d = 0 \quad [\text{with } a, b, c, d, x \in \mathbb{C}]$$

from the corresponding equipollence. The former is a purely syntactic expression, conceived as a combination of symbols which obey the laws of algebra (*logistica speciosa*, according to Viète). The latter, on the other hand, is an algebraic-geometrical expression, for instance:

$$X^4 + AX^3 + BX^2 + CX + D \doteq 0$$

where A, B, C, D, X are oriented segments (vectors).

Bellavitis's treatment of the various theorems is a little obscure. Thus we will try to reconstruct the essential meaning of Bellavitis's ideas by giving them philological attention. We think that Bellavitis, who always had a great respect for the mathematical tradition, related his construction to the theme of the geometrical construction of algebraic equations as it was studied by the algebraists of the XVI century (and the XVII century) and differently by Descartes.[23] Substantially, Bellavitis obtained the following results:

[Th. 4.1]. *Given an algebraic polynomial function:*

$$y = a_0x^n + a_1x^{n-1} + \cdots + a_n, \tag{4}$$

[23]The geometrical constructions of algebraic equations which we find in the treatments of algebrists of the XVI century (Cardano, Ferrari, Tartaglia, Bombelli, Bonasoni, Stevin) constitute the 'synthetic geometrical theory of algebraic equations' (see P.Freguglia "Sur la théorie des équations algébriques entre le XVI et le XVII siècle", *Bollettino di Storia delle Scienze Matematiche*, vol. XIV(1994) fasc.2, and "Sul principio di omogeneità dimensionale tra Cinquecento e Seicento", *Bollettino UMI* 1999, (8) 2–B). These constructions are different from those made by Descartes in his *Géométrie* (see H.J.M.Bos "Arguments on Motivation in the Rise and Decline of a Mathematical Theory; the 'Construction of Equation', 1637–ca. 1750", *Arch. for Hist. of Exact Sciences*, XXX (1984)).

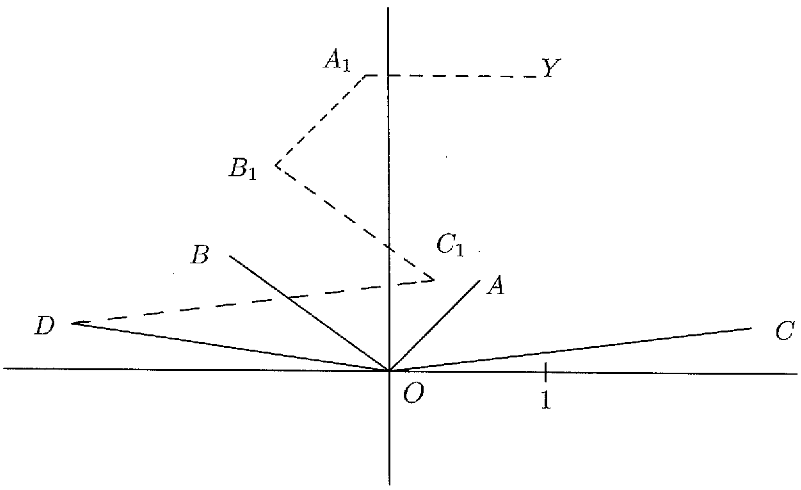

Figure 8:

then an equipollence:

$$Y \doteq A_0 X^n + A_1 X^{n-1} + \cdots + A_n \tag{5}$$

exists and it represents (4) *in* $\mathbb{R}^2$ *(Euclidean plane).*

We will present the construction as regards (4) for the case of fourth degree, in the same way Bellavitis did. For any $x \in \mathbb{C}$ we can give the following construction (see Fig.8):

1. X can represent any complex number, but let us consider (with Bellavitis) the case $X = 1$ to make the drawing easier.
2. We will draw (see Fig.8) the vectors — points $1, A, B, C, D$ (coefficients of the equation, which are in general complex numbers).
3. $DC_1 \doteq OC$; $C_1B_1 \doteq OB$; $B_1A_1 \doteq OA$ and then $A_1Y \doteq O1$.
4. So we have the open vectorial polygon $ODC_1B_1A_1Y$.

We have gone from O as far as Y. We obtain the same result if we add vectors in succession as follows:

$$\begin{aligned} OD + OC &\doteq OC_1, \\ OC_1 + OB &\doteq OB_1, \\ OB_1 + OA &\doteq OA_1, \\ OA_1 + O1 &\doteq OY. \end{aligned}$$

Therefore:

$$OY \simeq O1 + OA + OB + OC + OD$$

and also:

$$OY \simeq 1^4 + OA \cdot 1^3 + OB \cdot 1^2 + OC \cdot 1 + OD$$

and thus, by putting X in place of 1 (of course the variable of Y depends on X):

$$Y \simeq X^4 + AX^3 + BX^2 + CX + D,$$

which is (5) for the case of fourth degree.

Now, if $F(x) = 0$ is an algebraic equation, *we will associate to the equation* $F(x) = 0$ *an identity namely* $F(x_0) = 0$, where x_0 is a root of $F(x) = 0$. Moreover, an *(open or closed) vectorial polygon* is a finite succession of vectors where the origin of one coincides with the end of the previous one, save that we say otherwise. It is possible to have a closed vectorial polygon where its first vector has the origin which coincides with the origin of its last vector (see for example Fig.9, where OB is the first vector and OA is the last vector of the closed vectorial polygon). We have:

[Th. 4.2]. *If X_0 is a geometrical representation of a root x_0 of the equation:*

$$x^n + a_1x^{n-1} + \cdots + a_n = 0, \tag{6}$$

then a closed vectorial polygon (of $n+1$ vectors) exists in such a way that the origin of the first vector coincides with the origin of the last vector and they both coincide with the origin O (for example see the closed vectorial polygon OBA of Fig.9*).*

Proof. Indeed if x_0 is a root of (6) then to equation (6) we associate the identity:

$$x_0^n + a_1x_0^{n-1} + \cdots + a_n = 0. \tag{7}$$

But (7) is (4) for $x = x_0$ and $y = 0$. According to the earlier theorem 4.1 we can then represent (7) in $\mathbb{R}^2$ by the (vectorial) equipollence:

$$X_0^n + A_1X_0^{n-1} + \cdots + A_n \simeq 0, \tag{8}$$

where X_0 represents x_0 in $\mathbb{R}^2$. Fig.9 represents the special case of the quadratic equation.

$x^2 - 2x + 4 = 0,$ with the root $x_0 = 1 + i\sqrt{3}$,
making the angle $\alpha = 60°$ with the fixed axis.

$X_0^2 - 2X_0 + 4 \simeq 0$

$X_0^2 \simeq 2X_0 - 4$ OBA is the closed vectorial polygon of 3 vectors:
$X_0^2 \equiv OA; \quad -4 \equiv OB; \quad 2X_0 \simeq BA.$

□

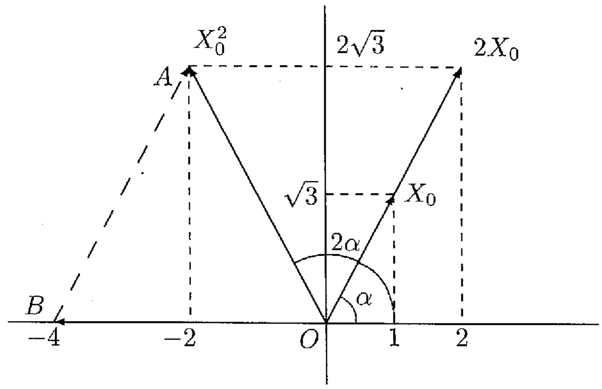

Figure 9:

Bellavitis wrote this essay in 1847. Later, Karl Culmann in his *Graphische Statik* (1864, 1865) proposed a method of solving algebraic equations by using graphical-numerical approximate values.[24]

From a foundational point of view Bellavitis's position regarding the fundamental theorem of algebra is in line with Français's formulation of this theorem, that is:[25]

> All roots of an algebraic equation of whatever degree are real, and they can be represented by segments whose lengths and directions are given.

[24]Culmann was inspired by the works of some of his contemporary mathematicians (Stamm, D'Egger, Jäger), see K.Culmann [1880] pp.16–19.

[25]See J.F. Français [1813] p.73, "THÉORÈME IV. Toutes les racines d'une équation de degré quelconque sont *réelles*, et peuvent être représentées par des droites données de grandeur et de position".

5 Conclusion

Bellavitis had an extensive correspondence[26] above all with Italian mathematicians (for example Fusinieri, Casari, Genocchi, Chelini, Minich, Piola). He also had contacts with foreign (European) mathematicians such as the Frenchmen C.A. Laisant and M.J.Hoüel, and the Bohemian Zahradnik, who all translated and elaborated[27] Bellavitis's work, in particular his equipollences theory. Bellavitis was one of the first mathematicians in Italy who knew and studied W.R.Hamilton's work regarding quaternions. He translated the theory of quaternions into his own language and gave a lecture on this subject at the Istituto Veneto di Scienze, Lettere ed Arti on March 21st 1858. In the same year he wrote and published an essay about the relationship between quaternions and the equipollences method.[28] The ideas of H.Grassmann were also known to and studied by Bellavitis. [29]

Lastly we can try to draw some conclusions about Bellavitis's ideas on complex numbers. We can conclude that Bellavitis's contribution was interesting, but conceptually it imparted no particularly new ideas compared to the contributions of other authors who studied the same theme during the same period. The epistemological approach of Bellavitis was closely related to that of Buée and Français. Nevertheless, we must give credit to Bellavitis for having developed the structure of the calculus and for having applied it to the foundation of the theory of complex numbers and to the deduction of theorems of elementary geometry (see the following appendix) and so to have proposed a synthetic and constructive method which is not in contrast with analytical geometry.

Appendix

A.1. Bellavitis's proof of Desargues's theorem on homological triangles

(See G.Bellavitis [1854] pp.20–21)—(*Theor.*):"If the vertexes of two triangles ABC and $A'B'C'$ are in a straight line with the fixed point S, then the

[26]See *Appendix B* (by G.Canepa) of P.Freguglia [1992] and G.Canepa [1994].
[27]See M.J.Hoüel [1869], C.A.Laisant [1874], [1887].
[28]See G.Bellavitis [1858].
[29]See G.Bellavitis [1854b].

intersection points T, U, V of their corresponding sides are also in a straight line".

(*Proof*): The given conditions on the triangles ABC and $A'B'C'$ can be translated into the following equipollences (see Fig.10):

$$SA' \doteq aSA, \quad SB' \doteq bSB, \quad SC' \doteq cSC.$$

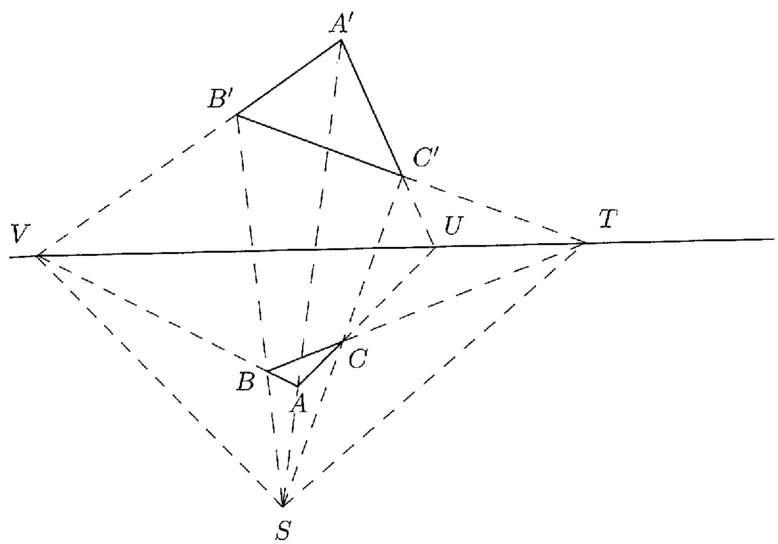

Figure 10:

where a, b, c are numerical coefficients. Moreover the condition that V belongs to the straight line AB is expressed by $AV \doteq nAB$, or if we reduce to SA, SB and SC, by:

$$\begin{aligned} SV &\doteq SA + AV \doteq SA + nAB \\ &\doteq SA - nBA \doteq SA - n(SA - SB) \doteq (1-n)SA + nSB. \end{aligned} \quad (9)$$

But V must also belong to $A'B'$, hence analogously we will have:

$$SV \doteq (1-m)SA' + mSB' \doteq a(1-m)SA + bmSB. \quad (10)$$

Comparing (9) with (10) we have:

$$(1-n)SA + nSB \doteq a(1-m)SA + bmSB,$$

which by the second canon implies that

$$(1-n) = a(1-m) \quad \text{and} \quad n = bm. \tag{11}$$

From the system (11) we can deduce the value of m, and if we replace it in (10) we have:

$$SV \simeq [(1-b)/(a-b)]SA' + [(a-1)/(a-b)]SB'. \tag{12}$$

Analogously we have:

$$ST \simeq [(1-c)/(b-c)]SB' + [(b-1)/(b-c)]SC' \tag{13}$$

and

$$SU \simeq [(1-a)/(c-a)]SC' + [(c-1)/(c-a)]SA'. \tag{14}$$

From (13) we find SB' and from (14) we find SA'. If we replace the respective expressions in (12), we have

$$(a-b-ac+bc)SV + (c-a+ab-bc)SU + (b-c-ac-ab)ST \simeq 0. \tag{15}$$

But since $SV \simeq TV + ST$ and $SU \simeq TU + ST$, we get from (15):

$$(a-b-ac+bc)TV + (c-a+ab-bc)TU \simeq 0. \tag{16}$$

From the second canon we conclude that TV has same inclination as TU, i.e. that the points T, U, V are in the same straight line. Hence Desargues's theorem is demonstrated.

A.2. Pythagoras's theorem as proved by Bellavitis

(See G.Bellavitis [1854] pp.13 and foll.) — Given any triangle ABD (see Fig.11), where AC is its median, then

$$AB + BC \simeq AC \quad \text{that is} \quad AB \simeq AC + CB.$$

Similarly

$$AD \simeq AC - CB.$$

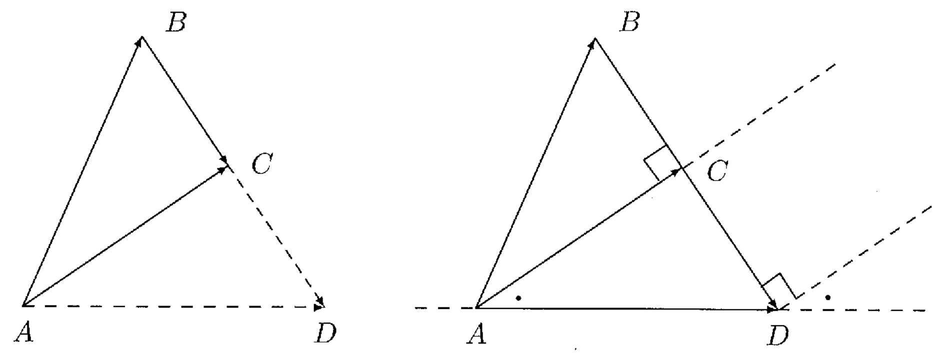

Figure 11:

By multiplying these two equipollences with each other we have:

$$AB \cdot AD \simeq (AC + CB)(AC - CB) \simeq (AC)^2 - (CB)^2$$
$$\text{or } AB \cdot AD + (CB)^2 - (AC)^2 \simeq AB \cdot AD + (CB)^2 + AC \cdot CA \simeq 0. \quad (17)$$

Besides, if we suppose that CB is perpendicular to AC, that is

$$\operatorname{inc} CB = \operatorname{inc} AC \pm 90^\circ, \quad (18)$$

we also have:

$$2 \operatorname{inc} CB = 2 \operatorname{inc} AC \pm 180^\circ \quad (19)$$

Moreover:

$$\text{since } (CB)^2 \simeq CB \cdot CB \quad \text{we have} \quad 2 \operatorname{inc} CB = \operatorname{inc} CB + \operatorname{inc} CB \quad (20)$$

and

$$\text{since} \quad -(AC)^2 \simeq AC \cdot CA \quad \text{it follows that}$$
$$\operatorname{inc} AC + \operatorname{inc} CA = \operatorname{inc} AC + \operatorname{inc} AC \pm 180^\circ = 2 \operatorname{inc} AC \pm 180^\circ. \quad (21)$$

If we compare (20) and (21) with (19) we find that the last two terms of (17) have the same inclination. Therefore we can apply the third canon (to (17)), that is:

$$\begin{aligned} \operatorname{gr}(AB \cdot AD) &= (\operatorname{gr} CB)^2 + (\operatorname{gr} AC)^2 \\ \operatorname{inc}(AB \cdot AD) &= (2 \operatorname{inc} AC \pm 180^\circ) \pm 180^\circ = 2 \operatorname{inc} AC. \end{aligned} \quad (22)$$

But since $\operatorname{inc} AC \pm 90^\circ = \operatorname{inc} DB$ we have that $2\operatorname{inc} AC \pm 180^\circ = 2\operatorname{inc} DB$ or $2\operatorname{inc} AC = 2\operatorname{inc} DB \pm 180^\circ$; from which:

$$\begin{aligned} &\operatorname{inc}(AB \cdot AD) = \operatorname{inc} AB + \operatorname{inc} AD = 2\operatorname{inc} DB \pm 180^\circ \\ \text{and} \quad &\operatorname{inc} AB + \operatorname{inc} DA \pm 180^\circ = 2\operatorname{inc} DB \pm 180^\circ \end{aligned}$$

so that:

$$\operatorname{inc} AB + \operatorname{inc} DA = 2\operatorname{inc} DB. \tag{23}$$

We also have:

$$AD + DB \doteq AB \quad \text{or} \quad BA + DB + AD \doteq 0. \tag{24}$$

Given (23) and (24), we can apply the fourth canon, from which we conclude that:

$$\operatorname{gr} AB = \operatorname{gr} AD. \tag{25}$$

If we substitute (25) into (22) we have:

$$(\operatorname{gr} AB)^2 = (\operatorname{gr} CB)^2 + (\operatorname{gr} AC)^2$$

which is Pythagoras's theorem.

From the technical viewpoint this proof is different from the Euclidean proof, but it is based on the same principles (see definition of equipollence and the proof of the fourth canon).

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Hypercomplex Numbers in the Work of Caspar Wessel and Hermann Günther Grassmann: Are there any Similarities?

Karl–Heinz Schlote *

1 Introduction

At first glance one might argue that it is somewhat peculiar to compare the work of Caspar Wessel and Hermann Günther Grassmann with regard to hypercomplex number systems. In fact, it is quite apparent that Grassmann never heard of Wessel's paper "Om Directionens analytiske Betegning" (On the analytical representation of direction)[1], and reading that title as well as the titles of most of Grassmann's papers we would not expect a contribution to hypercomplex number systems. However, a second glance at their lives and work already reveals that there are some formal similarities. Both Wessel and Grassmann had rather isolated positions in the scientific community and their ideas and findings were only appreciated after considerable delay. Until now there are only a few points of resemblance in the living circumstances of these two mathematicians. Therefore, this article examines whether there are similarities in the contributions of Wessel and Grassmann to the theory of hypercomplex numbers.

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[1]In the following the English translation [Wessel, 1999] of Wessel's essay is used.

2 The contribution of C. Wessel to the theory of hypercomplex numbers

In his publication "On the analytical representation of direction" C. Wessel aimed at developing a geometrical calculus for calculation with directed magnitudes, i.e. in modern terms – for calculations with vectors. In the introduction he stated: "The present attempt deals with the question of how to represent the direction analytically, or, how one ought to express straight lines, if from a single equation in one unknown line and some given lines one is to be able to find an expression representing both the length and the direction of the unknown line." ([Wessel, 1999], 103)

Thus, Wessel had to give an appropriate definition of operations with directed magnitudes. While working on this, he was led to fundamental considerations on the character of these operations. He clearly expressed that the new magnitudes differed substantially from the ordinary numbers and that the operations with the former had to be redefined. He solved this problem by generalising from the operations known for calculating with natural numbers. In other words, he wanted to expand the domain of applicability or — in modern terms — the domain of definition of these operations. At the same time he tried to maintain as many properties of these operations as possible. In this sense his approach is comparable to the ideas developed in the English algebraical school, and in particular it reminds us of Peacock's principle of permanence of form. However, Wessel's work only marked the beginning of this development.

Late in the 18th century, several mathematicians tried to work out an exact foundation for the number system, above all, to incorporate negative and imaginary numbers into the system. During their studies they recognised basic properties of calculations with numbers, i.e. those properties from which all other rules for numerical calculations could be derived. However, basic rules for number systems and calculation with numbers had yet to be abstracted and, in consequence, the principle of permanence for operations with abstract magnitudes likewise remained unknown. We can trace this process in the work of Wessel. His general notion was to speak of properties for calculations with numbers, and he stressed for the operation of addition that the added terms can be permuted and that multiplication and addition are distributively connected. Of course he did not use the terms commutative and distributive, since they were only coined several years later.[2] At nearly the same time, C.F. Gauß, working on binary quadratic forms (cf. [Gauss, 1801]), pointed to the fact that commutativity and associativity cannot be taken for granted for connections in domains of general magnitudes. These properties must be proven as specific in every connection. This clearly demonstrates the basic problem of that process: To be able to investigate a system of magnitudes which includes the realm of positive real numbers or contains magnitudes which are different from numbers, one has always to decide which rules of connections are the basic ones. In other words, one has to depict which rules of combination determine the algebraic structure of the realm of magnitudes. Therefore, Wessel's statement to the effect that the rules of connection traditionally used in numerical realms are maintained, was definitely not a trivial one. At that early stage of development it represented a first significant insight into the structure of numerical domains.

Shortly after Wessel had published his essay, the now well known properties associativity, commutativity and distributivity were abstracted and became standard conditions in considerations about numerical realms. However, it should be underlined that this process of abstraction was not only due to investigations on number systems (cf. [Novy, 1973], Chap. 4 & 5).

A second important feature of Wessel's work should be considered. Wessel chose two directions, first the real axis and a second axis perpendicular to the first. He introduced the units 1 and ε, corresponding to those directions, and made clear that all other magnitudes, i. e. every line segment in the plane, can be represented by a linear combination of these units. Wessel derived the multiplication of the units with each other including -1 and $-\varepsilon$ as units. At this stage he remarked that $\varepsilon = \sqrt{-1}$, and therefore complex numbers are contained in his investigation as a special case. The geometrical representation of the units, together with the rule that the amplitude of the product of two line segments is equal to the sum of the amplitudes of its factors, formed the basis for Wessel's determination of the multiplication of any two line segments. He stated that the product was taken componentwise and was determined unequivocally by the products of the units.

That concept of unit was geometrically motivated but one should not minimize its stimulating impact on algebra. The same is valid for Wesel's insight

[2]The terms "distributive" and "commutative with each other", for instance, were coined by F.-J. Servois in a paper about the theory of operators in 1814. [Servois, 1814]

that the product was determined by the products of the units. The geometrical motivation caused Wessel to extend his method to space, hence to extend multiplication to three-dimensional magnitudes. Trying to realize this, Wessel introduced a further unit η, whose square is equal to -1, which was the same condition as for ε. Geometrically, the new unit represented a direction which is perpendicular to the plane generated by the units 1 and ε. Then Wessel defined a specific connection, though not a multiplication as Wessel remarked, since it was applicable only to some but not to all of his three-dimensional magnitudes. The connection pointed to a strong geometrical influence and was constituted in such a way that products of the units ε and η were avoided. Thus, Wessel had targeted a problem, the solution of which resulted in the emergence of the first system of hypercomplex numbers. However, the solution of the problem was not given by W.R. Hamilton until October 1843, after more than 10 years of intensive investigations. This was the birth of the quaternions and marked a climax in the history of hypercomplex number systems. His own restricted calculus was obviously sufficient for Wessel to reach his objectives.

Apart from the idea of extending and generalizing the complex numbers to space, most of Wessel's results mentioned above were also stated by other mathematicians who directed their efforts toward the foundation of imaginary numbers.

Concluding the discussion on Wessel's work, two aspects should be emphasized. First, Wessel's essay was the first publication giving an exact treatment of the geometrical representation of complex numbers and, on this basis, of calculation with them. Second, Wessel situated his investigations in the general framework of searching for a geometrical calculus. This forced him to think about the foundation of the connections of the new magnitudes and the properties of the connections. Furthermore, his search for a geometrical calculus was an important impulse to extend his derived method to space. Although Wessel was not entirely successful in this attempt, the description of which new problems emerge in the calculations with three-dimensional magnitudes was of the utmost importance for further developments.

In this context it should only be noted that Wessel prepared and developed basic elements of a vector calculus. It remains speculative whether Wessel had been aware that he followed the tradition of Leibniz. The commentators on the French translation of Wessel's article, H. Valentiner and T.-N. Thiele, did not refer to this tradition. They only indicated that the geometrical representation of imaginary numbers as well as calculations with them are of relevance and established a relation to the quaternions of Hamilton.

3 The contributions of H. G. Grassmann to the theory of hypercomplex numbers

Many of Grassmann's ideas, developed about 50 years later, can be seen as a continuation of Wessel's results. This supports the assessment that Wessel's essay had already uncovered important problems for future research.

At first some biographical data on Grassmann should be given as far as they concern his research discussed below. Hermann Günther Grassmann was born in Stettin on April 15th, 1809. Starting in 1827 he studied theology for 6 semesters at the University of Berlin. During this time he also attended lectures in philosophy and philology. Thus, Schleiermacher's ideas on dialectics, with which he became acquainted during his studies, as well as the philosophy of Kant, had a strong impact on Grassmann's work. After he had returned to Stettin in 1830, he devoted his efforts to the intensive private study of mathematics and physics as well as theology. Finally, he qualified to teach these subjects with a career as a teacher in view. Between 1831 and 1840 he passed teaching examinations for the gymnasium in Stettin, where he worked as a teacher from 1835 onwards. All his efforts to obtain a professorship at a university failed even though he published his theory of extensions ("Die lineale Ausdehnungslehre ... ") in 1844 and contributed to the advance of mathematics in several books and articles.. Only a few years before Grassmann's death did some mathematicians come to appreciate his ideas, at which point he received some acknowledgement in the scientific community. An exception was his philological studies on Sanskrit which were immediately appreciated by scientists. Grassmann died on September 26, 1877.

The mathematical ideas of Grassmann developed slowly. It took several years before he had brought these into a programme which he attempted to realize in cooperation with his brother Robert. One source from which Grassmann derived aspects of his research was his occupation as a teacher.

In this context he undoubtedly had to think about the structure of mathematics as a science and realized that it was necessary to give an exact foundation and construction of mathematics. Thus, at the beginning of the 1830s he was led to the concept of a new addition of directed line segments. This was nothing less than the addition of vectors. From a retrospective point of view, the description Grassmann gave was very similar to Wessel's explanations in his essay. However, daily duties at school prevented Grassmann from working out his new method in more detail. Moreover, he had not yet been convinced of the efficiency of the new method. His further research was stimulated by publications of his father, who had investigated products in geometry in particular in the book "Raumlehre" [Grassmann, 1824]. Grassmann developed his father's approach further, defined a product of directed line segments which corresponds to the modern vector product, and combined the product with the addition of vectors established some years ago. This last step seems to be very natural, but it was a decisive one. Now he suspected that he had created the beginnings of a new efficient method. It was already during this early stage that Grassmann discovered that the new multiplicative operation was non-commutative. In fact this "curious result" as Grassmann named it initially embarrassed him, nevertheless it was not a sufficient reason for him to reject this multiplication ([Grassmann, 1894] Band I, Theil 1, 8). Once again, he did not have the time to elaborate his findings. He returned to his method only in 1839 when he worked on the theory of ebb and flow of P.S. Laplace during his teaching examinations. He simplified and improved the representation of Laplace's theory and concluded:

> Thus I feel entitled to hope that in this new analysis I have found the only natural way in which mathematics should be applied to nature, and likewise that in which geometry should be treated ...
> ([Grassmann, 1844a], 10)[3]

Within four years he elaborated the calculus completely and published the book: "Die lineale Ausdehnungslehre ... " Nobody denies that Grassmann

[3] "Durch diesen Erfolg nun hielt ich mich zu der Hoffnung berechtigt, in dieser neuen Analyse die einzig naturgemässe Methode gefunden zu haben, nach welcher jede Anwendung der Mathematik auf die Natur fortschreiten müsse, und nach welcher gleichfalls die Geometrie zu behandeln sei, ... " ([Grassmann, 1894] Band I, Theil 1, 8). The quotation follows the English edition of Grassmann's theory of extension. (I am grateful to Dr. G. Schubring who sent me a copy of a part of that edition.)

presented important new results on geometry but here we shall consider only his ideas on hypercomplex numbers. One would not expect a relation between geometry and hypercomplex numbers in a book entitled "Lineare Ausdehnungslehre" (Linear extension theory). The results on hypercomplex numbers are basically due to the fact that Grassmann treated problems in a very abstract manner. He started the book with a general philosophical introduction and defined mathematics as a formal science that examined objects established by pure thought. He intended to give a uniform formal and constructive development of mathematics.[4] For that, he chose the concept of form or of "thought-form" as a starting point and began with general considerations on connections of abstract magnitudes. In the end he justified his discussions for fitting in the new science too. Grassmann regarded the properties of connections as those rules which are valid for all forms and with it for all parts of mathematics. This resulted in an emphasis on the rules of connections and a removal of the connected magnitudes from the world of concrete particular objects. In abstracting from the concrete realization of the connections in several parts of mathematics, Grassmann performed an important step and tried to grasp the structure determined by the connections. This demonstrates Grassmann's insight that it was necessary to abstract from the peculiarities of the connected objects to arrive at common structural properties. The significance which Grassmann attached to the rules of connection was documented, among other things, by his division of mathematics into four branches and his definition of mathematics as a "science of the connection of magnitudes", both based on the concept of connections. This opinion was still expressed in more detail in the book "Formenlehre" (Theory of forms) [Grassmann, 1872] written by Robert Grassmann in cooperation with H. G. Grassmann. There Robert Grassmann wrote:

> The theory of forms or mathematics consists of five branches, a general one, the science of magnitudes, and four particular branches.
>
> 1) The science of magnitudes, both the first and general branch of the theory of forms, tells us about the connection of magnitudes which are a common part of all branches of the theory of forms ... ([Grassmann, 1872], 11)[5]

[4]The philosophical background is discussed in [Lewis, 1977].

[5]The following quotations were translated by the author. The original German version is given in the notes."Die Formenlehre oder die Mathematik zerfällt in fünf Zweige, einen

The "theory of forms" was part of the programme for a new foundation of mathematics pursued by Robert and Hermann G. Grassmann. The above quotations also reflect H.G. Grassmann's opinion towards this aspect. Moreover, these quotations express an extended concept of magnitudes elaborated by Grassmann in his research. In 1844, in the "Ausdehnungslehre", Grassmann refused the characterization of mathematics as a science of magnitudes because the concept of magnitudes was too narrow and as a consequence restricted to numbers. Step by step Grassmann presumably recognized that his definition of a magnitude as "everything which was received via thinking" ([Grassmann, 1894] Band I, Theil 1, 24) can be interpreted in a much more abstract manner than he had done in the book "Ausdehnungslehre".

After he had divided mathematics into four branches and determined the position of the theory of extensions as a new branch of mathematics, Grassmann analysed the properties common to all connections in the general theory of forms. At first he introduced the concept of equality, then he treated the connections. He supposed the following as a principle: Any two magnitudes are connected and the result of their connection has to be unambiguous. If more than two magnitudes should be connected, this will be carried out by successive operations on two magnitudes.

In this context Grassmann addressed the important problem of whether a set of magnitudes is closed under the connection. This property is very conclusive for defining a mathematical structure. Grassmann did not discuss the question in an abstract manner. That is understandable, since it was not his purpose to define and analyse mathematical structure. Nevertheless it can be seen that Grassmann discerned this problem as an important one. In most of the cases he investigated, Grassmann considered the system of magnitudes as closed. He derived this conviction from the manner in which magnitudes were generated. But in some cases he could not draw this conclusion: for instance, when he connected extensive magnitudes by multiplication he got extensions of a higher degree. The result was again an extensive magnitude of course, but the multiplication could formally yield a magnitude (or extension) of any degree. In such cases Grassmann did

allgemeinen Zweig, die Grössenlehre und vier besondere Zweige. 1) Die Grössenlehre, der erste oder der allgemeine Zweig der Formenlehre, lehrt uns die Knüpfungen der Grössen kennen, welchen allen Zweigen der Formenlehre gemeinsam sind, ... "

not answer the question of closure. Only when the system is reduced to a finite number of units of first degree the extensions of higher degrees become closed in a natural way. (Every product with more than n factors is equal to zero.) As examples we mention, first, the combination of progressive and regressive product to the "product with regard to a principal domain" and second, the investigation of subdomains in a larger domain of magnitudes (or what we would call, in modern terms, the properties of a vector subspace). In the first case the product was defined in such a way that the result was contained in a domain determined in advance. To describe the second case Grassmann stated:

> Since every system which contains some magnitudes has to contain also all magnitudes depending on the former ones and also the outer product of them, i. e. the whole system which is determined by them ... ([Grassmann, 1894], Band I, Theil 1, 206f)[6]

Again, both instances clearly showed the significance which Grassmann attached to a closure of the system.

In a system of magnitudes with one connection, the permutation of the members and a change of the brackets are the only two possible operations which could be carried out without changing the forms or the result of the connection, respectively. This would mean that the connection satisfies the commutative and the associative laws. A connection which satisfies both laws is called a simple connection. After that Grassmann treated the so-called resolution of a given connection or the inverse operation, which he called an analytical connection. The definition of this resolution is so general that a non-commutative connection can be considered initially, which Grassmann also termed a synthetic one. However, Grassmann restricted his investigations immediately to simple synthetic connections and asked for conditions under which both operations can be permutated and brackets can be added or omitted. He recognized that the analytical connection can be ambiguous in some particular cases. In cases where the synthetic connection was simple and the accompanying analytical connection was single-valued, he called the former addition and the latter subtraction. Later Grassmann additionally introduced an indifferent and a purely analytical form. In modern notation these correspond to an identity element and an inverse element,

[6] "Da nun jedes System, welches gewisse Grössen enthält, auch sämmtliche von ihnen abhängige Grössen, das heißt das ganze durch sie bestimmte System, also auch das äussere Produkt jener Grössen, enthalten muss, ... "

respectively. The indifferent form was obtained by an analytical connection of two equal forms, the purely analytical one by an analytical connection of an indifferent form with the initial one. Notably, Grassmann explained that if the analytical connection is assumed to be single-valued, then the definition of the indifferent form is independent from a special choice of the form. This can be seen as a first sign of a new algebraic way of thinking and as a step towards defining objects by means of equivalence classes. In addition to this prospective aspect one can find a remarkable weakness in this definition. An identity element had only been defined, if the inverse operator existed and was unambiguous. Thus, a ring with zero-divisors would never have an identity.

When a second synthetic connection existed, Grassmann regarded the mutual relation as the main problem. He gave an argumentation from which a distributive combination of both operations derived. If the operations were distributively connected and the first one was an addition, then he called the second one a multiplication. A set of magnitudes supplied with two such connections constitutes a ring, but Grassmann neglected this structural point of view. There is no one position in Grassmann's work where he alluded to the definition of a new mathematical object, a new algebraic structure. Although he had mentioned all properties which are neccessary for an axiomatic characterization of a commutative group or a ring, he never realized this step. The possibility of defining a set of magnitudes having one or two connections as a new mathematical entity was overlooked by Grassmann. He identified the concept of connection in a system of magnitudes as the most decisive one, and the different determinations of the connection as presenting possibilities for the analysis of the various systems of magnitudes, but he did not recognize that the systems represent different mathematical objects with regard to the different algebraic structures. This gives a clear idea of the extent to which Grassmann contributed to the genesis of modern algebra and his limitations. He established the opportunity for defining abstract structures, but he was not in a position to consider abstract algebraic structures. However, this does not diminish Grassmann's achievement. How far he was ahead of his contemporaries can be seen from the fact that according to him, a system of magnitudes carrying an addition has to fulfil certain conditions which implicitly suppose the existence of a certain structure.

Grassmann concluded his general theory of forms with some remarks on division as the analytical connection to multiplication, but he did not present any new ideas. Again he stated the necessity of defining two analytical connections as a consequence of the non-commutative multiplication. After these general considerations on connections, Grassmann developed his elaboration of the theory of extensions in which he applied his ideas in various ways. His description was verbose and difficult to understand. Grassmann was not satisfied with the level achieved by his theory, and he therefore explained some of its details in further articles. Finally, he published a revised edition of the "Ausdehnungslehre" in 1862 "in Euclidean form" (that is, in stronger Form), which pointed out the formal aspects and completely omitted the philosophical considerations. This shows that Grassmann did not extend the domain of research chosen earlier, but within it he expressed his ideas more clearly and gained a better mastery of the methods.

Within a relatively short time he had reached a new grasp of the structure of his theory and an improved mastery of it. This can be derived from a comparison of the book "Ausdehnungslehre" (1862) with his textbook on arithmetic from 1861 and the article "Sur les différents genres de multiplication" from 1855. What basic changes can be traced?

Grassmann started with the concept of unit and a system of units as a basis of a system of magnitudes or extensive magnitudes, respectively. An extensive magnitude was defined by

$$\alpha_1\mathbf{e}_1 + \alpha_2\mathbf{e}_2 + \cdots = \sum \alpha_i\mathbf{e}_i$$

with α_i real numbers, called numbers of derivation (Ableitungszahlen) by Grassmann and $\{\mathbf{e}_i\}$ denoting a system of units. The sum above was of course a finite one, but Grassmann did not mention it in any case, and usually it is also possible to think of an infinite one. He defined the addition of extensive magnitudes componentwise and had then to define the multiplication of an extensive magnitude with a scalar, usually a real number. He determined it in a formal way by

$$\beta * \sum \alpha_i\mathbf{e}_i = \left(\sum \alpha_i\mathbf{e}_i\right) * \beta = \sum(\beta\alpha_i)\mathbf{e}_i.$$

Although he regarded only real numbers as the domain of coefficients, it is possible to choose any abstract coefficient field without difficulties. The multiplication satisfies the following rules (cf. [Grassmann, 1894], Band I, Theil 2, 14)

$$\begin{aligned} \mathbf{a} * \beta &= \beta * \mathbf{a}, & (\mathbf{a} * \beta) * \gamma &= \mathbf{a} * (\beta\gamma), \\ (\mathbf{a} + \mathbf{b}) * \beta &= \mathbf{a} * \beta + \mathbf{b} * \beta, & \mathbf{a} * (\beta + \gamma) &= \mathbf{a} * \beta + \mathbf{a} * \gamma, \\ \mathbf{a} * 1 &= \mathbf{a}, \\ \mathbf{a} * \beta &= 0 \quad \text{if and only if either } \mathbf{a} = 0 \text{ or } \beta = 0. \end{aligned}$$

In an attempt to compare Grassmann's and Wessel's work, a short overview of some geometrical aspects of Grassmann's theory of extension should be given at this stage. At first Grassmann had selected all properties which he would need for an abstract definition of a linear vector space as a new mathematical object and again he omitted this level of abstraction. He performed his calculations in a domain or system of extensive magnitudes without considering it as a mathematical object in its own right. Nevertheless, he derived important results for vector spaces. Thus he described the linear dependence of extensive magnitudes as standing in a numerical relation to each other. In the article mentioned above he also supposed the linear independence of the relative units which form the basis of the extensive magnitudes. All in all Grassmann included the following theorems of linear vector spaces in his book:

— A domain (or system) of nth order is generated by exactly n independent elements. The concept of a basis is not used by Grassmann. A domain of nth order corresponds to an n-dimensional space.
— The theorem of exchange, later also called the fundamental replacement theorem of Steinitz, considering the possibility of replaceing a certain set of q vectors from among the vectors $\mathbf{a}_1, \mathbf{a}_2, \dots, \mathbf{a}_p$ which span a vector space L by q linear independent vectors $\mathbf{b}_1, \mathbf{b}_2, \dots, \mathbf{b}_q$ of L ($q < p$).
— The theorem on the dimension of the sum of two linear subspaces

$$\dim(E + F) + \dim(E \cap F) = \dim E + \dim F.$$

Even in this context Grassmann appears as an intellectual successor of Wessel, and his initial findings on vector calculus. It is common knowledge that some of the above stated theorems of linear spaces also play an important role in algebra, which was probably first announced by R. Dedekind in his famous 10th supplement to Dirichlet's "Lectures on number theory". However, Grassmann could not have foreseen those aspects.

There is still another problem on a less abstract level which has to be analysed with regard to Grassmann's contribution to the theory of hypercomplex numbers. It concerns the product of two extensive magnitudes. The most detailed description is given by Grassmann in the already mentioned article "Sur les différents genres de multiplication" written in a priority claim made against A.L. Cauchy.

In accordance with his general remarks in the "Ausdehnungslehre" of 1844, Grassmann defined the multiplicative connection of two extensive magnitudes by

$$\sum \alpha_r \mathbf{e}_r * \sum \beta_s \mathbf{e}_s = \sum \alpha_r \beta_s (\mathbf{e}_r * \mathbf{e}_s).$$

The distributive law was tacitly assumed and no further restrictions were made. This was completely correct since he wanted to define a multiplicative connection of magnitudes which were already connected by addition. In this case both connections have to be combined by the distributive law. Furthermore he stressed that the product of the two relative units has to be regarded as a new unit of second degree, or of nth degree if n units are multiplied. This point of view was very general, even too general, to derive some results or to apply his considerations on a geometrical problem, for instance. Thus Grassmann had to make further specifications to the product $(\mathbf{e}_r * \mathbf{e}_s)$, and therefore he introduced the so-called "equations of determination" (Bestimmungsgleichungen). He stated:

> Since the product of two extensive magnitudes is according to the definition again either an extensive magnitude or a numerical magnitude (Zahlgröße), it must be numerically derivable from a system of units. This definition does not explain which system of units it should be and how the products $[\mathbf{e}_r * \mathbf{e}_s]$, of which these products are composed, are numerically derivable from these units. If a particular product should be determined in detail, the necessary determinations on this system of units and these derivations have to be made. ([Grassmann, 1894], Band I, Theil 2, 28)[7]

[7] "Da das Produkt extensiver Grössen nach der Erklärung wieder entweder eine extensive Grösse oder eine Zahlgrösse ist so muss dasselbe (...) aus einem System von Einheiten numerisch ableitbar sein. Welches dies System von Einheiten sei, und wie aus ihnen die Produkte $[\mathbf{e}_r * \mathbf{e}_s]$, aus denen jenes Produkt zusammengesetzt ist, numerisch abzuleiten seien, darüber sagt die Definition nichts aus. Soll also der Begriff eines besonderen Produktes genau festgestellt werden, so müssen noch über jenes System von Einheiten und über diese Ableitungen die nötigen Bestimmungen getroffen werden."

Each system of equations of determination defined a specific kind of product. Grassmann realized that different products result in different systems of magnitudes and therefore the kinds of products or the equations of determination furnish a decisive distinguishing feature for systems of magnitudes and a possibility to classify them. To master this problem would require a complete classification of hypercomplex number systems, and thus it is not surprising that Grassmann solved these problems only in particular cases. His choice of equations for a further determination of the products was clearly influenced by geometrical ideas, since, as he said: "nature calculates in a geometrical manner" ([Grassmann, 1894], Band III, Theil 1, 33). But many factors which cannot be analysed in this article have to be borne in mind.[8] Only two facts should be stated. Firstly, Grassmann did not take into account other ideas of his time such as Hamilton's quaternions, maybe as a consequence of Grassmann's isolated position at his grammar school at Stettin. Secondly, he restricted the equations of determination to those in which all products have the same number of factors, mostly on products comprising only two factors. Grassmann gave no explanation of this very restrictive condition and his reasons remain a mystery for us. Maybe his statement that only connections of two objects will be considered had an impact on the decision. As a consequence, the reduction of a product to a linear combination of units was excluded since in this case a product of two units would be equal to a sum of basic units (with some numerical coefficients). The same was applied to relations like the associative law for instance. Only as far as the associative law was concerned did Grassmann break this rule and he considered some associative systems, too.

In order to select from the infinity of possible special multiplications those which seemed to be useful in science, Grassmann formulated three general properties. He formulated these properties as an invariance of the equations of determination towards some transformations of the units of the basis. These transformations were as follows:

(1) The change of the sign of a relative unit or the change of the signs of two units and the permutation of those units with each other.

(2) The substitution of any two units by two extensive magnitudes, which are formed by those units but which are different from those units as

[8] For an extended discussion of that problem as well as Grassmann's contributions to the theory of hypercomplex number systems cf. [Schlote, 1985].

well as a multiple of them.

(3) The substitution of any unit by any extensive magnitude formed by these basic units.

The multiplications satisfying the first, the first as well as the second, and all properties were called symmetrical, circular and linear, respectively. Despite the restrictions, Grassmann distinguished between sixteen different types of symmetrical multiplications and analysed them. Applying these examples he was able to demonstrate how he perceived the realization of his principles. Among others, he deduced the equations of determination for all these different kinds of multiplication. These equations were obtained by a combination from the following ones:

$$\begin{aligned}
&\mathbf{e}_r * \mathbf{e}_s = \mathbf{e}_s * \mathbf{e}_r,\\
&\mathbf{e}_r * \mathbf{e}_s + \mathbf{e}_s * \mathbf{e}_r = 0,\\
&\mathbf{e}_1 * \mathbf{e}_1 = \mathbf{e}_2 * \mathbf{e}_2 = \cdots = \mathbf{e}_n * \mathbf{e}_n,\\
&\mathbf{e}_1 * \mathbf{e}_1 + \mathbf{e}_2 * \mathbf{e}_2 + \cdots + \mathbf{e}_n * \mathbf{e}_n = 0.
\end{aligned}$$

Grassmann continued by separating the circular multiplications and, finally, the linear multiplications from the deduced 16 kinds. The four linear ones he characterized as being of the utmost importance for analysis, geometry, mechanics and physics. Among the linear products there are some of particular interest: the algebraic product which generates a commutative algebra (in general, the algebra is assumed to be associative); and the combinatorial product or outer multiplication from which a non-commutative algebra was derived (later called a Grassmann algebra by W.C. Clifford). The equations are

$$\mathbf{e}_r * \mathbf{e}_s = -\mathbf{e}_s * \mathbf{e}_r \quad \text{and} \quad \mathbf{e}_r^2 = 0.$$

The circular multiplication also encloses some further remarkable products which were not linear, the inner product or inner multiplication and the complex one. The first was known from its geometrical applications and Grassmann refered to his "Ausdehnungslehre" and his "Geometrische Analyse" where he extensively used this product. The latter one was a generalization of the multiplication of complex numbers, a fact which was also pointed out by Grassmann. The defining equations were

$$\begin{aligned}
&\mathbf{e}_r * \mathbf{e}_s = 0 \quad \text{and} \quad \mathbf{e}_1 * \mathbf{e}_1 = \mathbf{e}_2 * \mathbf{e}_2 = \cdots = \mathbf{e}_n * \mathbf{e}_n,\ r \neq s, \quad \text{respectively}\\
&\mathbf{e}_r * \mathbf{e}_s = \mathbf{e}_s * \mathbf{e}_r \quad \text{and} \quad \mathbf{e}_1 * \mathbf{e}_1 + \mathbf{e}_2 * \mathbf{e}_2 + \cdots + \mathbf{e}_n * \mathbf{e}_n = 0.
\end{aligned}$$

Again, there is an analogy to Wessel's work. Both Wessel and Grassmann referred to a useful application of their ideas in the theory of functions. However, Wessel's remark was only a very vague suggestion. It is difficult to estimate how far they went in this direction or whether they had concrete important results in mind. In the final analysis, a lasting value can be seen in the attempt to stimulate some parts of mathematics which were to constitute a dominating feature in the development of mathematics in the nineteenth century: the theory of complex functions and the theory of functions of several variables, respectively.

4 A comparison of Wessel's and Grassmann's work

The analysis of Wessel's and Grassmann's work on hypercomplex numbers has revealed several similarities. With regard to their mathematical research they had a comparable fate. Their ideas were not appreciated by their contemporaries, and the importance as well as the implications of the new methods was underestimated. Whereas Wessel published his article as a man of mature age, Grassmann was a young man at the beginning of his career. The failure of Wessel's essay to find an audience did not influence his professional career, while Grassmann suffered from the lack of acceptance of his ideas. His professional career was also affected by this. During his life he fought for the diffusion of his method and for an appropriate assessment of it but only with little success near the end of his life.

From the mathematical point of view the investigations of both mathematicians can be integrated into the Leibnizian programme to look for a geometrical calculus. At first Wessel and Grassmann did not see themselves in the Leibnizian tradition. Later however, Grassmann perceived it and considered the theory of extensions as a solution of the Leibnizian task. He drew up the theory from this point of view in the article "Geometrische Analyse" (The geometrical analysis) [Grassmann, 1847]. Of course, there were many other factors which had an effect on the work of both mathematicians and which were quite different. Nevertheless, the search for a geometrical calculus was a common characteristic. In addition, there are some similarities in the applications of their methods they searched for, in geometry for instance, but also in other parts of mathematics. In many ways Grassmann's work can be seen as a continuation and broadening of Wessel's ideas without any direct reference of Grassmann to Wessel's essays. The topics they discussed were of interest at their time, and both men provided a number of solutions which were ahead of their time. Their mathematical achievements had erroneously been marginalized for a long time, but they were never outsiders in mathematical research. On the contrary, Caspar Wessel and Hermann Günther Grassmann had a profound knowledge and understanding of mathematical research problems. Their contributions to mathematics were durable, and they are still appreciated in our time.

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Julius Petersen, Karl Weierstrass, Hermann Amandus Schwarz and Richard Dedekind on Hypercomplex Numbers

Jesper Lützen *

Abstract

Caspar Wessel's paper *On the Analytical Representation of Direction* was the first Danish-Norwegian contribution to the field of hypercomplex numbers. The present paper is devoted to the second Danish contribution to this field: the papers by Julius Petersen. These contributions that were marred by flaws and unrecognized repetitions of earlier results are analysed in the context of the contemporaneous work of Weierstrass and Dedekind. This brings out the central position of H.A. Schwarz. As a side issue it is discussed how diverging interpretations of a quote by Gauss resulted in a polemic between Weierstrass and Dedekind.

1 Petersen and Danish Mathematics at the end of the 19th Century

While astronomy and geodesy flourished in Denmark-Norway around Wessel's time Wessel's own, then overlooked paper, was the only valuable Danish and Norwegian contribution to mathematics. After Erasmus Bartholin (1625–98) and Georg Mohr (1640–97) Danish mathematics only reached an international level around 1880 with the works of H.G. Zeuthen (1839-1920) on enumerative geometry and history of mathematics as well as those of

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Julius Petersen (1839–1910), T.N. Thiele (1838–1910) and later J.P. Gram (1850–1916) and L.V.W.V. Jensen (1859–1925). Julius Petersen, who was Zeuthen's former schoolmate and lifelong friend, was a docent at the Polytechnic Highschool in Copenhagen from 1871 to 1887 when he got a chair at the University of Copenhagen. He was famous (infamous) to Danish schoolchildren who almost all used his system of textbooks in mathematics. Internationally he became known at the time as the author of *Methods and Theories for the Solution of Problems of Geometric Constructions* [1866/79], the best selling mathematics book ever written by a Danish mathematician, translated into innumerable languages and republished as late as 1990. His book on the theory of equations [1877] and his lectures on mechanics [1881,84,87] also enjoyed wide fame. In recent years he has become famous mainly through his pioneering contributions to graph theory, cryptography and mathematical economy.

His works were characteristic through their concise, elegant, often geometric intuitive style, which was not overburdened by details and sometimes passed over major problems without noticing it. The way he did research had several connected characteristics: 1° he rarely read the works of other mathematicians, 2° he was a problem solver, not a builder of theories, 3° he only pursued an idea a little way (until he had solved his problem) and then dropped it. This had the unfortunate consequence that many of his "discoveries" turned out not to be new and his genuinely new ideas had little influence on his contemporaries because he did not pursue them far enough.

In this paper I shall illustrate many of these points in a discussion of Petersen's research on hypercomplex numbers. More information about Petersen and his work can be found in [Lützen, Sabidussi and Toft 1992] and [Christiansen et al 1992].

2 Petersen on the Foundation of Mathematics

Petersen displayed his first interest in hypercomplex numbers in 1883 in connection with a discussion on the nature of mathematical axioms that took place in the Danish journal *Tidsskrift for Mathematik*, also called *Zeuthen's Journal*. Compared to other participants in the discussion who argued that axioms were based on experience or were a priori in Kant's sense, Petersen's view was extremely modern:

> 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 1883, 3–4]

Petersen illustrated this view of axioms by introducing a model of a non-Euclidean geometry (the so-called Riemannian or single elliptic geometry in Klein's terminology). He added the model of hyperbolic geometry published by Poincaré the previous year in *Acta Mathematica* [Poincaré 1882], but he seems to have been unaware of Beltrami's work as well as of Klein's more farreaching work from 1871 on the "so-called non-Euclidean geometry" [Klein 1871]. However, he soon learned about it. Indeed, a highschool teacher from Schleswig-Holstein, Rudolph von Fischer-Benzon (1839–1911), who over the years translated many books and papers by Petersen into German, also translated this paper and sent it to Felix Klein (1849–1925) for publication in the *Mathematische Annalen.* Klein did not find much new in Petersen's paper:

> The comparison with Poincaré's geometry is old. The same applies to C_3 [Petersen's projective model]. The use of the linebundle is also due to me as well as the criticism of Legendre's proofs. What is now left for Petersen? Many interesting specific points. Then the exposition of the general principles which are indeed creditably clear. [Klein's remark in the margin of a letter from Fischer-Benzon to Klein, November 6, 1886]

In the end Klein decided that the new observations and the clarity of the exposition warranted that the paper be printed in the *Mathematische Annalen* provided that Petersen added references to the relevant literature that he had originally ignored.

3 Petersen's 1885 Paper on the Basic Concepts of Algebra

Petersen's 1883 paper also contained a few remarks concerning the basic concepts of algebra. He returned to this subject two years later in a talk to the Royal Danish Academy of Sciences and Letters, of which he had become a member in 1879. The paper was printed in *Tidsskrift for Mathematik* the same year [Petersen 1885]. Petersen defined algebra as a theory of sign language with the following characteristics:

1) The language must contain signs that designate the elements in one or more sets (Petersen used the word "group" in the sense "set" or "algebraic structure"). The meaning of the signs is only determined when the algebraic theory is applied.
2) The language must contain signs for operations.
3) The language contains an equality sign.
4) The language has certain arbitrarily chosen basic characteristic equations.

This characterization of algebra and Petersen's description of isomorphic structures again seem very modern in view of the fact that by 1885 the development of algebra as the science of algebraic structures was hardly visible. However, it is in fact unclear precisely how generally Petersen considered his characterization. As one among the commonly chosen "basic equations" Petersen mentioned the commutative law: $a + b = b + a$, but he did not introduce the axiomatic definition of a group (in the modern sense of that word). In his algebra book from 1877 groups were defined as permutation groups. In fact, in order to illustrate what he had in mind, Petersen did not mention any of the axiomatic structures that were taking shape around this time such as group or field (cf. e.g. [Wussing 1984]), but he turned to hypercomplex numbers.

3.1 Complex Numbers

He first introduced ordinary complex numbers in an unorthodox projective way: Let L be a given straight line and O a given point outside the line. Petersen now defined the sum C of two points A and B in the half plane containing O by the construction in figure 1. He claimed without proof that this definition of addition is commutative and associative and has O as a neutral element (called 0). Moreover, the points of L are infinite in the sense that they are not changed if another point is added to them. Although addition of two points on the same line through O is not directly defined by the above construction, Petersen hinted that it can be defined by a limiting

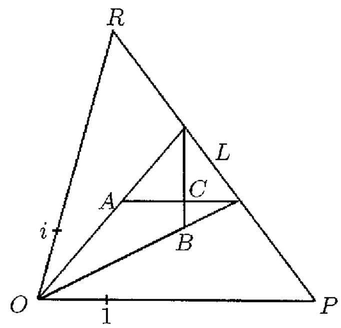

Figure 1

process or by first adding a point outside the line and afterwards subtracting it. In this way one can define $k \cdot a$, where a is a point in the half plane and $k \in \mathbb{N}$ by first setting $a + a = 2a$, $2a + a = 3a$ etc: "in the usual way we can extend to fractional and irrational values of k" [Petersen 1885, 4]. Now Petersen introduced two arbitrary but fixed half lines OP and OR and chose a point called 1 and i on them. Every point of the half plane can now be decomposed as a sum of a point on OR and a point on OP and therefore has the form $a \cdot 1 + b \cdot i, a, b \in \mathbb{R}$ or as Petersen wrote $a + bi$. He did not remark that he had here confused the real number 1 and the point 1 on OR. He remarked that if one chooses L at infinity, OR perpendicular to OP and $Oi = O1$ then one would get the usual description of the complex numbers. That this usual description had been presented by Wessel to the same learned society to whom Petersen addressed his talk was unknown to him as well as to all his contemporaries.

Finally Petersen introduced multiplication. He remarked that in order to "obtain the usual laws of calculation" one must assume that

$$(a + ib)(a_1 + ib_1) = aa_1 + (ab_1 + ba_1)i + bb_1 i^2 \tag{1}$$

(distributativity. associativity and commutativity are used, but Petersen did not specify which "laws of calculation" he specifically referred to). Moreover, in order for this to make sense (for the "group" to be closed under multiplication) i^2 must be of the form $\alpha + i\beta$, $\alpha, \beta \in \mathbb{R}$. "It is most natural to put $i^2 = -1$" he continued without explaining in what sense this choice was the most natural; nor did he give a geometric explanation of the resulting multiplication. Instead he argued that just as lines through O are "closed subgroups" under addition and subtraction, so the ellipse

$\{x = a + ib \mid a^2 + b^2 = 1\}$ is the only infinite "subgroup" with respect to multiplication and division. How he could miss the positive real axis, i.e. the half line OP (with O left out), is odd.

3.2 Ternions

Having hinted at the new possibilities opened by such a more general treatment of complex numbers Petersen turned to "ternions". From the outset he wrote a ternion of the form $x + \alpha y + \beta z$, where $1, \alpha, \beta$ are points on the three "axes" (probably orthogonal) and $x, y, z \in \mathbb{R}$. Addition is defined by the parallelogram rule (vector addition). In order to define a multiplication that satisfies the "usual rules" (only commutativity is mentioned explicitly) we must define α^2, $\alpha\beta$ and β^2, i.e. we must choose a_i, b_i, c_i, $(i = 1, 2, 3)$, so that

$$\begin{aligned} \alpha^2 &= a_1\alpha + b_1\beta + c_1, \\ \alpha\beta &= a_2\alpha + b_2\beta + c_2, \\ \beta^2 &= a_3\alpha + b_3\beta + c_3. \end{aligned} \tag{2}$$

In fact one should also define 1^2, 1α and 1β where 1 is the point chosen on the first axis. However, since Petersen confused this with the real number 1 he implicitly assumed that $1^2 = 1$, $1\alpha = \alpha$ and $1\beta = \beta$.

Petersen remarked that one cannot chose a_i, b_i, c_i arbitrarily; it is required that expressions of the form $\alpha^m\beta^n$ always give the same result independently of the order. He did not explicitly state if there were other requirements that had to be fulfilled, nor did he spell his one condition out as an explicit condition in a_i, b_i and c_i because it "is rather complex". However, he stated that the requirement could be satisfied in "different ways" and went on to consider "one of the simplest", namely:

$$\alpha^2 = \beta, \quad \beta^2 = \alpha, \quad \alpha\beta = 1. \tag{3}$$

As in the two-dimensional case Petersen then sought "groups" that are closed under multiplication and division. He found the surface

$$x^3 + y^3 + z^3 - 3xyz = 1 \tag{4}$$

as well as the "null surface"

$$x + y + z = 0 \tag{5}$$

and the "null line"

$$x = y = z\ . \tag{6}$$

The null surface and the null line have the additional property that a product lies in the null surface (null line) when one of the factors lies in the null surface (null line). This means that the product of a point in the null surface and a point on the null line is zero. Thus, there exist zero divisors, and division is not always uniquely defined. The null line and the null surface span the whole space (they are orthogonal in this case) and so any ternion can be decomposed as a sum of a point ℓ on the null line and a point p on the null plane. Expressed in this way multiplication can be expressed in the simple form:

$$(\ell + p)(\ell' + p') = \ell\ell' + pp'.$$

Here ℓ and ℓ' multiply as real numbers and Petersen showed with some geometric considerations that points in the null plane multiply like complex numbers.

Finally he argued (§12) that an n'th degree ternion equation has less than n^2 solutions. However, that is not correct. Indeed, if the polynomial equation

$$bx + \cdots + hx^m = 0 \tag{7}$$

has coefficients that all belong to the null plane (or all belong to the null line) then all the infinitely many points on the null line (null plane) will solve the equation. More generally, if all the coefficients of

$$a + bx + \cdots + hx^m = 0 \tag{8}$$

are products of the same null divisor k:

$$a = ka'\ , \ b = kb'\ , \dots\ , h = kh' \tag{9}$$

then if $a', b', \dots, h'$ are chosen such that

$$a' + b'x + \cdots + h'x^n = \ell \tag{10}$$

has solutions for infinitely many values of ℓ for which $k \cdot \ell = 0$, then all these infinitely many solutions will solve the original equation. This consequence of the existence of null divisors played a major role in Weierstrass's discussion of hypercomplex numbers (see below), but it was entirely overlooked by Petersen.

In the last section of his paper Petersen discussed quaternions, and from the start he chose Hamilton's quaternions. Of course he mentioned the lack of commutativity and he mentioned the uniqueness of (right and left) division by non-zero quaternions. Otherwise he did not explicitly stress that in the four dimensional case he had made a choice different from the one in three dimensions. He had sacrificed commutativity but avoided null divisors.

3.3 General Evaluation

Petersen's 1885 paper is typical of his production. It is elegant, geometrically intuitive, not overburdened with rigour, flawed in some details and without a single reference. It is in fact unclear whether he knew of Weierstrass's paper on hypercomplex numbers from 1884 or Schwarz's additions from the same and the following year. Dedekind's paper of 1885 may not have appeared when Petersen gave his talk. If Petersen did not know of Weierstrass's paper one must grant him that particularly the section on ternions reveals some original insights. However, even then it is characteristic that Petersen did not dig deeper. For example he did not ask if there was anything special about the choices he made in the two, three and four dimensional cases. The answer is that the choice of $i^2 = -1$ in the two-dimensional case and Hamilton's relations between i, j and k are very special giving the complex numbers, and Hamilton's quaternions specially nice algebraic properties. Petersen's choice: $\alpha^2 = \beta$, $\beta^2 = \alpha$, $\alpha\beta = 1$ in the ternion case on the other hand, gives no special results that one cannot obtain with all "suitable" other choices. It is characteristic of Petersen that he did not make this discovery, and that he did not initially investigate if his arguments could be generalized to higher dimensional commutative n-ions.

Fischer-Benzon also translated this paper by Petersen and sent it in 1886 to Klein for publication in *Mathematische Annalen*, but this time Klein refused. His reason was probably that Petersen's paper could at best be seen as a simple exemplification of some of the results in Weierstrass's paper in *Göttinger Nachrichten* of 1884.

4 The Background to Weierstrass's 1883–84 Paper

According to his own testimony [Weierstrass 1884, 396] Karl Weierstrass (1815–1897) lectured on hypercomplex numbers as early as the winter semester 1861–62,[1] but his results were not published until 1884 on the initiative of Hermann Amandus Schwarz (1843–1921). Schwarz had heard Weierstrass's lectures on hypercomplex numbers and had later tried (probably in 1877) to reconstruct their contents. However, he got stuck at a particular detail in the argument: Weierstrass had assumed that for a domain of n-dimensional hypercomplex numbers there exists an element g such that $e, g, g^2, g^3, \dots, g^{n-1}$ spans the space where e is the neutral element under multiplication. Now Schwarz was unable to prove the theorem that there exists such an element g in each domain of hypercomplex numbers.[2] He turned to Weierstrass, but as he recalled in 1883:

> I have already once talked with you (Weierstrass) about this point, but you had just mentioned other things that were in the foreground of your interests, and so we did not discuss this question in detail. [3]

However, Schwarz did not doubt the truth of the theorem until 1883 when he visited Paris and met the Greek mathematician Kyparissos Stephanos, who gave him a simple example of the three-dimensional system of hypercomplex numbers, where the theorem fails. Stephanos's numbers were of the form

$$x = \alpha_0 e_0 + \alpha_1 e_1 + \alpha_2 e_2\,, \quad \alpha_i \in \mathbb{R}, \tag{11}$$

where e_0, e_1, e_2 are the three basic units of which e_0 is the neutral element under multiplication and the other elements multiply according to the simple rules:

$$e_1 e_1 = 0, \quad e_1 e_2 = 0, \quad e_2 e_2 = 0. \tag{12}$$

[1] I have not been able to locate any lecture notes from this lecture which Weierstrass had to interrupt halfway through the term due to a breakdown.

[2] Schwarz's attempts at reconstructing Weierstrass's theory on the basis of lecture notes of 1863/64 and 1865/66 are preserved in [Ms 1] dated May 15th 1877. Schwarz proved the existence of a unit element, but when he arrived at the problem of the existence of g his notes degenerated into a series of seemingly unfruitful examples.

[3] "Ich habe schon einmal mit Ihnen über diesen Punkt gesprochen, aber Sie hatten gerade andere Gegenstände, welche im Vordergrunde Ihrer Interesse standen, angeregt und so unterblieb ein genaueres Eingehen auf diesen speciellen Fragepunkt". [Schwarz to Weierstrass May 9, 1883].

In this case

$$x^2 = 2\alpha_0(\alpha_0 e_0 + \alpha_1 e_1 + \alpha_2 e_2) - \alpha_0^2 e_0 \tag{13}$$

so that

$$x^2 - 2\alpha_0 x + \alpha_0^2 e_0 = 0, \tag{14}$$

which shows that e_0, x, x^2 are linearly dependent, so that they do not span the space, no matter how x is chosen.[4]

When Schwarz returned from Paris he wrote a letter to Weierstrass (from which I have drawn the above information) informing him about Stephanos's counterexample and concluded:

> Now arises the question: which theorem should replace it? Do the consequences that you have drawn from the above theorem e.g. "in a number domain with an uneven number of units there exists no number whose square is equal to $-e_0$" still hold true or are they subject to a modification?
>
> It would really be rather horrible if one had to add the following qualification to all those theorems. "There are cases where this or that holds true; however it does not always occur".
>
> Which modification applies to Hazzidakis's theorem?[5] [Schwarz to Weierstrass, May 9 1883]

[4] Kyparissos (or Cyparissos) Stephanos (1857–1917) studied mathematics in Paris until 1884 when he returned to Athens to become a professor at the University and the Technical Highschool. While in Paris he communicated his counterexample to Schwarz on a sheet of paper [Ms 2] preserved in the Schwarz Nachlass. Later (in 1888) he published an 11 page paper in Greek on "Systems of complex numbers with several symbolic units" in the jubilee publication celebrating the 50th anniversary of the University of Athens, and at the first International Congress of Mathematicians he gave a talk "Sur les systèmes associatifs de nombres symboliques" (see the abstract in the "Verhandlungen des ersten internationalen Mathematiker-Kongresses", Leipzig 1898 pp. 141–142.) In this talk he reduced the study of hypercomplex numbers to the study of trilinear forms. Many of his other publications, most in French journals, concern binary and ternary forms.

[5] Johannes Hazzidakis (or Hatzidakis) (1844–1921) studied mathematics in Germany from 1870 to 1873. Afterwards he was first a professor at the Military School in Athens and then in 1884 at the University of this town, at the same time as his compatriot Stephanos. He published several papers in Crelle's *Journal*, but apparently nothing on hypercomplex numbers. His role in the story is therefore not quite clear (see however the end of §4.1 below.)

Weierstrass answered Schwarz in a long letter that he began on May 19th and finished on June 27th [6] 1883, a letter that Schwarz subsequently read to the *Königliche Gesellschaft der Wissenschaften zu Göttingen* on December 1st of the same year and published the following year in the *Göttinger Nachrichten*, [Weierstrass 1884].

The short version of Weierstrass's answer runs as follows. He had early on been aware of the problem, not as a result of the example given by Stephanos, but as a result of an analysis of the noncommutative case, where the quaternions give a similar counterexample. However, he had not explicitly called attention to this circumstance in his lectures prior to 1882–83 except for showing that it did not occur "in general".[7] In his 1882–83 lectures he had more explicitly stated the condition that had to be fulfilled to avoid the problem, but he had not had the time to explain why this condition was necessary, in Weierstrass's opinion, in order to have a domain of hypercomplex numbers that is algebraically interesting at all.

This direct answer to Schwarz's question concludes Weierstrass's letter to Schwarz [Weierstrass 1884, 411–414]. In the first main part of the letter Weierstrass aimed at giving

> ... an authentic account of my theory of the complex quantities; the notes, even by the best in my audience contain many misunderstand-

[6]In the published version [Weierstrass 1884] the date is given as "19.–27. June". That does not correspond to the dates given in the original letter.

[7]In his lectures of 1863/64 (only two years after his first treatment of hypercomplex numbers) Weierstrass does not seem to have mentioned the problem at all. At least in the lecture note [Ms 3] the tricky point is passed over in silence. Having remarked that for any hypercomplex number x, the numbers $e, x, x^2, \ldots, x^\rho$ are linearly dependent (cf. (30) below) when ρ is the dimension (called n below) he simply stated: "Wenn ich irgend eine Zahl x als Einheit einführe und x mit der Potenze; Jede Zahl auf die Form zu bringen:

$$a = \alpha e^0 + \alpha_1 e^1 + \alpha_2 e^2 + \cdots \alpha_{\rho-1} e^{\rho-1}."$$

In English translation: "When I introduce an arbitrary number x as unit and x as its powers; To represent every number on the form

$$a = \alpha e^0 + \alpha_1 e^1 + \alpha_2 e^2 + \cdots \alpha_{\rho-1} e^{\rho-1}."$$

To be sure this passage is somewhat obscured by its odd grammatical structure and by the change of notation (x is called e in the formula), but it is obvious that the student who took the notes did not detect any problem concerning the existence of x (or e or g in Weierstrass's later notation). The lecture notes break off only half a page after this introduction of e.

> ings — with or without the fault of the docent — namely in places that go beoynd mere reproduction of calculations.[8]

This account was devoted to showing that a strong structure theorem about hypercomplex numbers will hold in general, and to show that in the special cases where it does not hold, non trivial algebraic equations can have infinitely many solutions, which made such algebras too unlike the real numbers to be of any importance.

4.1 Weierstrass's Theory of Hypercomplex Numbers

I shall summarize Weierstrass's argument at some length, not only because it is of interest in itself, but also because it sets the mathematical and historical background for both Petersen's and Dedekind's later papers.

Weierstrass considered a space (he called it a "Gebiet" (domain)) of n-dimensional hypercomplex quantities of the form

$$\xi_1 e_1 + \xi_2 e_2 + \cdots + \xi_n e_n \tag{15}$$

where $\xi_1, \ldots, \xi_n \in \mathbb{R}$ and $e_1, e_2, \ldots, e_n$ are n independent basic units. I shall call this space C_n. He explicitly stated that the space should be closed under addition, subtraction and multiplication as well as under division if it is defined (see below), and he explicitly set down the following axioms:

$$\left.\begin{array}{rcl} a+b &=& b+a \\ (a+b)+c &=& (a+c)+b \\ (a-b)+b &=& a \end{array}\right\} \quad (16) \qquad \left.\begin{array}{rcl} a\cdot b &=& b\cdot a \\ (ab)c &=& (ac)b \\ a(b+c) &=& ab+ac \\ \frac{a}{b}\,b &=& a \end{array}\right\} \quad (17)$$

(remark the strange mix of the associative and the commutative laws.)

Weierstrass then claimed that it followed from the first set of axioms that addition and subtraction are performed componentwise, i.e.

$$\text{if} \quad a = \sum_{i=1}^{n} \alpha_i e_i \quad \text{and} \quad b = \sum_{i=1}^{n} \beta_i e_i \tag{18}$$

$$\text{then} \quad a \pm b = \sum_{i=1}^{n} (\alpha_i \pm \beta_i) e_i. \tag{19}$$

[8] A sad view from a mathematician, many of whose ideas became known through his students!

Here he has implicitly used that the e_i's themselves can be considered as hypercomplex numbers and that the "+" in the expressions (18) for a and b is the same as the composition used to add two hypercomplex numbers. Moreover he has assumed that the "multiplication" with the real numbers α_i satisfies the distributive law

$$\alpha_i e_i + \beta_i e_i = (\alpha_i + \beta_i)e_i. \tag{20}$$

Weierstrass did not explicitly define a multiplication of a general hypercomplex number with a real number, but if we do so in the obvious way we can consider C_n as a real vector space. I shall therefore somewhat anachronistically use vector space terminology in the following discussion.

Implicitly using that $\alpha_j e_j \cdot \beta_k e_k = \alpha_j \beta_k (e_j e_k)$ Weierstrass further concluded that if a and b are given as in (18) their product is given by

$$a \cdot b = \sum_{j,k=1}^{n} \alpha_j \beta_k (e_j e_k). \tag{21}$$

Therefore the multiplication is completely determined once $e_j \cdot e_k$ has been determined for all $j, k = 1, 2, \ldots, n$. If ε_{ijk} denote the coordinates of $e_j e_k$ in the basis $\{e_i\}$, i.e. if

$$e_j \cdot e_k = \sum_{i=1}^{n} \varepsilon_{ijk} e_i \tag{22}$$

then the n^3 real numbers ε_{ijk} completely determine the multiplication.

The central exercise is to find the conditions that the ε_{ijk}'s must satisfy in order to define acceptable multiplications. First Weierstrass pointed out that the commutative and "associative" laws (17) will determine a number of constraining equations. These equations, namely

$$\textbf{First constraining requirement} \left\{ \begin{array}{rcll} \varepsilon_{ijk} & = & \varepsilon_{ikj} & i, j, k = 1, 2, \ldots, n, \qquad (23) \\ \displaystyle\sum_{k=1}^{n} \varepsilon_{ijk}\varepsilon_{krs} & = & \displaystyle\sum_{k=1}^{n} \varepsilon_{isk}\varepsilon_{krj}, & i, r, s = 1, 2, \ldots, n, \qquad (24) \end{array} \right.$$

were not written out by Weierstrass, but Dedekind [1885, 145] later did.

In order to define the quotient $\frac{a}{b} = \sum_{i=1}^{n} \gamma_i e_i$ such that $\frac{a}{b} \cdot b = a$, the coordinates γ_i must satisfy the equation

$$\alpha_i = \sum_{j,k=1}^{n} \varepsilon_{ijk} \beta_j \gamma_k. \tag{25}$$

This is possible in a unique way if the determinant

$$\varepsilon(b) = \left| \sum_{j=1}^{n} \varepsilon_{ijk} \beta_j \right| \tag{26}$$

is different from zero. If it is zero for a given b there are either zero or infinitely many solutions $\gamma_1, \ldots, \gamma_n$ to (25), depending on the value of a. Now Weierstrass claimed that for $n > 2$ one cannot define a unique division for every $b \neq 0$. Indeed this is a consequence of the following analysis. However, he required that a unique division can be defined for some values of b. This amounts to requiring that $\varepsilon(b)$, which is a homogeneous function of degree n in the β's, is not identically zero. This is his **second constraining requirement** on the numbers ε_{ijk}.

If both requirements are satisfied, Weierstrass claimed that there exists a unit element $e_0 \in C_n$ such that $e_0 \cdot a = a$ for all $a \in C_n$. Schwarz included his 1877 proof (see footnote 2) of this fact in an appendix.

Now a third constraining requirement was made to answer the problem Schwarz had struggled with: Let

$$x = \xi_1 e_1 + \xi_2 e_2 + \cdots + \xi_n e_n \tag{27}$$

denote an arbitrary element of C_n. Then its powers can be expressed in the form

$$x^\nu = \xi_1^{(\nu)} e_1 + \xi_2^{(\nu)} e_2 + \cdots + \xi_n^{(\nu)} e_n. \tag{28}$$

Weierstrass made the **third constraining requirement** that the determinant

$$X_0 = \left| \xi_\mu^{(\nu)} \right| \quad \mu, \nu = 1, 2, \ldots, n \tag{29}$$

is not identically zero. He claimed, and Schwarz proved in an appendix, that this would indeed be the case except for "special values of the system ε_{ijk}".

Under this assumption we can therefore choose a quantity $g = \xi_1 e_1 + \xi_2 e_2 + \cdots + \xi_n e_n$ for which $X_0 \neq 0$. Then $e_0, g, g^2, \ldots, g^{n-1}$ will be a new basis of C_n as Schwarz had wanted.

Moreover, since $e_0, g_1, \ldots, g^n$ will obviously be linearly dependent g must satisfy a polynomial equation of the form

$$f(g) = g^n + \varepsilon_1 g^{(n-1)} + \varepsilon_2 g^{(n-2)} + \cdots + \varepsilon_o e_0 = 0. \tag{30}$$

Weierstrass then made the **fourth** and last **constraining requirement** on the ε_{ijk}'s, namely that this equation, considered as an equation in $\mathbb{C}$, has n *different* solutions. That amounts to saying that its discriminant D^2 is different from zero.

Now the field is laid out for the proof of the main structure theorem: First Weierstrass remarked that multiplying in the basis $e_0, g^0, g^1, g^2, \ldots, g^{n-1}$ corresponds to multiplying polynomials modulo $f(x)$ where f is the polynomial in (30). More precisely: to a hypercomplex number

$$a = \sum_{i=0}^{n-1} \alpha_i g^i \tag{31}$$

we associate the unique polynomial

$$\varphi_a = \sum_{i=0}^{n-1} \alpha_i x^i. \tag{32}$$

Then

$$a \cdot b = c \tag{33}$$

if and only if

$$\varphi_a \cdot \varphi_b \equiv \varphi_c \mod f. \tag{34}$$

Moreover, the classical result of decomposition in unit fractions states that if φ is a polynomial of degree less than n then $\frac{\varphi(x)}{f(x)}$ can be decomposed uniquely in the form

$$\frac{\varphi(x)}{f(x)} = \frac{\varphi_1(x)}{f_1(x)} + \frac{\varphi_2(x)}{f_2(x)} + \cdots + \frac{\varphi_r(x)}{f_r(x)}, \tag{35}$$

where $f_1, f_2, \dots, f_r$ are the real irreducible linear or quadratic factors of f (that by the fourth requirement are all diferent) and where $\varphi_1, \varphi_2, \dots, \varphi_r$ are real numbers or first degree polynomials (of degree less than their respective denominators).

That means that if f_μ is of the first degree, the subspace G_μ of hypercomplex numbers associated (by the association (31) $\leftrightarrow$ (32)) with polynomials of the form

$$\frac{f(x)}{f_\mu(x)}\varphi_\mu(x) \tag{36}$$

is one-dimensional and spanned by the hypercomplex numbers associated with $\frac{f(x)}{f_\mu(x)}$. On the other hand if f_μ is a quadratic polynomial the subspace G_μ of hypercomplex numbers associated with polynomials of the above form (36) is two-dimensional and spanned by the hypercomplex number associated with $\frac{f(x)}{f_\mu(x)}$ and $\frac{f(x)}{f_\mu(x)} \cdot x$.

Since any hypercomplex number is associated with a polynomial φ of degree less than n and since $\varphi(x) = \frac{\varphi(x)}{f(x)} \cdot f(x)$, equation (35) shows that any hypercomplex number can be written uniquely as a sum of elements in $G_1, G_2, \dots, G_r$. In modern language $C_n = G_1 \oplus G_2 \oplus \cdots \oplus G_r$.

Elementary calculations with polynomials modulo f now show that if $a \in G_i$ and $a' \in G_j$ then

$$a \cdot a' = 0 \quad \text{if } i \neq j, \tag{37}$$

but

$$a \cdot a' \neq 0 \quad \text{if } i = j \quad \text{unless } a = 0 \text{ or } a' = 0. \tag{38}$$

Assume for the moment that all the subspaces G_i are one-dimensional and write $e_0 = g' + g'' + \cdots + g^{(n)}$ where $g^{(i)} \in G_i$. Then it is easy to see that

$$(\alpha g^{(\mu)})(\beta g^{(\mu)}) = (\alpha\beta) g^{(\mu)}. \tag{39}$$

That means that if we use $g^{(1)}, \dots, g^{(n)}$ as a new basis for C_n multiplication is simply reduced to componentwise multiplication:

$$\sum_{i=1}^{n} \alpha_i g^{(i)} \cdot \sum_{i=1}^{n} \beta_i g^{(i)} = \sum_{i=1}^{n} (\alpha_i\beta_i) g^{(i)}. \tag{40}$$

With a little more effort Weierstrass was able to prove that it is possible to pick bases $g^{(\mu)}, k^{(\mu)}$ in the two-dimensional subspace G_μ such that multiplication of two elements of G_μ is done as follows:

$$(\alpha g^{(\mu)} + \alpha' k^{(\mu)})(\beta g^{(\mu)} + \beta' k^{(\mu)}) = (\alpha\beta - \alpha'\beta')g^{(\mu)} + (\alpha'\beta + \alpha\beta')k^{(\mu)} \quad (41)$$

i.e. they multiply exactly as the two complex numbers $\alpha+i\alpha'$ and $\beta+i\beta'$. We can therefore think of the two-dimensional subspaces G_i as one-dimensional complex vector spaces.

Thus Weierstrass had proved the

Main Structure Theorem. *If $\{\varepsilon_{ijk}\}$ satisfies the four constraining requirements one can decompose C_n as a direct sum of one or two dimensional subspaces:*

$$C_n = G_1 \oplus G_2 \oplus \cdots \oplus G_r \quad (42)$$

such that all algebraic computations involving addition, subtraction, multiplication and division are done componentwise, and such that one computes with the components in each G_i as with real numbers (in the one-dimensional subspaces) or with complex numbers (in the two-dimensional subspaces). Or expressed in a more modern way C_n is isomorphic to a direct sum of copies of the real and complex numbers with multiplication defined componentwise.

This theorem is a complete and general higher dimensional analogue of the theorem found (later) by Petersen for his specific choice of ternions. We remark that any hypercomplex number with a zero component in at least one of the subspaces G_i will be a zero divisor because it will give zero if multiplied with $g^{(i)}$. Thus as a special corollary Weierstrass has established that if the dimension of C_n is higher than two (so that there will be at least two terms G_i in the sum $\oplus G_i$) there must necessarily be zero divisors. [9]

This theorem, showing that there do not exist hypercomplex number*fields* of dimension higher than 2, is often attributed to Weierstrass (see e.g. [Kline 1972, 793]). It is however worth mentioning that Hermann Hankel (1839–1873) had already published a proof of the theorem in his book *Theorie*

[9]Strictly speaking the above argument only leads to this conclusion if $\{\varepsilon_{ijk}\}$ satisfies all the four requirements. However, he also showed that if the last requirement is not fulfilled, there is a λ such that $x^\lambda = 0$ has infinitely many solutions. This also gives zero divisors.

der Complexen Zahlensysteme [1867]. Yet Weierstrass did not find it worth mentioning. Did he not know Hankel's book or did he not feel obliged to mention it in a "letter" to a friend and student, since he had derived the theorem long before the appearance of Hankel's book, or did he even feel that Hankel had in some way stolen some of the ideas from his earlier lectures? I cannot say on the basis of the available documents. However, it is equally conspicuous that Hankel did not refer to Weierstrass's lectures in his book despite the fact that he was in Berlin and followed courses by Weierstrass in exactly the year 1861 [Crowe 1972], in which the latter gave his first account of hypercomplex numbers.

Another question of priority concerns Hazzidakis's contribution. Having proved that one can choose a basis in the two-dimensional subspaces G_i such that multiplication corresponds to complex multiplication of the components, Weierstrass added in a footnote:

> As is well known to you Mr Hazzidakis has arrived at the same result in a different way. [Weierstrass 1884, 406]

It is not quite clear how much of the Main Structure Theorem Hazzidakis had proved: Had he proved the whole theorem? This seems unlikely considering the little mention he got. It seems more likely that he had heard of Weierstrass's decomposition $C_n = \oplus G_i$, and had found the special basis in the two-dimensional subspaces G_i, in which the multiplication reveals itself as complex multiplication. The way Schwarz referred to Hazzidakis's theorem in his letter to Weierstrass (see §3) even suggests, that Hazzidakis was the first to see that the two-dimensional subspaces G_i behave like complex numbers.

Weierstrass's first two requirements seem natural, but the last two may seem artificial. However, Weierstrass showed that if one of them is not satisfied, then a polynomial equation

$$a + bx + cx^2 + \cdots + hx^m = 0 \tag{43}$$

can have infinitely many solutions even when its coefficients $a, b, \dots, h$ are not multiples of the same zero divisor. On the other hand he could show that if the requirements are satisfied, a polynomial equation has finitely many solutions except when its coefficients $a, b, c, \dots, h$ are multiples of the same zero divisors. Since the existence of zero divisors cannot be avoided (for $n \geq 3$) this is the closest one can get to the usual fundamental theorem of algebra. Conditions 3 and 4 therefore ensure that we keep as close as possible to ordinary algebra, and they are therefore natural according to Weierstrass.

5 Petersen's 1887 Paper

In 1887 Petersen published his own treatment of general n-dimensional hypercomplex numbers in *Göttinger Nachrichten*. It is phrased as an alternative to Weierstrass's treatment, of which he had become aware in the meantime (probably through Klein or Fischer-Benzon). In fact, much of the treatment follows Weierstrass closely, but there are notable differences. First of all Petersen used complex numbers. Initially he defined hypercomplex numbers as quantities of the form $\xi_1 e_1 + \xi_2 e_2 + \cdots + \xi_n e_n$ where $\xi_i \in \mathbb{R}$. Here he followed Weierstrass. However, when he had defined the quantity g and the polynomial f (30), he used all its real and complex roots $x_1, x_2, \ldots, x_n$ to define a new set of elements: $p_1, p_2, \ldots, p_n$ by:

$$p_i = \frac{f(x)}{f'(x_i)(x - x_i)}; \quad (x = g), \tag{44}$$

where it is understood that one must set $x = g$ after having made the division. Petersen now showed that the new set of points p_i multiplies each other in the following simple way

$$p_i \cdot p_j = \delta_{ij} p_i \quad \text{where } \delta_{i,j} = \begin{cases} 1 & \text{for} \quad i = j, \\ 0 & \text{for} \quad i \neq j. \end{cases} \tag{45}$$

The p-system is also a basis and (42) implies that expressed in this system, multiplication is simply termwise multiplication of coordinates, i.e. if $a_1, a_2, \ldots, a_n$ are a's coordinates and $b_1, b_2, \ldots, b_n$ are b's coordinates in the p-basis, then $a \cdot b$ has the coordinates $a_1 b_1, a_2 b_2, \ldots, a_n b_n$. In the same coordinates the Null set has the equation

$$x_1 x_2 \cdots x_n = 0 \tag{46}$$

and consists of the n $(n-1)$-dimensional subspaces:

$$x_1 = 0,\ x_2 = 0, \ldots, x_n = 0. \tag{47}$$

Petersen then proceeded with the example which he had given in his 1885 paper. However, this time he explicitly mentioned the multiplication laws for the first "basis vector" e_1 (which he had implicitly identified with 1 earlier) and he decomposed the null-surface from 1885 into two complex null-lines. He also generalized his search for "subgroups" invariant under multiplication and concluded that they are all given by an equation of the form

$$x_1^{\alpha_1} x_2^{\alpha_2} \cdots x_n^{\alpha_n} = 1, \tag{48}$$

except the earlier mentioned null-spaces and their intersections. Petersen then went on to analyse Weierstrass's four requirements and first showed that they were independent in the sense that the fulfilment of three of them does not ensure the fulfilment of the last one. He finally found a "surprisingly simple" requirement that he proved to be equivalent to Weierstrass's last three requirements: Consider the equations of condition (22)

$$e_j e_k = \sum_{i=1}^{n} e_{ijk} e_i \tag{49}$$

as an equation in n numbers (complex numbers) $e_1, e_2, \ldots, e_n$. Then Weierstrass's three conditions are satisfied if and only if one can find n sets of solutions

$$e_{i1}, e_{i2}, e_{i3}, \ldots, e_{in}, \quad i = 1, 2, \ldots, n, \tag{50}$$

such that the determinant $|e_{ij}|$ is different from zero. Expressed in vector space language Petersen's argument runs somewhat along the following lines.

First assume Weierstrass's conditions are satisfied. Then the previous argument shows that there exists a basis $p_1, p_2, \ldots, p_n$ in which multiplication is simply coordinatewise multiplication. If we express the original basis vectors $e_1, e_2, \ldots, e_n$ in this system

$$e_j = \sum_{i=1}^{n} e_{ij} p_i, \tag{51}$$

then the original equations of condition separate according to the coordinates into the equations:

$$e_{ij} e_{ik} = \sum \varepsilon_{\ell jk} e_{i\ell}. \tag{52}$$

Thus $e_{i1}, e_{i2}, \dots, e_{in}$ is a set of solutions to (49) for all $i = 1, 2, \dots, n$ and since $\{e_i\}$ is a basis the determinant $|e_{ij}|$ of its coordinate vectors is non zero.

Conversely, assume we can find solutions $e_{i1}, e_{i2}, \dots, e_{in}$ of (49), $i = 1, 2, \dots, n$ so that $|e_{ij}| \neq 0$. That means that if we consider the n vectors in $\mathbb{R}^n$

$$e_i = \begin{bmatrix} e_{i1} \\ e_{i2} \\ \vdots \\ e_{in} \end{bmatrix} \qquad i = 1, 2, \dots, n \tag{53}$$

they are a basis of $\mathbb{C}^n$ and under coordinatewise multiplication they satisfy the equations of condition (49). That means that the product determined on C_n by the equation of condition (49) has the property that it can be described as coordinatewise product in a suitable basis, or said differently, $C_n = \mathbb{C} \oplus \mathbb{C} \oplus \cdots \oplus C$, and that can only happen if Weierstrass's conditions are satisfied. Indeed these conditions are satisfied by the ε_{ijk} that define coordinatewise multiplication, and they remain true under a change of basis.

So when Petersen in his condition asks for n solutions of the equations (49), he really asks for the construction of a set of linearly independent vectors $e_1, e_2, \dots, e_n$ in $\mathbb{C}^n$, such that multiplication defined coordinatewise in the standard base in $\mathbb{C}^n$ will yield the equations of condition (49). Since the existence of a basis in which multiplication acts termwise is equivalent to Weierstrass's conditions, Petersen's conditions are clearly correct.

One may question the value of Petersen's result. Indeed the structure of Weierstrass's argument is to establish that under rather weak conditions (1–4) fulfilled by a host of (almost all) choices of ε_{ijk} (and by all choices that will yield acceptable algebras at all), one can prove that the multiplicative structure so to speak, separates out in the sense that expressed in a suitable basis it is nothing but coordinatewise multiplication. Conversely the existence of a basis in which the product is expressed coordinatewise implies Weierstrass's conditions, but of course, this converse result is less interesting or surprising. What Petersen's conditions ask is to construct the basis in which the multiplication is termwise in order to verify Weierstrass's conditions. It seems to verify the less complicated by the more complicated.

6 The Fate of Petersen's Paper

Petersen wrote his paper in German and sent it in May 1887 to Schwarz for publication in the *Göttinger Nachrichten*. At this time Petersen knew Schwarz quite well[10]. They had met for the first time in 1877 in Copenhagen, and later the same year Petersen had sent Schwarz a paper on integration of differential equations in closed form for publication in the *Nachrichten*. Already at this time Schwarz was faced with the problems involved in publishing Petersen's ideas. He asked Petersen to correct imprecisions and make references to the relevant literature. Moreover, when the paper appeared in 1878, Leo Königsberger (1837–1921) wrote a letter to Petersen calling his attention to a paper he had published the previous year in *Crelle's Journal* in which he had proved a more general theorem.

In 1886 (or 87) Petersen visited Schwarz in Göttingen. It may have been a discussion about hypercomplex numbers on this occasion that made Petersen compose his paper and send it to Schwarz. Schwarz immediately presented it to the *Königliche Gesellschaft der Wissenschaften zu Göttingen* on May 1887, but already five days later, while preparing the manuscript for publication, he found himself in a situation similar to the one he had been in ten years earlier: To be sure Petersen had referred explicitly to Weierstrass's paper and to a paper by Schwarz himself in which he proved that the subspaces G_i do not depend on the choice of the quantity g used in its construction [Schwarz 1884]. He had also referred to a paper by Otto Hölder (1859–1937) [1886], in which Hölder showed that the subspaces G_i are uniquely determined by the requirements that a) they are one- or two-dimensional, b) every point in C_n can be uniquely written as a sum $g_1 + g_2 + \cdots + g_r$ where $g_i \in G_i$, c) that $g_i \cdot g_j = 0$ if $g_i \in G_i, g_j \in G_j, i \neq j$ and d) $g_i \cdot g_i' \in G_i$ if $g_i, g_i' \in G_i$ and the product is only 0 if g_i or g_i' are zero.

However, as Schwarz informed Petersen, he had missed a central paper by Richard Dedekind (1831–1916) from the intervening year 1885:

> ... you have not quoted a paper by Professor Dedekind in Braunschweig, that was printed in the volume of 1885 of our Nachrichten on page 141–159, and to which Mr Dedekind has also published a supplement in the present volume (1887 page 1–7). The theorem that

[10]The following information is drawn from the Petersen-Schwarz correspondence [Petersen-Schwarz 1877–87].

> you have discovered is already to be found by Mr Dedekind. Therefore it is not enough just to *add* a reference to Mr Dedekind's paper to your list of references. Rather, it is absolutely necessary that you express yourself clearly about the relation between the content of your paper and the content of Dedekind's two articles. You alone can do that. Therefore I hurry to ask you to acquaint yourself intensively with the papers by Dedekind, so that you can complete your paper and your quotes. [Schwarz to Petersen, May 13, 1887]

The same month Dedekind himself also wrote Petersen a letter explaining his approach. Apparently Schwarz had informed Dedekind about the content of Petersen's paper, and it may very well be Schwarz who had suggested that Dedekind wrote directly to Petersen. Schwarz's letter shows clearly that he knew it would be difficult to make Petersen read Dedekind's paper and take it into account in a serious way. This proved to be correct. Petersen just added the following reference to Dedekind:

> The main theorem that I intend to announce in this paper says that each multiplication rule for complex numbers composed of n basic units, that are allowed according to the basic conditions stated by Mr Weierstrass, is determined by n systems of each n real or ordinary complex quantities such that the determinant formed by these n^2 quantities has a value different from zero. This main theorem has already been formulated by Mr Dedekind. The proof I have found seems to me to be simpler than the one published by Mr Dedekind. [Petersen 1887, 489–490].

Let us turn to Dedekind's paper and compare it to Petersen's. One similarity between them which opposes them to Weierstrass is that they both treat the problem in a complex setting. Here Dedekind is more consistent than Petersen in dealing from the start with complex coefficients (we would say he deals with vector spaces over $\mathbb{C}$) whereas Petersen first considered real coefficients, and only later introduced complex numbers. Dedekind's main theorem says (in slightly modernized version):

If $\{e_{ij}\}$ is a matrix with non zero determinant and we define ε_{ijk} such that its column vectors e_i multiplied coordinatewise will satisfy the equations (20,49)

$$e_r e_s = \sum e_i \varepsilon_{irs}\,, \quad r,s = 1,2,\dots,n \tag{54}$$

then the ε_{irs} will satisfy the commutativity and associativity conditions (23) and (24) and the condition that the determinant of

$$\Delta = \left\{\sum \sigma_i \varepsilon_{irs}\right\}_{r,s} \quad (\text{with } \sigma_i = \sum_j \varepsilon_{jij}) \tag{55}$$

is non zero. Conversely, and that is the main part of the theorem, if ε_{irs} satisfy these conditions they arise in the described way from a matrix $\{e_{ij}\}$ with non zero determinant.

Thus Dedekind's theorem did not directly establish the relation to Weierstrass's conditions but to another condition on the determinant Δ that is definitely simpler to check. So, when Dedekind in the introduction to his paper spoke of "a small simplification of the permissibility conditions formulated by Weierstrass", he did not talk about Petersen's condition but about the non vanishing of $|\Delta|$. Although it is a direct consequence of Dedekind's argument, he did not specify that one may think of the numbers e_{ij} in each row as a set of solutions to the basic equations (54). To make this explicit is Petersen's main contribution.

It is also arguably correct that Petersen's proof, as he wrote himself, is simpler than Dedekind's. However, that is mostly due to the fact that Dedekind proved a somewhat deeper theorem. Indeed, Weierstrass's conditions are so to speak proof-generated to fit the proof that a set of basic equations (54) with coefficients ε_{ijk} satisfying these conditions, will yield a basis in which multiplication is coordinatewise, leading in turn to the matrix $\{e_{ij}\}$. To deduce the same conclusion from Dedekind's one, seemingly simpler, requirement is a long story.

On September 9, 1887 four months after presenting Petersen's paper to the *Göttingen Academy*, Schwarz was again busy preparing Petersen's paper for publication, and he discovered that another major correction was needed: Petersen had accused Weierstrass of an imprecision (Ungenauigkeit), and had offered a counterexample to Weierstrass's proof. It seems from Schwartz's letter to Petersen (September 9, 1887) that Petersen suggested that by taking $g = e_0$ in his favourite example of ternions (§2), one would not get the desired result even though X_0 is not identically zero for this system. However, as Schwarz pointed out to Petersen, X_0 is equal to zero for this particular choice of g (or rather of $(\xi_1, \xi_2, \xi_3) = (1, 0, 0)$). He further argued that Weierstrass had rather explicitly stated that one must pick g in such a way that X_0 is different from zero for this particular g. Therefore he suggested to Petersen that "either your remark about the imprecision must be deleted completely or it must be phrased quite differently" (Schwarz to Petersen, September 9, 1887). Petersen chose to erase any criticism of Weierstrass.

Later the same day Schwarz sent Petersen a new letter suggesting a simplification of an example in Petersen's paper. Petersen had proved that the three quantities ε, X_0, D (see (26) and (29)) were related through the equation:

$$X_0 = \varepsilon D. \tag{56}$$

He had therefore asked the question: is Weierstrass's fourth condition $D \neq 0$ not a consequence of the second ($\varepsilon \neq 0$) and the third ($X_0 \neq 0$)? However, he correctly observed that the equality (56) had been derived under the condition that X_0, $\mathcal{E}$ and D are all non zero, and that therefore one cannot make the suggested conclusion. He had even given the following two-dimensional example

$$e_1^2 = e_2, \quad e_1e_2 = -e_1 - ae_2, \quad e_2^2 = ae_1 + (a^2-1)e_2, \tag{57}$$

which for $a = 2$ gives D equal to zero while X_0 and ε are not identically zero. He had learned this example from his friend Frederik Bing (1839–1912). However, Schwarz remarked that in this case the identity element is $e_0 = -ae_1 - e_2$, and he suggested using e_0 and $e_1 + \frac{1}{2}ae_0$ as new basic units. If we let δ denote $\frac{1}{4}(a^2-4)$ the fundamental multiplication laws simply look like:

$$e_0^2 = e_0, \quad e_0e_1 = e_1, \quad e_1^2 = \delta e_0. \tag{58}$$

He even indicated that any multiplication law for two-dimensional numbers could be reduced to this one.

> For this reason I cannot at all accept that an underhand game is being played in Mr. Bing's example.[11]

In Schwarz's example $\varepsilon = \xi_1^2 - \delta\xi_2^2$ and $X_0 = \xi_2(\xi_1^2 - \delta\xi_2^2)$ and a general quantity $g = \xi_1e_0 + \xi_2e_1$ satisfies the equation

$$g^2 - 2\xi_1 g + (\xi_1^2 - \delta\xi_2^2)e_0 = 0, \tag{59}$$

[11] "dass bei dem Beispiele des Herrn Bing thatsächlich *Verstecken* gespielt wird". [Schwarz to Petersen, September 9, 1887].

whose discriminant D^2 is $\delta\xi_2^2$. Therefore $\delta = 0$ gives the desired result $D^2 = 0$, $X_0 \neq 0$, $\varepsilon \neq 0$. Schwarz even claimed to have known this example for many years.

Petersen again followed Schwarz's advice, and replaced Bing's example with that of Schwarz, leaving out any reference to either of his two friends. With these corrections Schwarz considered Petersen's paper "as a real progress" (Schwarz to Petersen, September 9, 1887).

The printer sent Petersen a proof on October 4, 1887, but ten days later Schwarz informed Petersen about a new problem. He had asked Otto Hölder to go over the proofs, and Hölder had discovered that a remark Petersen had made about "the factors which are substitution determinants must also be made about ε (Schwarz to Petersen, October 19, 1887 [the meaning is not entirely clear to me]). However, as Schwarz remarked to Petersen:

> Now this change or correction cannot be made in a twinkling, because there is a risk that it may easily lead to a worsening rather than an improvement. [Schwarz to Petersen, October 19, 1887]

Therefore Schwarz simply left out the remark. He also announced to Petersen that Hölder would write and explain this point and other points to him in private.

Thus, when Petersen's paper finally appeared in the November 16, 1887 issue of *Göttinger Nachrichten* it bore a heavy imprint of its editor H.A. Schwarz.

7 Dedekind against Weierstrass, Göttingen against Berlin

In addition to presenting his complex alternative to Weierstrass's treatment and his simpler condition Dedekind had two goals with his 1885 paper on hypercomplex numbers. He called attention to §159 in his 10th supplement to his 1871 edition of Dirichlet's number theory. Here Dedekind had considered algebraic field extensions $\mathbb{Q}(r)$ of the rationals ("endliche Körper" in his terminology). If r is a solution of an irreducible equation of degree n over $\mathbb{Q}$ then elements of $\mathbb{Q}(r)$ can be uniquely represented as

$$\xi_1 + \xi_2 r + \cdots + \xi_n r^{n-1}, \tag{60}$$

where ξ_i $(i = 1, 2, \dots, n)$ are rational numbers.

In his 1885 paper Dedekind pointed out that if we allow the coefficients ξ_i to be real (or complex as Dedekind preferred), one would get an expression for a hypercomplex number written in the basis $e, g^1, g^2, \dots, g^{n-1} (g = r)$ where g is a solution of the given equation. He did not mention hypercomplex numbers explicitly in his Dirichlet supplement, but in the 1885 paper he claimed that he had "dealt with them in passing", implying that this happened in 1871. Thus the claim of priority he seems to make is that he was the first to publish on something strongly related. Weierstrass could still claim priority for his earlier discussion of hypercomplex numbers. Indeed, in his paper from 1884 he had written that he had "years ago conceived the following considerations" to which he added in a footnote: "I lectured on this subject for the first time during the winter semester 1861–62" [Weierstrass 1884, 396]. Dedekind did not question how many of the "considerations" Weierstrass knew about in 1861–62, nor did he push his own claim of priority beyond the vague hint to his 1871 supplement.

He was much more insistent on another matter, namely the evaluation of the following paragraph in a paper by Carl Friedrich Gauss (1777–1855) on biquadratic reciprocity:

> The author has planned to give later a more complete treatment of the subject which is really only touched upon in the present paper. Here the following question will then also find its answer. Why the relations between things that present a manifold of more than two dimensions cannot yield other types of quantities which are allowed in the ordinary arithmetic. [Gauss 1831, Werke II, 178].

This quote was the point of departure for Weierstrass's investigations of hypercomplex numbers. He interpreted Gauss as saying that it is impossible to define a multiplication for hypercomplex numbers of dimension 3 or more that satisfies all the arithmetical rules of the real numbers (i.e. that makes it a field). Having proved his main theorem Weierstrass concluded:

> When I compare the results of the preceding investigations with the quoted Gaussian remark, to wit complex quantities with more than two basic units are not allowed in the usual arithmetic, it seems to me that Gauss believed this impossibility was founded on the fact that the product of two quantities (when $n > 2$) can vanish without any of its factors vanishing. For if he had not considered this fact an insurmountable hindrance, he would probably not have failed to notice

> that it is possible to substantiate an arithmetic of such quantities in which the theorems are either identical to those of the arithmetic of the usual complex quantities, or at least find their analogue there. In that case he would undoubtedly have modified his claim to say that although the introduction of the general complex quantities in arithmetic is not impossible, it is superfluous. In fact, the theorem stated above (page 407) [Weierstrass's main structure theorem] shows that the arithmetic of the general complex quantities cannot lead to any results that cannot be deduced from the results of the theory of complex quantities with one or with two basic units. [Weierstrass 1884, 410–11].

Thus Weierstrass thought that Gauss's insight into the matter was limited to seeing that all the usual arithmetic laws, including unique division with non zero elements, could not be generalized beyond 2 dimensions. Weierstrass did not think that Gauss had investigated what would happen if one dropped the requirement of unique division with non zero elements, and he even suggested how Gauss ought to have phrased himself if he had seen what Weierstrass later saw.

Such a mild criticism of the *Princeps Mathematicorum* was too much for Gauss's former student Dedekind.

He agreed with the last part of Weierstrass's quote above and argued that this was indeed the meaning of the quote by Gauss. However, he phrased it somewhat differently. In fact, as admitted above, none of the three mathematicians we have considered in this paper used the language of vector spaces, and here is a point where it matters. Instead of talking about expressing the vectors $e_1, e_2, \ldots, e_n$ in a base in which multiplication is coordinatewise multiplication, Dedekind considered one system of complex numbers

$$e_1, e_2, \ldots, e_n,$$

but considered the system to be multiple valued, being able to obtain the n values

$$\begin{array}{ccc} e_1', e_2' & \cdots & e_n' \\ e_1'', e_2'' & \cdots & e_n'' \\ \vdots & & \vdots \\ e_1^{(n)}, e_2^{(n)} & \cdots & e_n^{(n)}. \end{array} \tag{61}$$

Here we would consider the columns to be the coordinates of the vectors e_i in the p-base. Dedekind, however, wrote that any polynomial equation in the e_i's would hold true if it hold true whichever of the multiple values of the system we substitute in the equation. More particularly, if we express everything in terms of $e_0, g, g^2, \ldots, g^n$ as above, any equation in the e's can be translated into an equation in $g : \varphi(g) = \psi(g)$, and such an equation is fulfilled, whichever of the roots $x_1, x_2, \ldots, x_n$ of the polynomial (30) we insert in the equation. I.e. $\varphi(r) = \psi(r)$ in this multiple-valued algebra means

$$\varphi(r) - \psi(r) = 0 \quad \mathrm{mod}\ f, \tag{62}$$

(this is similar to Weierstrass's observation). Thus the arithmetic of hypercomplex numbers is nothing but multiple-valued arithmetic of ordinary complex numbers. As an example Dedekind mentioned the system of ternions determined by the following basic equation:

$$\begin{array}{rcl} e_1^2 &=& -2e_1 - e_2 - 2e_3 \\ e_2^2 &=& -2e_1 - 2e_2 - e_3 \\ e_2^2 &=& -e_1 - 2e_2 - 2e_3 \\ e_1e_3 &=& e_1 + e_2 \\ e_3e_1 &=& e_2 + e_3 \\ e_1e_2 &=& e_1 + e_3 \end{array} \tag{63}$$

It is an easy matter to check that this system satisfies Dedekind's basic condition. But as pointed out by Dedekind, calculation with these quantities is indistinguishable from calculation with the three-valued periods

$$e_1 = r + r^{-1}, \quad e_2 = r^2 + r^{-2}, \quad e_3 = r^2 + r^{-2} \tag{64}$$

of the circle division polynomial

$$r^6 + r^5 + r^4 + r^3 + r^2 + r + 1 = 0, \tag{65}$$

when r runs through the different roots of this equation. Knowing Gauss's interest in the periods of the circle division polynomials [Gauss 1801], Dedekind suggested that Gauss had already obtained the insight that Weierstrass published, but that he drew the conclusion that such an arithmetic was not new, but corresponded to an arithmetic of multiple-valued complex numbers that had been studied long ago. This new interpretation of Gauss's somewhat ambiguous statement gives much more credit to Gauss. Dedekind put forward his interpretation already in his 1885 paper, but since he discovered that he had been misunderstood, he expressed it more explicitly in his supplement of 1887.

This small difference between Weierstrass's and Dedekind's opinion is symptomatic of the tensions between Berlin and Göttingen at the time. It is a clash between a Göttingen educated former student of Gauss, who revered his master, and a Berlin mathematician who did not mind attributing some limits to the insights of the Göttingen Princeps.

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Priority Claims and Mathematical Values: Disputes over Quaternions at the end of the Nineteenth Century

Tom Archibald *

1 Introduction: Priority and Values in the Case of Wessel

In this volume as well as in general treatises on the history of mathematics Wessel is identified as the first to arrive at a satisfactory geometric representation of complex numbers. However, Wessel's idea scarcely exhausts the relationship between imaginaries and geometry, as the paper of David Rowe in this volume shows. The fact, important to Wessel and others, that his geometric representation gives a way of making imaginary numbers real, likewise does not account for our declaring this particular creation as a "first" which is worthy of interest. For as other papers in this volume indicate, there are earlier efforts at resolving this imaginary character into reality, by Wallis, Foncenex, and so on. We therefore have to ask why it is this particular thing, the geometric representation of complex numbers, that we select for attention.

It seems to me that the reasons for this lie embedded in our mathematical values. This priority question was of key interest in the second half of the nineteenth century precisely because of the enormous importance of the theory of functions of a complex variable, a theory in which this representation is a fundamental tool. The work of Riemann on complex analysis,

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and the geometric interpretation of the Cauchy integral by Puiseux, Briot and Bouquet showed the basic importance of the complex plane. If we look at the way in which Wessel's priority came to be accepted, we see that the key event appears originally to have been taken as the 1831 *Anzeige* of Gauss, after which the "usual" complex plane appears to have become generally known and accepted, though the details of its reception remain to be worked out. In any case, looking at Hermann Hankel's 1867 *Theorie der complexen Zahlensysteme*, we get the impression that during Hankel's education the view of Gauss as originator of this representation was generally held. Hankel however revised this view, following Cauchy by mentioning a number of less well-known writers, and giving priority to Argand. In my view this is because for Hankel the exact form of the Gaussian invention is the key innovation, rather than for example the work of Buée, also mentioned by Cauchy. Thus an earlier formulation — that of Argand — which is essentially identical to that of Gauss counts as a prior discovery, whereas others less completely identical to Gauss's do not. Hankel's line was followed by Hoüel (1874), thus becoming the general view in France as well. The discovery of the fact that Wessel had done essentially the same thing as Argand thus gave weight to the claim of Valentiner, Thiele and Lie, and it is Wessel who generally gets credit from then on, as we can see for example in the *Encyklopädie der mathematischen Wissenschaften*. Thus the importance of the Wessel discovery stems from the importance originally given to the Gaussian contribution, via the historical revision to include Argand.

2 The Quaternion Priority Question

Quaternions are a different case. Now more marginal mathematically, their geometric representation presents a more complex set of issues in the resolution of priority. The efforts of Wessel to create an algebra adequate to handle rotations algorithmically clearly prefigure quaternions, as does work of other writers in the same vein, most notably Servois, Argand's contemporary, in the *Annales de Gergonne* of 1813. To grasp the negotiated character of mathematical priority, let us examine a dispute between P. G. Tait and Felix Klein which centres precisely on the issue of the extent to which the notion of rotation, and geometrical transformation more generally, captures the essence of the quaternion idea.

On December 18, 1899, Peter Guthrie Tait read one of his last papers before the Royal Society of Edinburgh. Tait titled his paper "On the Claim recently made for Gauss to the *Invention* (not the *Discovery*) of Quaternions" (emphasis and parentheses in the original). Tait's paper contested the argument advanced by Felix Klein to the effect that the essential idea of quaternions had been discovered by Gauss as early as 1820, based on a manuscript fragment found in the course of preparing the Gauss collected works. Tait's argument rested on two related points: first, that Klein had misunderstood what a quaternion was; and second, that the discovery of the quaternion operators (as opposed to the mere invention of the four-dimensional algebra with imaginary units i, j and k) was far more profound and consequential than anything envisaged by Gauss.

Tait thus was defending the priority of his old mentor Hamilton, as well as the mathematical ability of the British in general, against an attack from abroad. Indeed, Tait was no stranger to this genre: his *Sketch of Thermodynamics* of 1868 had begun a long series of efforts to assert priority for various British colleagues in diverse fields of mathematical and physical endeavour.

There is nothing particularly new about priority disputes in the sciences. However, the specifically national context of the disputes — the effort to claim glory not only for an individual but for a nation — took on a particularly strident form in the late nineteenth century. Not surprisingly, this coincides with the upsurge of national communities in mathematics and with national and international meetings.

In the absence of plagiarism, disputes about priority arise because of what is usually called independent discovery. Even when such discoveries are not synchronous, they force us to think about the historical circumstances under which they occur and the conditions which make such events possible. As Thomas Kuhn has discussed (Kuhn, 1959) for the case of simultaneous discovery, it is often the case that there is relatively little overlap in the actual findings, and this is certainly true for our case. As is clear from many of the other papers in this volume, considerable variation is possible in representing directed quantity mathematically. In mathematics, we have come to expect that such representations are to some extent translatable into one another with the benefit of hindsight. However such translations often overlook important aspects of the author's vision, and the differences between theories and their translations offer important fodder for disputes between interested actors.

3 The Background to Klein's Claim

3.1 Germany

One important part of the German background to Klein's claim is presented in Hankel's (1867) *Theorie der complexen Zahlensysteme*. Hankel's historical remarks concerning the geometrical representation of complex numbers mention a number of eighteenth-century precursors, but he follows Cauchy in giving priority to Argand (See Belhoste 1991 p. 232). Noting one French and one English rediscovery (by Mourey and Warren respectively) Hankel then asserts the importance of the German rediscovery by Gauss (1831), which brought this representation — as well as the very term complex number — to the attention of a wider mathematical public.

It should be noted that there had already been a Franco-German priority dispute in a closely related matter, namely between Grassmann in Germany and Cauchy and Barré de Saint-Venant in France. The matter was raised by Grassmann, and investigated in France by the Académie des Sciences, who sagely appointed a commission to investigate (which included Cauchy). This commission never reported, but the very fact of the dispute — which concerned the basic features of Grassmann's *Ausdehnungslehre* and Cauchy's "calculus of keys" — points out problems associated with simultaneous discovery in mathematics. (Belhoste 1991 236–37 and notes). Such simultaneous discovery is usually explained by appealing to an idea of the "natural" character of the object or representation in question. This appeal to what is natural, however, not only obscures the actual process of discovery, it overlooks the historical fact that what is natural to one observer may be not at all natural to another.

In the context of quaternions, the geometrical representation is certainly key. Indeed, Hamilton was in part motivated to develop quaternions in order to extend the geometrical properties of complex numbers in the plane to 3-dimensional space (See Hankins 1980, Chapter 21). Hankel's account of the theory of quaternions is based on the usual algebraic basis but gives a geometric interpretation which is quite his own. He is very proud of his economical account of the theory, and his remarks comparing his own work to Hamilton's are informative:

> The theory itself he [Hamilton] then presented with some applications in the very comprehesive "Lectures on Quaternions" [...] [of 1853],

> in a way which is extremely uncongenial to continental mathematicians but which for the English is, so it seems, completely natural. The theory is broken into scattered pieces, problems are not treated in their generality, but rather in special cases, then interrupted by applications and other investigations; so that only later, and at times only incidentally, are they solved fully.[1] (Hankel 1867, 194–196)

He continues:

> As I wished to transplant this theory onto German soil, it was necessary to alter the presentation totally.[2] (Hankel 1867, 196)

Hankel's change in the presentation of the work consists in the use of a much stricter axiomatic style. There is no discursive content about the comparative merits of the quaternion approach, and applications are kept to a minimum. Thus Hankel claims that he has cut down Hamilton's original 545 pages to his account of about 50 pages with the sacrifice of only a few applications. We note, however, that his geometrical interpretation of quaternions is markedly different from that of Hamilton. In particular it is much more restricted, treating them as motions which bring into coincidence arcs on the surface of a sphere.

Hankel noted specific differences between the German approach and that of the British, insisting that Hamilton's work unaltered could not take root in German soil, indeed on the continent at all. This view is borne out by the French reception of quaternions, which received their first serious discussion in France via the thesis of Charles-Ange Laisant in the late 1870s and his subsequent books.

3.2 France

Despite the limited interest in quaternions as such, the French did take up the question of the priority claim to geometric representation of complex

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[1] Die Theorie selbst hat er dann mit einigen Anwendungen derselben in den sehr umfangreichen "Lectures on Quaternions [...]" [of 1853], in einer den continentalen Mathematikern sehr unbequemen, den Engländern aber, wie es scheint, durchaus natürlichen Weise dargestellt: Die Theorie ist aufgelöst in zerstreute Strecke, die Probleme werden nicht in ihre Allgemeinheit, sondern zunächst in speciellen Fälle behandelt, dann unterbrochen durch Anwendungen und andere Untersuchungen, um erst später, zuweilen nur gelegentlich in ihren ganzen Umfang erledigt zu werden [...]

[2] Wollte ich diese Theorie auf deutschen Boden verpflanzen, so war es notwendig, die Darstellung total zu verändern.

numbers. A few years after the appearance of Hankel's monograph, in 1874, Argand's 1806 treatise was republished in France under the editorship of J. Hoüel. The book also contained the texts of the papers in the *Annales de Gergonne* by Servois, Argand, and Français which had served to make public the result of Argand and establish his priority. The appearance of the book attests to the perceived national interest in Argand's work — Gauthier-Villars was notorious for business acumen, so the market must have at least seemed to be solid. In a preface, Hoüel outlined the story of discoveries and rediscoveries, citing Hankel to indicate that his (and Cauchy's) claim for Argand's priority was not merely a nationalistic one, but was accepted in Germany. At the time (1874) this was necessary as the aftermath of the Franco-Prussian War had seen a number of unfortunate incidents in which allegations of plagiarism were traded back and forth between agitated French and German nationals. (See Dugac, 1984, passim).

4 Gauss, Klein and Sommerfeld

The text that roused Tait's ire was a report by Klein on the progress of the collected works of Gauss, which at first was available only in the *Göttinger Nachrichten* but which was republished in the *Mathematische Annalen* in 1899 (Klein, 1899). Specifically, Klein asserted that the foundations of the theory of quaternions appeared in an unpublished note of Gauss which had been dated by the editor (Paul Stäckel) to 1819. The key point, and the point of subsequent contention, concerned what Klein meant by the foundations of the quaternion theory. His description makes this reasonably clear:

> [...] already in 1819 he represented the mutations of space (as he calls them) by the same four parameters as used in the quaternion theory [...] [where by mutations of space he means] rotations of space about the origin of coordinates, combined with an arbitrary similarity transformation likewise based at the origin. He designated the concept of these four parameters as a "mutation scale", and gives explicit formulas for the composition of two scales (thus the multiplication of two quaternions), in which context he used the symbolic notation
>
> $$(abcd)(\alpha\beta\gamma\delta) = (ABCD)$$
>
> and expressly remarked that one is here dealing with a non-commutative process![3] (Klein 1899,130–131)

[3][...] er hat die Mutationen des Raumes (wie er es sagt), d.h. die Drehungen des

To assess Klein's remarks, we first examine the Gauss fragment in question. It should be borne in mind that Tait's later remarks rest on Klein's description, rather than on the fragments themselves.

Gauss's few notes on the question appear in two parts, the first of which Stäckel, who was the editor of this portion of the *Werke*, dated to 1819, with the exception of one brief note apparently of later date which he ascribed to 1822 or 1823. The second part is on a separate slip which he describes merely as somewhat later. In the first of these rather sketchy notes, Gauss appears to be thinking about generalizing the mathematical description of rigid body motions to include stretches. He expresses such transformations of space coordinates (as he puts it) in terms of parameters a,b, c, and d, the *Transformationsscale* in his terms, which specify such a transformation. The second fragment specifies that $k = \sqrt{a^2 + b^2 + c^2 + d^2}$ is the stretching factor and that if $r = \sqrt{b^2 + c^2 + d^2}$ then the direction cosines of the rotation are b/r, c/r, and d/r. This is implicit in the first fragment. He further notes that if we consider the composition of two transformations with parameters a, b, c, d and α, β, γ, δ respectively, then this composition is exactly what Hamilton would later call the quaternion product of the two. He specifically notes that the order of the composition is not indifferent, in fact using the notation that $(a, b, c, d)\,(\alpha, \beta, \gamma, \delta)$ is not to be interchanged with $(\alpha, \beta, \gamma, \delta)\,(a, b, c, d)$. (Gauss, 1819, 360). Missing in Gauss's notes is any account of the additive quaternion group, as we would call it today.

Tait's counterargument in support of Hamilton rests on the view that this conception of quaternions does not capture the richness of Hamilton's ideas about these objects. Nevertheless, the Gaussian fragment does very nicely prefigure Klein's own account of quaternions, which appeared in the first part of his joint work with Sommerfeld on the mechanics of the top. This

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Raumes um den Coordinaten-Anfangspunkt, verbunden mit einer beliebigen von letzteren auslaufenden Aehnlichkeitstransformation bereits 1819 durch dieselben vier Parameter dargestellt, welche die spätere Quaternionentheorie benutzt; er erzeichnet den Inbegriff dieser 4 Parameter als Mutationsscala und giebt die expliciten Formeln für die Zusammensetzung zweier Scalen (also die Multiplication 2 Quaternionen) wobei er die symbolische Schreibweise

$$(abcd)(\alpha\beta\gamma\delta) = (ABCD)$$

benutzt und ausdrücklich bemerkt, dass es sich dabei um einen nicht commutativen Prozeß handelt!

long book contained an "Exkurs über die Quaternionentheorie" which appeared as a short section at the end of the first chapter. This book had grown out of lectures given by Klein in 1895/96, and was originally intended to foster aims which Klein, writing in the third person in his preface, gave a specific national colour: However, in contrast to Hankel's rather harsh rejection of the expository and theoretical chaos of Hamilton's treatise, Klein diplomatically emphasized the richness of the subject for mechanics:

> [In the lectures] first of all he [Klein] undertook to mention the direct expression of mechanical problems widespread in England, in contrast to the more abstract colour of the German schools. On the other hand he set out to render fruitful the methods of Riemann's function theory which were developed in Germany.[4] (Klein and Sommerfeld 1897–1910, III)

It was only subsequently that Klein integrated applications into the published work, in part due to the influence of Sommerfeld, who had by that time taken a position involving teaching of mechanics. The book itself appeared in four parts, in 1897, 1898, 1903 and 1910 repectively, and these exhibit a change in authorial standpoint corresponding to a shift towards interest in applications and to the appeal to a wider readership. Thus some of the theory set up at the beginning of the book — for example the introduction of quaternions — is not really used in what follows. Nevertheless we'll examine the view of quaternions given by Klein and Sommerfeld with the end in mind of understanding Tait's objection to their approach, and Klein's interest in the Gauss fragment. Though the work is jointly authored I will perhaps unfairly refer to the synthesis presented as Klein's, mostly for abbreviation.

Klein begins by noting that despite the age of quaternions – he dates them to Hamilton's book *Lectures on Quaternions* of 1853 — they remained controversial, in his view due to the metaphysical language in which Hamilton couched his presentation. This, says Klein, obscured the "simple geometrical meaning" which can be given to the operations of this calculus. He hopes to clarify this, the more so as he will show that quaternions are, as he puts it

[4]Unternahm er es in erster Linie, die namentlich in England verbreitete unmittelbarere Auffassung der mechanischen Probleme gegenüber der abstrakterer Färbung der deutschen Schule zu betonen, andererseits die namentlich in Deutschland ausgebildeten Methoden der Riemannschen Funktionentheorie für die Mechanik fruchtbar zu machen.

"a special case" of the vector calculus. [Klein and Sommerfeld 1897–1910, 55].

In his initial efforts to describe the top and its motions, Klein introduces parameters which are his first example of a quaternion quantity. He considers a rectangular coordinate system xyz, thought of as fixed, and another, XYZ, which moves with the rotating top but which is fixed with respect to it. The orthogonal transformation from one coordinate system to another is then specified in the usual way by the 3×3 matrix of direction cosines. These nine parameters are redundant, and may be reduced to three (since the top is rigid). The classical way of doing this involves the use of the Euler angles ϕ, ψ, and θ, defined as follows. The nodal line (*Knotenlinie*) is defined as the line through the common origin of both coordinate systems which is perpendicular to the plane of the vertical (the z-axis) and the axis of rotation (the Z-axis). The angle ϕ is then the angle between the nodal line and the positive X-axis, ψ is the angle between the nodal line and the positive x-axis, and finally θ is the angle between the z-axis and the Z-axis. A routine calculation thus gives the matrix of the transformation from the XYZ to the xyz-system in terms of these Euler angles.

This familiar description is not the most useful for the top, a result of the fact that elliptic functions are involved in Klein's eventual description. To obtain better parameters for his purposes, Klein then introduces complex variables in each of the horizontal planes, so that taking

$$(\xi, \eta, \zeta) = (x + iy, -x + iy, -z)$$

and

$$(\Xi, H, Z) = (X + iY, -X + iY, -Z)$$

he gets the transformation matrix in the form

$$\begin{bmatrix} \alpha^2 & \beta^2 & 2\alpha\beta \\ \gamma^2 & \delta^2 & 2\gamma\delta \\ \alpha\gamma & \beta\delta & \alpha\gamma + \beta\delta \end{bmatrix}$$

where

$$\alpha = \cos(\theta/2)\, e^{\frac{i(\phi+\psi)}{2}},$$

with similar expressions for the other parameters. These parameters have various computational advantages arising from the fact that $\alpha\delta - \beta\gamma = 1$. The complex parameters may in turn be expressed in terms of real ones:

$$\begin{aligned}
\alpha &= D + iC \\
\beta &= -B + iA \\
\gamma &= B + iA \\
\delta &= D - iC.
\end{aligned}$$

In this notation the matrix of the change of coordinates becomes

$$\begin{bmatrix}
D^2 + A^2 - B^2 - C^2 & 2\,(AB - CA) & 2\,(AC + BD) \\
2\,(AB + CD) & D^2 - A^2 + B^2 - C^2 & 2\,(BC - AD) \\
2\,(AC - BD) & 2\,(BC + AD) & D^2 + A^2 + B^2 + C^2
\end{bmatrix}.$$

The quantities A, B, C and D satisfy the relation $A^2+B^2+C^2+D^2 = 1$, and are termed quaternion quantities by Klein. (Klein and Sommerfeld 1897–1910, 19–22).

We see that these do not appear to be quaternions in the ordinary sense of today. They are however in the sense of Klein, who regards a quaternion as a geometric transformation called a *Drehstreckung*: the product of a rotation and a similarity. In the transformation just described, if the angles between the axes of the XYZ and those of the xyz systems are a, b, and c respectively, and if ω is the amount of the rotation, then we find

$$A = \sin\frac{\omega}{2}\cos a, \quad B = \sin\frac{\omega}{2}\cos b, \quad C = \sin\frac{\omega}{2}\cos c \quad \text{and} \quad D = \cos\frac{\omega}{2}.$$

Generalizing from this, Klein notes that if we choose any four parameters and insert them in a matrix like the matrix of transformation just given, the result is a *Drehstreckung* with stretch factor $A^2 + B^2 + C^2 + D^2 = T$. Thus from any system of four values we can get a transformation, and any transformation of this kind can be described by four such parameters. [Klein and Sommerfeld 1897–1910, 55–56].

Klein then introduces Hamilton's usual notation and the terminology that T is the tensor of the quaternion, D its scalar part, and $Ai + Bj + Ck$ its vector part. Klein notes in particular that it has the character of a vector since it has a specified "axis" given by the angles a, b, and c and a specified length $\sqrt{T}\sin\frac{\omega}{2}$. In this sense, Klein notes the "quaternionic" (in his meaning) interpretation of a pure vector: it has scalar component $D = 0$, so the angle of rotation must be π, accompanied by an arbitrary stretch (a *Wendestreckung* in his terminology).

Passing to the algebra of quaternions, Klein derives the usual multiplication from the identification of a quaternion with a *Drehstreckung.* This in turn implies the identities among the quaternion units (thus far treated as irreducible symbols), as well as non-commutativity. He then continues with the vectorial interpretation, noting the position of the scalar product (Grassmann's inner product) and the vector product (exterior product) in the quaternion product and giving the standard geometric interpretations of both.

We note that Klein's quaternions are very much the Gaussian objects, as even a casual inspection of Gauss's text shows. The *Drehstreckung* is exactly what Gauss sought to describe, and modulo some small changes in notation his results agree completely with those of Klein. It is hardly necessary to observe that in the Germany of the late nineteenth century one could scarcely hope for a better pedigree for one's work. It is not clear to me at this time whether the two are independent. Klein's announcement of the Gauss discovery is certainly subsequent to the lectures, as is Klein's involvement with the Gauss edition which occurred only after the death of Schering.

5 Tait's Criticism of Klein

Tait's complaint was to rest on the fact that the view of quaternions presented by Klein misses their essence. He emphasized various fundamental differences between the true quaternions of Hamilton and the *Drehstreckungen* of Klein. He underlines two aspects of quaternion theory: the *invention* (as he terms it) of quaternion algebra and the *discovery* of the rich quaternion calculus spanning the last half of the nineteenth century. Both of these are, in the view of Tait, entirely due to Hamilton.

Arguing in his usual polemic tone, Tait firmly rejected Klein's conclusion that a *Drehstreckung* is exactly the same as a quaternion:

> Unfortunately [...] a *Drehstreckung* is [...] a totally different kind of concept. It is obviously only a very limited form of linear and vector

> operator (kinematically a strain) depending on four quantities instead of the usual nine; and might [...] have been designated by the name quaternion had the name not been already more worthily bestowed. (Tait 1899–1900, 19).

A further problem with the *Drehstreckung* is the fact that the set of these motions is not closed under addition.

What then is a quaternion in Tait's view? As he puts it, it

> expresses the relation of one vector to another, or supplies the factor required to turn one into another. (Tait 1899–1900, 19).

For Tait, then, a quaternion is formally a vector quotient, where by a vector is meant a free directed line segment. Completely determined by a pair of vectors, the quaternion is "as real as either" (ibid, 19). This is the essence of the great discovery to which Tait referred, which is accompanied by the following representation. The pair of vectors (if distinct) determines a plane, an angle and has a certain ratio of lengths. This leads to the expression

$$q = \frac{\beta}{\alpha} = \frac{b}{a}\left(\cos A + \epsilon \sin A\right)$$

where α, β are the vectors, a and b their lengths, A the angle they determine, and ϵ the unit normal to the plane.

The difference between this and a *Drehstreckung* may then be summed up by noting that when a quaternion is *applied* to a vector in or parallel to its plane, the result is indeed a rotation combined with a change of length. However, when the vector is not perpendicular to ϵ, the operation still works, producing another quaternion, not merely a vector. Hence in Tait's analysis, a quaternion has no direct connection with rotation, though "as an organ of expression capable of dealing with all space-problems it may be used to describe a rotation". Klein's *Drehstreckung*, "like everything else in space, can be represented by means of quaternions, but as a quaternion *operator*, not as a quaternion." (ibid. 20–21. Emphasis in original). This is because a *Drehstreckung* is not what Tait terms a "space-reality". It rather needs something to operate on, or as Tait puts, it, it "requires a subject before it can attain embodiment." (ibid. p. 20).

Tait elaborates on this for some pages, displaying in various calculations the greater simplicity (and in his view "reality") of the quaternion. The modern reader may feel that at times rather subtle distinctions bordering on the theological are being insisted upon. For example, emphasizing his point that Gauss, had he possessed the quaternion concept fully, would surely have discussed addition, Tait elaborates on the impossibility of adding *Drehstreckungen* as one adds quaternions:

> To add two *Drehstreckungen* they first must be embodied, separately, in any common vector, and the resulting vectors geometrically compounded. Then the *Drehstreckung* which produces the resultant must be found. (ibid. 23, emphasis in original).

6 Conclusion

What was the effect of Tait's response? There is more than one answer. As far as quaternions proper were concerned, we see some acknowledgement of Tait's remarks in the relevant articles of the *Encyklopädie der mathematischen Wissenschaften*. Writing in 1898, E. Study repeats the claim in Klein's paper in the first volume, before Tait's claims were made (Study 1898 p. 183). H. Rothe's article in the third volume accepts Valentiner's claim of the priority of Wessel over Argand, but further states of Gauss that he had the multiplication theorem for quaternions in 1819 or 1820 — a more restricted claim than that of Klein; he furthermore notes Tait's article, though he does not comment on its content.

By 1905, however, Study was more measured in his comments. In (Study, 1905, 423) he claims that Gauss had the multiplication theorem, but further remarks:

> With this, however, he was far from possessing the entire theory of Quaternions, for which among other things the association of vectors with simple figures in space — pairs of vectors — is essential. We must concur with English authors when they seek to treat Hamilton as the true discoverer of the quaternion theory after [the discovery of Gauss' work] as well as before. The further assertion that there is a difference of principle between Gauss' four-tuple and Hamilton's quaternions seems to us unconvincing.[5] (Study, 1905, 423)

[5]Damit besaß er aber bei weitem noch nicht die ganze Quaternionentheorie, für die unter anderen die Zuordnung der Quaternionen zu einfachen Figuren im Raume — paaren von Vektoren — wesentlich ist. Wir werden englischen Autoren recht geben müssen,

Study thus yielded to the view of Tait that Gauss lacked the full power of the quaternion theory, while nevertheless holding out in support of Klein's view, and against Tait's, that Gauss and Hamilton were not so very different in their ways of thinking.

There is another aftermath, however, one which would take us too far afield to explore in detail, but which should be mentioned. This is in the context not of quaternions but of vector calculus, where many of the most important quaternion notions eventually came to roost. Recasting quaternion notions gave further opportunities to examine their roots, For example, we find Emil Jahnke in 1905 tracing vectorial analysis to the barycentric calculus of Möbius — again a German priority over Hamilton, this time a published one. Quaternions were eventually marginalized completely as vector calculus became the norm, pushing debates concerning their priority well into the background.

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wenn sie nach wie vor Hamilton als den eigentlichen Erfinder der Quaternionentheorie betrachtet wissen wollen. Unzutreffend scheint uns indessen die weitergehende Behauptung, dass zwischen den Gauss'schen Grössenquadrupel und den Quaternionen Hamiltons ein prinzipieller Unterschied vorhanden sei.

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On the Role of Imaginary Elements in 19th-Century Geometry

David E. Rowe *

Our present-day knowledge of the principal research trends in geometry during the nineteenth century may aptly be characterized as fragmentary and, in many resepects, thin. Reasons for this are not far to seek. Some mathematicians might be inclined to the ungenerous view that those few classic results which survived the conceptual upheaval that gave birth to modern mathematics during the early decades of this century constitute the vital core of the previous century's accomplishments. Historians of mathematics would presumably take a more liberal stance, but they, too, have done rather little to illuminate the vast body of geometrical knowledge that accrued during the period spanned by two nearly forgotten giants, Gaspard Monge and Sophus Lie. If modern mathematicians often take comfort in knowing that today's standards in geometry are infinitely more rigorous than the ones that guided the best work of the mid-nineteenth century, historians should be especially sensitive to the kinds of conceptual and technical problems that preoccupied the geometers of the distant period.

Given the sparsity of historical studies on classical nineteenth-century geometry, we might well wonder how one can hope to achieve an overview of this vast terrain. One very useful source for scanning the literature on geometrical research during the nineteenth century was published by Duncan Sommerville in 1911 under the title *Bibliography of Non-Euclidean Geometry, including the Theory of Parallels, the Foundations of Geometry, Space of n Dimensions.*[34] This meant non-Euclidean geometry in the broad sense. In fact, the territory Sommerville covered might well be regarded as an indicator of the breadth of activity that served to undermine the once-dominant

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paradigm of Euclidean geometry. The so-called non-Euclidean revolution, however, has normally been understood, first and foremost, as a supplanting of the Kantian view that physical space was *a priori* Euclidean by a doctrine that at least recognized the remote possibility that space *could* be curved.[1] Yet beyond the immediate space problem, an issue that first really reared its head with the work of Beltrami and Helmholtz in the late 1860s, we encounter in Sommerville's bibliography a host of geometrical investigations written decades earlier that no longer fit into the traditional Euclidean mold. Based on this information alone, it would seem quite plausible to argue, as Erhard Scholz has done, that a growing familiarity with projective properties, higher-dimensional spaces, and the geometries associated with higher analysis played a major part in weakening the privileged position of Euclidean geometry.[2]

Running alongside these developments, geometrical practitioners took various sides in a complex, but long-standing methodological debate. This takes us to the heart of the topic I will be concerned with here, namely the role of imaginaries in geometry. From nearly the beginning of the nineteenth century, imaginary points and other constructs played a central, yet problematic role in numerous geometrical investigations. This issue carried profound implications for the conduct of research, and not only because it reinforced the inner tension between those who advocated an analytic as opposed to a synthetic style. As we shall soon see, even the leading lights for the promotion of synthetic geometry, figures like Steiner and Staudt, knew very well that they could not dispense with imaginaries altogether. Their strategy, rather, was to make these "ghostlike" entities accessible to "Anschauung", which for Staudt meant interpreting them by means of real constructions carried out in Euclidean (or affine) space. Thus, while Sommerville's bibliography sheds considerable light on work that undermined Euclidean geometry by exploring new theories of parallelism or the geometries of higher-dimensional spaces, etc., his list ought to be supplemented by a number of other publications that document the intense efforts of nineteenth-century mathematicians to grapple with complexified geometries, or at least ones which gradually made room for imaginary elements. Naturally, in this essay I can only touch upon the relevant literature, but I do wish to leave you with

[1]For an account of how this space problem played out in Britain, see [28].

[2]For an illuminating discussion of the currents that gradually undermined the Euclidean paradigm, see [33], 342–345.

an impression of its larger importance. This aspect of classical geometry constitutes, in my opinion, one of the central themes that accompanied the non-Euclidean revolution when understood in its broadest terms. Indeed, one can follow the role of the same theme of complexification and reality relations in the context of the geometrization of relativity theory beginning with the work of Minkowski.[3]

1 Imaginaries as Ghosts: Poncelet and Steiner

Standard histories of mathematics have often overlooked the role of imaginaries in nineteenth-century geometry, in part because these accounts tend to portray the work of Victor Poncelet (1788–1867), Joseph Diaz Gergonne (1771–1859), and Michel Chasles (1793–1880) largely as a revival of *synthetic* geometry, a view promoted by both Poncelet and Chasles themselves.[4] This interpretation can be quite misleading, however, for it tends to overlook how much of this French work was directly inspired by methods taken from algebraic analysis. Indeed, contemporaries often referred to the approach of Chasles and others as a *méthode mixte*, a style characterized by flip-flopping between algebraic and synthetic techniques.

Euler's work already contributed to a widespread recognition of the utility of imaginaries in algebra and algebraic analysis during the latter half of the eighteenth century. A cornerstone property of the complex numbers, namely the fundamental theorem of algebra, was given by Euler but without adequate proof, so that both D'Alembert and Gauss sought to remedy this flaw afterward. A still more general algebraic result was Bézout's Theorem, which originally arose in the context of algebraic elimination theory. From a geometrical standpoint, this theorem simply asserts that two (irreducible) algebraic curves C_m and C_n given by polynomial equations $P(x, y) = 0$ and $Q(x, y) = 0$ of degree m and n, respectively, will, in general, intersect in exactly mn points when due account is taken of their multiplicities ($C_m \cap C_n = mn$ points). Some of these points of intersection may, of course, be imaginary. Maclaurin had already conjectured this result in 1720, and Euler handled various cases where imaginary roots arise. Bézout was able

[3]See the discussion in [42].

[4]Both, however, stressed the decided superiority of modern methods over those of classical Greek geometry. See, for example [3], 268–269.

to prove that if C_m and C_n have no common asymptotic directions, then the polynomial equation in x that one obtains by elimination methods will always be of degree mn.

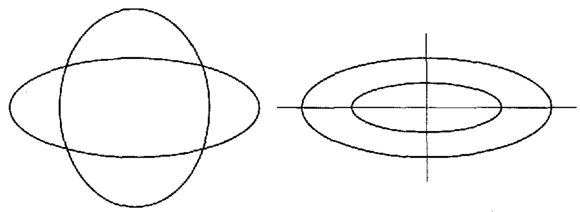

Figure 1: Conics illustrating Bezout's Theorem for $m = n = 2$.

The significance of this result for geometry was undoubtedly very clear to Victor Poncelet, who used imaginary points freely, introducing I, J, the so-called circular points at infinity. As the simplest example of Poncelet's bold treatment of imaginaries, consider a pair of intersecting circles given by the equations $x^2 + y^2 + ax + by + c = 0$ and $x^2 + y^2 + a'x + b'y + c' = 0$. Clearly, these meet on the line ℓ, known as the radical axis, given by

$$(a - a')x + (b - b')y + (c - c') = 0.$$

Noticing that the same algebra leads to a real radical axis even when the circles fail to intersect, Poncelet merely indicated that the vanishing points of intersection were imaginary points that must lie somewhere on the radical axis of the two circles. His argument was based on continuity: if two intersecting circles are gradually pulled apart, their points of intersection slide along the radical axis until they eventually coincide—at which point the circles touch one another—and thereafter they become imaginary. The crucial issue for him was to uphold the legitimacy of his principle of continuity as a heuristic device in geometry. In the face of harsh criticism by France's leading analyst, A. L. Cauchy, Poncelet argued that his synthetic methods could be employed with the same freedom that the algebraist enjoys in manipulating equations. In his *Traité des propriétes projectives des figures* of 1822 Poncelet wrote:

> ... would it not be legal to accept the continuity principle in its total generality in theoretical geometry, just as one has accepted it first in algebra and then in the applications of this calculus to geometry,

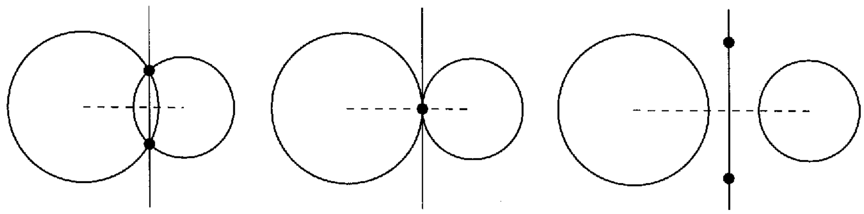

Figure 2: The radical axis contains two of the four points of intersection of two circles.

> as a means of discovery and invention if not of proof? ([27], xiv, my translation.)

Just how problematic Poncelet's techniques were can be seen by returning to the case of circles considered above. The algebra of Bézout's Theorem leads us to conclude that two circles, just like any two conic sections, being curves of the second degree must have *four* points of intersection and not just two. This led Poncelet to contemplate systems of concentric circles, which can be viewed as analogous to systems of rectangular hyperbolae. Since his mode of argumentation was synthetic, Poncelet was necessarily obscure about the nature and properties of imaginary points, but by the mid 1800s French geometers had grown accustomed to such arguments, enough so that these "ghosts" seem not to have haunted them terribly much. Indeed, imaginaries became part of the "standard equipment" of algebraic geometers, and little concern was shown as to their precise ontological status. For the French school it apparently sufficed that one could visualize the most important imaginary constructs by employing an appropriate imaginary transformation.

Thus, for example, the circular points at infinity could be "seen" by employing an appropriate imaginary transformation on the points in the projective plane. Poncelet argued synthetically, but for purposes of clarity we will find it convenient to employ homogeneous coordinates (x, y, z), where $z = 0$ represents the line at infinity (homogeneous coordinates were routinely used by both Plücker and Möbius in the 1830s). If we consider the image of the family of concentric circles centered at the origin, $\{x^2 + y^2 = rz^2 \mid r \in \mathbb{R}^+\}$, under the transformation

$$x' = x, \qquad y' = iy, \qquad z' = z,$$

the resulting curves form the family of rectangular hyperbolae $\{x'^2 - y'^2 = rz'^2 \mid r \in \mathbb{R}^+\}$ whose asymptotes make an angle of 45 degrees with the coordinate axes. In particular, the circle $x^2 + y^2 = 0$ goes over into the hyperbola $x'^2 - y'^2 = 0$, which shares the same points of tangency at infinity, namely $(1, \pm 1, 0)$, with the other curves in the family. Poncelet reasoned similarly that this behavior of the hyperbolae must also hold for the family of circles since the imaginary circular points $(1, \pm i, 0)$ get mapped to the *real* infinitely distant points $(1, \pm 1, 0)$. Thus, the circular points at infinity could be seen as lying on *all* circles in the plane. Poncelet further called attention to the analogous figure in three-space, the imaginary spherical circle which lies on all spheres.

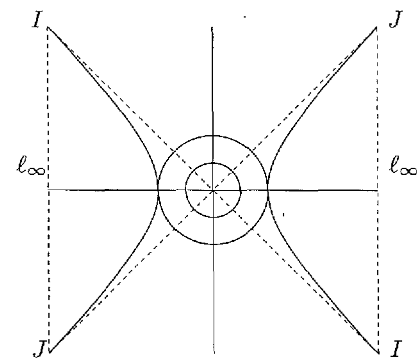

Figure 3: For $r = 0$ the isotropic lines of the family of concentric circles go over into the asymptotes of the corresponding system of rectangular hyperbolae.

In this connection, one should bear in mind that the French geometers of the early nineteenth century never severed projective geometry from its Euclidean roots. Thus, Poncelet often derived projective properties from metrical ones by employing appropriate transformations and his infamous principle of continuity. His leading disciple, Michel Chasles, made copious use of isotropic (sometimes called minimal) lines and the imaginary spherical circle at infinity. These lines arise in pairs; in the complex projective plane $\mathbf{P}^2(\mathbb{R})$ they correpond to the points of the equation $(x-a)^2 + (y-b)^2 = 0$, which factors into $[(x-a) + i(y-b)][(x-a) - i(y-b)] = 0$. We see at once that only the point P with coordinates (a, b) is real, but for any such fixed P the isotropic lines will pass through the imaginary circular points $(1, \pm i, 0)$.

Chasles also cultivated the study of confocal systems of conics. Such systems have the property that the tangents to any conic drawn from the circular points at infinity I, J intersect in its foci (two of which are real and two imaginary), as first noted by Julius Plücker.

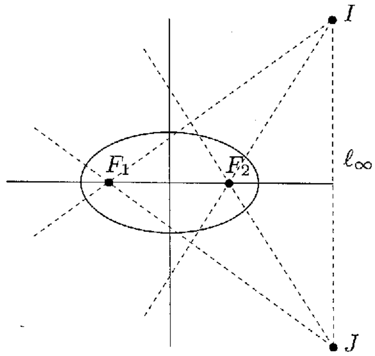

Figure 4: Plücker's Theorem.

The connection between confocal conics and projective concepts only became somewhat clearer through Edmond Laguerre's projective formula for the angle Θ determined by two lines ℓ_1, ℓ_2 that meet at a point P: $\Theta(\ell_1, \ell_2) = \frac{1}{2i}\log(CR(\ell_1, \ell_2, \ell_I, \ell_J))$ where CR is the cross ratio and ℓ_I, ℓ_J

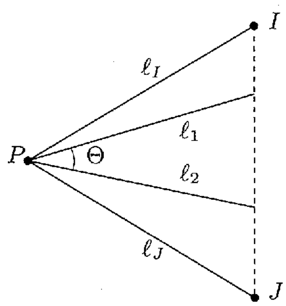

Figure 5: Laguerre's Formula.

denote the isotropic lines determined by the point P, i.e. the lines joining P with I and J, respectively. The orthogonal intersection of confocal conics follows directly from this formula and the fact that the tangents ℓ_1, ℓ_2 at a point P common to two confocal conics are harmonically separated by ℓ_I, ℓ_J

(since in this case $CR(\ell_1, \ell_2, \ell_I, \ell_J) = -1$).

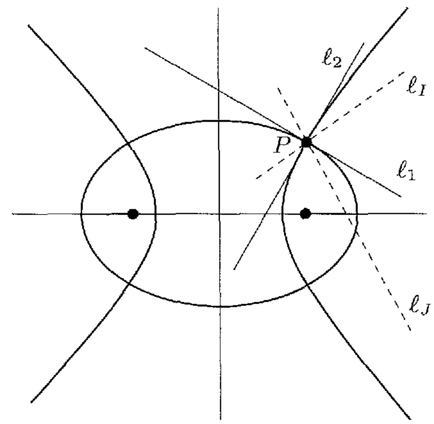

Figure 6: Confocal conics intersect orthogonally.

The possibilities for developing a more rigorous, but purely synthetic foundation for projective geometry were first explored by Jacob Steiner (1796–1863). During his thirty-year tenure on the faculty at the University of Berlin, Steiner emerged as the most influential geometer in Germany (see [1], 55–60). It was no accident that he evinced skepticism when it came to the status of imaginary entities in geometry, calling them "ghosts" (*Gespenster*).[5] On the other hand, he seems to have found it impossible to dispense with these mysterious creatures in his own work on projective geometry. The leading representative of the opposite, analytic approach, Otto Hesse (1811–1874), remarked perceptively at the time of Steiner's death that the last part of his career:

> ... scheint dem Kampfe mit dem Imaginären gewidmet gewesen zu sein. Es gewinnt diese Hypothese an Wahrscheinlichkeit, wenn man von dem Gespenst—wie Steiner sich auszudrücken liebte—in der Ebene und Raume hört, mit dessen Hilfe er die verborgenen Wahrheiten enthüllte. ([10], 199.)

Hesse further surmised that Steiner had been driven to publish many of his findings without any kind of supportive arguments because his own syn-

[5]One is reminded here of Bishop Berkeley's retort to Newton that his fluxions were nothing but the "ghosts of departed quantities" ([39], 338).

thetic methods, as set forth in his classic *Systematische Entwickelung der Abhängigkeit geometrischer Gestalten von einander*, were far too weak to support the far-reaching researches he undertook during the later part of his career. It should also not be forgotten that Steiner's foundational work was far from complete when he turned to his more ambitious investigations of algebraic curves and surfaces. His *Systematische Entwickelung* of 1832 presented only part one of a study that had originally been projected as consisting of seven parts. Thus, even in his systematic investigations, Steiner's work remained in torso.

In the meantime, Plücker, Möbius, Cayley, Salmon, Hesse, and others had made impressive strides forward in algebraic geometry mainly by employing new analytic methods (homogeneous coordinates, determinants, and invariant-theoretic techniques). Thus, the much-vaunted synthetic style promoted by Poncelet, Steiner, and their followers looked hard-pressed when it came to matching the achievements set forth by analytic geometers who could appeal to a fourth dimension to explain what was meant by imaginary points. If the synthetic geometers hoped to compete with them, then somehow they would have to create a theory of imaginary elements within the context of Euclidean space. The problem of interpreting imaginaries within a purely synthetic framework eluded Jacob Steiner, but not his contemporary, Karl Georg Christian von Staudt (1798–1867), who succeeded in creating a precise method of handling the mysterious imaginary points and lines that had become so indispensible to geometrical research. As Felix Klein once commented:

> "... bei [Jacob] Steiner sind die imaginären Größen in der Geometrie noch "Gespenster", die gleichsam aus einer höheren Welt heraus sich in ihren Wirkungen bemerkbar machen, ohne daß wir von ihrem Wesen eine klare Vorstellung gewinnen können. Erst von Staudt hat ... jene Frage vollkommen gelöst ... "[6]

Since von Staudt, the central figure in our story, remains a barely known figure in the history of mathematics, I begin with a few remarks concerning his life and career.

[6] [20], 129.

2 Staudt's Theory of Imaginaries

K.G.C. von Staudt was born in the charming Franconian town of Rothenburg ob der Tauber. His parents were both descended from prominent patrician families whose ancestors had played major parts in Rothenburg's political and social life since the 16th century. The family home where he was born and raised dates from this period, and can still be seen in Rothenburg today. From 1817 to 1822 von Staudt studied in Göttingen, where he enjoyed the rare privilege of working closely with Carl Friedrich Gauss.[7] On numerous occasions, Gauss reported on the astronomical observations carried out by his young student, including those von Staudt made of Mars in 1820, the asteroid Pallas in 1821, and particularly his findings on the path of a comet that entered the solar system during that same year. After taking his doctorate in Erlangen in 1822 (apparently without having to submit a dissertation), he taught at Gymnasien in Würzburg and then Nürnberg before his appointment as Professor Ordinarius for mathematics in Erlangen in 1835.

Von Staudt retained this position up until his death in 1867. By all accounts, he was an extraordinarily dedicated teacher in an era when scholarly aspirations ran high and neohumanist ideals dominated the German universities. Still, Erlangen's academic environment certainly could hardly be compared with those in Berlin, Göttingen, or Königsberg. During the academic year 1858–59 only six students were studying for a degree in mathematics, a rather high number in view of the fact that the entire philosophical faculty recorded only nineteen auditors. The situation during the mid-1840's was even worse; then the philosophical faculty had just six students! Given these kinds of conditions, von Staudt clearly had little, if any, opportunity to expose young talents to his own research efforts. Thus, like many other German mathematicians of this era, he worked in quietude and virtual isolation.

After von Staudt's death his successor, the brilliant young mathematician and historian of mathematics Hermann Hankel (1839–1873), remained in Erlangen only two years before accepting a chair in Tübingen. This enabled von Staudt's closest disciple, Hans Pfaff (1824–1872), who had been *außerordentlicher Professor* in Erlangen since 1868, to assume his former

[7]The following information is based largely on [25].

mentor's position. Hans Pfaff was the son of von Staudt's predecessor, Johann Wilhelm Pfaff, and the author of *Neuere Geometrie* ([26]), published in Erlangen in 1867. This text was the first, following von Staudt's own, to expound the new theory of imaginaries.

Von Staudt's two principal works were entitled simply *Geometrie der Lage*, which appeared in 1847, and *Beiträge zur Geometrie der Lage*, the first installment of which came out in 1856 ([35], [36]). Both represent true milestones in the history of geometry. In his *Geometrie der Lage* von Staudt gave the first *systematic* synthetic presentation of the principal theorems of plane and solid positional geometry or, more precisely, affine geometry, since von Staudt took non-metric Euclidean space with its theory of parallels as his starting point. To this he appended the points at infinity where parallel lines intersect. These thus form a distinguished set of points, contrary to the purely projective standpoint which regards the so-called plane at infinity as indistinguishable from any other plane in projective three-space.

From the outset, von Staudt made use of topological concepts, such as parity or the lack thereof for curves and surfaces. Such concepts, along with the projective notion of harmonic separation, enabled him to uncover systematically a host of general theorems without making any appeal to metrical properties. It would be fair to say that with *Geometrie der Lage* the synthetic treatment of positional geometry emerged as a mature discipline. The well-known textbook with the same title by Theodor Reye (1866–1868, 2nd ed. 1886) served to anchor Staudt's achievement. Remarkably, Staudt's initial conception was quite traditional and partly inspired by the Euclidean tradition. He began by asserting that "geometry arises from the conception of an unbounded space," and then proceeded to introduce solid figures, surfaces, curves and points by means of general kinematical considerations. Thus, he observed that a moving point generates a curve, a moving curve a surface, and finally a body emerges from a moving surface. Von Staudt's style of argumentation throughout was only loosely deductive. In the main, he relied on what might well be called descriptive arguments that steadfastly avoided appeals to visual evidence, i.e. diagrams or pictures. Presumably the reader was expected to produce these himself, whether mentally or on paper, in the course of working through the text. This kind of minimalist approach, aimed at forcing active engagement from the reader, can be found in many synthetic texts of the nineteenth century, a feature that helps account for the nearly total lack of appeal this genre has today.

Already in the 1840s Staudt had hoped to develop a synthetic treatment of imaginaries based on elliptic involutions (about which more below). But, as he pointed out in the preface to *Beiträge zur Geometrie der Lage*, he was left stymied by the problem of how to distinguish between two complex conjugate points, which taken together yield the same involution. His breakthrough idea thus came sometime after 1847, and was elaborated beginning with the first installment of the *Beiträge* in 1856. To motivate his theory, he alluded in the preface to a particularly nice example of the insight that imaginary elements can bring to the theory of conic sections. Although he did not mention Newton in this connection, he was undoubtedly well aware that Sir Isaac had given a systematic treatment of the elements needed to construct a conic section in Book I of his *Principia.* There Newton carried out the constructions for all six cases in which any combination of five elements, whether points on the curve or tangents to it, are given. But Newton actually began with the easier case when two foci and a third point (or tangent line) are given, thereby implicitly raising the following question. Why do only three elements suffice in this case? Whether or not Newton ever asked himself this, we can be quite certain that he failed to find the following elegant answer, which depends on Plücker's insight, mentioned above, regarding the foci of conics. As Staudt pointed out, were we given only one tangent, knowledge of the two foci amounts to knowing four other (imaginary) tangents, since the foci are merely the points of intersection of the two pairs of tangents from the circular points I, J to the curve, so that five elements are, in fact, given, thereby resolving the mystery!

The theory of involutions, which von Staudt took as the basis for his theory of imaginaries, already formed a standard part of projective geometry in his day. Several properties of involutions play an important part, for example, in the work of Desargues. Simply stated, an involution of the points on a line is a projective mapping T for which $T^2 = Id$. Such a mapping can have either zero, one, or two fixed points, in which case one speaks, respectively, of an elliptic, parabolic, or hyperbolic involution. In the first case, an analytic representation of the mapping T sending $x \longrightarrow x'$ is given by

$$(x-a)(x'-a)+b^2=0,$$

where $a, b \in \mathbb{R}$ and $x \in \mathbb{C}$. From this, one sees at once that the two (missing) fixed points are given analytically by $x = a \pm bi$, which accounts for the representation of these conjugate imaginary points by means of the above elliptic involution on the real line "joining" this pair.

Staudt's basic idea was thus to consider the real line ℓ joining two conjugate imaginary points P and $\bar{P}$, and then to associate this pair of points with two *oriented* involutions of ℓ having P and $\bar{P}$ as fixed points. To make his argument as transparent as possible, we make use of the analytic presentation first given by Otto Stolz in 1871 ([38]). Thus, given an imaginary point P with homogeneous complex coordinates (ζ, η, τ) one writes:

$$\zeta = \zeta_1 + i\zeta_2, \quad \eta = \eta_1 + i\eta_2, \quad \tau = \tau_1 + i\tau_2.$$

These determine two real points P_1, P_2 with homogeneous coordinates:

$$P_1 = (\zeta_1, \eta_1, \tau_1); \qquad P_2 = (\zeta_2, \eta_2, \tau_2).$$

Since $P_1 \neq P_2$ (otherwise P would be a real point), P_1 and P_2 determine a *real* line $\overline{P_1P_2}$ given by

$$P = \{\zeta_1 + \lambda\zeta_2, \eta_1 + \lambda\eta_2, \tau_1 + \lambda\tau_2\}.$$

Here it should be emphasized that the parameter $\lambda \in \mathbb{C}$, so the line $\overline{P_1P_2}$ passes through the *conjugate imaginary point* $\bar{P}$ with coordinates:

$$\bar{\zeta} = \zeta_1 - i\zeta_2, \quad \bar{\eta} = \eta_1 - i\eta_2, \quad \bar{\tau} = \tau_1 - i\tau_2.$$

Stolz showed that from the given point pair $\{P_1, P_2\}$ one can write down a formula for the other corresponding point pairs of the involution T in terms of a parameter λ:

$$P_1' = (\zeta_1', \eta_1', \tau_1') = (\zeta_1 + \lambda\zeta_2, \eta_1 + \lambda\eta_2, \tau_1 + \lambda\tau_2)$$

and

$$P_2' = (\zeta_2', \eta_2', \tau_2') = (\zeta_1 - \tfrac{1}{\lambda}\zeta_2, \eta_1 - \tfrac{1}{\lambda}\eta_2, \tau_1 - \tfrac{1}{\lambda}\tau_2).$$

The oriented involutions on $\overline{P_1P_2}$ are then produced by letting λ vary. Following Staudt's convention, the point P is associated with the orientation obtained by letting λ increase continually; the opposite orientation, given by decreasing values of λ, yields a representation for $\bar{P}$.

Now von Staudt showed that the totality of point pairs $\{P_1', P_2'\}$ determined by the imaginary point P produces an involution T of the real points of $\overline{P_1P_2}$.

The fixed points (which Staudt called fundamental points) of T are just the imaginaries P and $\bar{P}$. Conversely, taking any two pairs of points $\{P_1, P_2\}$ and $\{P_1', P_2'\}$ on a real line ℓ one obtains a unique involution T. The fixed points of T will be imaginary if and only if these two pairs *overlap*. Thus, one of the points P_1', P_2' must lie between $\{P_1, P_2\}$ and the other outside the latter pair if the involution is to represent two conjugate imaginary points.

This novel idea would have remained nothing more than a curiosity had it not been possible to find a suitable interpretation for imaginary lines as well. And precisely at this juncture one can begin to appreciate the true beauty of von Staudt's theory: all he had to do was appeal to duality. Since, in the projective plane, an oriented involution T of a real line ℓ dualizes as an (oriented) involution of the pencil of real lines through a real point, it follows that the latter yields a representation for imaginary lines by means of points in $\mathbf{P}^2(\mathbb{R})$. Analogous to the situation above, in order to correspond to an imaginary line the pencil involution must contain no real double rays. This results if two pairs of corresponding lines always overlap with one another.

In this new context, one can now consider the possible *incidence relations* between points and lines. The following four cases arise:

1. real point and real line (the familiar situation in $\mathbf{P}^2(\mathbb{R})$);
2. real point P and imaginary line ℓ (since $P \in \ell \Rightarrow P \in \bar{\ell}$ it follows that P must be identical with the vertex of the line pencil representing ℓ);
3. imaginary point P and real line ℓ (the line ℓ must be identical with the line ℓ_P that represents P);
4. imaginary point P and imaginary line ℓ (in this case the involution on ℓ_P determined by P will be in perspective with the involution of the line pencil determined by ℓ).

Using these basic constructs, one could, in principle, not only distinguish between real and imaginary figurations in $\mathbf{P}^2(\mathbb{C})$) but also determine which entities within a known incidence structure were real and which imaginary. To see how this works, we consider how von Staudt employed the polar relation induced by a conic C in order to distinguish the real and imaginary points of the conic. For this purpose, we take an arbitrary point P lying on the line ℓ. From the polar relation induced by C, one obtains the polar line p' corresponding to P, and with it a new point P' given by $p' \cap \ell = P'$. Since the polar of P' passes through P, the relationship between P and P'

is reciprocal, and hence this procedure induces an involution on the points of ℓ.

By definition, the points of C consist of those points P that lie on their own polars. This property also characterizes the points of $C \cap \ell$. Thus, the two points where ℓ meets C remain fixed; they are the *fundamental points of the involution* on ℓ where the point pairs (P, P') satisfy $P = P'$. In this fashion, von Staudt reinterpreted the question of whether or not the point $P \in C$ is real or imaginary into a question about the involution induced by the polar system of C on an arbitrary line ℓ passing through P. In particular, an imaginary point P on the conic C was represented by an elliptic involution induced on a real line ℓ.

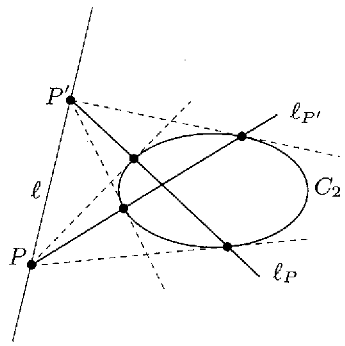

Figure 7: The involution on ℓ pairing P with P' is elliptic.

Analytically, Staudt's construction can be carried out as follows. For a conic C in $\mathbf{P}^2(\mathbb{C})$ with *real coefficients* given by

$$Ax^2 + 2Bxy + Cy^2 + 2Dxz + 2Eyz + Fz^2 = 0,$$

$$A, B, C, D, E, F \in \mathbb{R}; \quad (x, y, z) \in \mathbb{C}^3 - (0, 0, 0)$$

the polar system is:

$$Axx' + B(xy' + x'y) + Cyy' + D(xz' + x'z) + E(yz' + y'z) + Fzz' = 0.$$

If $P = (x', y', z') \in \ell$, then plugging into the polar equation one obtains a linear equation for p'. Clearly $\ell \cap p' = P$ if and only if $P \in C$. It should be pointed out, again, that von Staudt employed purely synthetic methods to describe these constructions.

3 Imaginaries after von Staudt: Lie and Klein

Von Staudt's achievements and their importance have been praised over the years by such eminent geometers as Felix Klein, Julian Lowell Coolidge ([6], 96–100), and Hans Freudenthal ([7]). Still, his theory was more readily admired from afar than it was closely emulated by leading synthetic geometers. Von Staudt's uncompromising style, while fully in keeping with the dictates of the trend-setting Steiner school, proved a major obstacle for the assimilation of his ideas. One of the first to appreciate this situation was the Austrian mathematician Otto Stolz (1842–1905). Thus, in 1871, Stolz gave the first analytic presentation of Staudt's theory of the imaginary in [38].[8] He also introduced Felix Klein to von Staudt's ideas when they met in Berlin during the winter semester of 1869–70, and also in Göttingen during the summer of 1871.

By this time Klein had also become friends with the Norwegian mathematician Sophus Lie, whom he also met in Berlin. Soon afterward Klein and Lie gained a clearer idea of the methods of French metrical geometers during a three-month sojourn in Paris. This French style, promulgated by the so-called anallagmaticians, including Darboux, Moutard, and Laguerre, was based on the idea of taking circles in the plane or spheres in space as the fundamental elements and employing appropriate coordinate systems to study the geometric objects that arise as envelopes of circles and spheres. The cyclids of Dupin, for example, provided a classical model for the latter. Lie and Klein were both deeply inspired by the possibility of finding a direct connection between this French tradition and the mostly German research on line geometry that had begun with Möbius but which received its main impetus through the work of Plücker (see [29]). Both traditions made heavy use of imaginary elements.

Initially, the problem of interpreting imaginary points by means of real geometric structures played a central role in Lie's mathematics. Indeed, his very first work presented a new theory in which points in the complex projective plane $\mathbf{P}^2(\mathbb{C})$ become associated with certain line congruences in $\mathbf{P}^3(\mathbb{R})$ ([29],

[8] Stolz wrote about the poor reception of von Staudt's work: "Die Untersuchungen dieses scharfsinnigen Geometers, welche nach rein geometrischer Methode ausgeführt sind, haben, so viel mir bekannt ist, bisher in der analytischen Geometrie keine Berücksichtigung gefunden" [38], 417.

224–226; and [22]).[9] This kind of "anschauliche" approach to imaginary figurations clearly had a great deal of appeal to both Klein and Lie. But whereas Lie soon freed himself from such concerns and came to feel entirely at home with the geometry of higher-dimensional complex spaces, Klein felt driven to unravel the relationship between real and complex structures. In fact, he emphasized the importance of so-called "Realitätsverhältnisse" throughout much of his career. Moreover, Klein's pursuit of a universal framework for geometry led him to recognize the significance of von Staudt's systematic methods, which he exploited in his well-known work on the projective basis for non-Euclidean geometries ([12]).

We have noted already how Klein learned about von Staudt's researches through Otto Stolz.[10] As a *Privatdozent* in Göttingen, Klein even lectured on von Staudt's theory of the imaginary and thereby contributed to the dissemination of these ideas. One of the students who attended his lectures was Ferdinand Lindemann (1852–1939). From the latter's unpublished memoirs we learn that this experience occasioned Lindemann to pursue von Staudt's theory himself and to include it in the second edition of Clebsch-Lindemann, *Vorlesungen über Geometrie.*[11]

When we hear of Klein's activities in Erlangen today, the first (and probably only) notion that is likely to come to mind is the famous *Erlanger Programm* that he wrote on his arrival in 1872. Curiously enough, however, during his tenure in Erlangen Klein did little to follow up on the ideas he outlined in his *Erlanger Programm.* Instead he concentrated most of his efforts on problems related to the role of imaginaries in algebraic geometry.[12] As a true disciple of Plücker, Klein hoped to shed light on the geometrical *Gestalt* of algebraic curves and their associated Riemann surfaces. This led him to develop what he called a "new type" of (projective) Riemann surface (see [14], [16], [17], [18]). The possibility of exploiting Riemann's function theory in algebraic geometry had been realized ten years earlier by Alfred Clebsch in [4]. Klein,

[9] Not long afterward, Gottlob Frege set forth an alternative theory for treating imaginary geometric figures in his Göttingen doctoral dissertation (see [31]).

[10] Klein himself later acknowledged the importance of the conversations he had with Stolz when they were together in Göttingen during the early 1870's ([19], vol. 1, 51–52).

[11] "[Klein] lud mich ein, seine Vorlesung zu besuchen, und das gab wohl die Veranlassung, daß ich diese Imaginär-Theorie in meiner späteren Bearbeitung von Clebschs Vorlesungen mit grosser Ausführlichkeit unter Hinzufügung eigener Untersuchungen dargestellt habe." ([23], 45)

[12] For more details, see [30].

however, found Clebsch's approach unsatisfying, particularly because he had not followed Riemann's lead with regard to the notion of genus, the fundamental topological invariant in Riemann's geometric approach to complex analysis. Yet Klein also realized that conventional Riemann surfaces were not well suited to the purposes of algebraic curve theory, which requires that the curves be situated in $\mathbf{P}^2(\mathbb{C})$ in order to exploit the advantages of Bézout's Theorem. This circumstance motivated him to set about looking for a projective analogue to conventional Riemann surfaces, which were then constructed over $\mathbb{C}$ or $\mathbb{C} \cup \{\infty\}$. Klein used his projective Riemann surfaces to obtain a variety of results, some of them new, regarding the number of real and imaginary configurations in which conics are tangent to a given quartic curve (see [16] and [18]). In none of these papers, however, did he draw any explicit connections with von Staudt's theory of the imaginary.

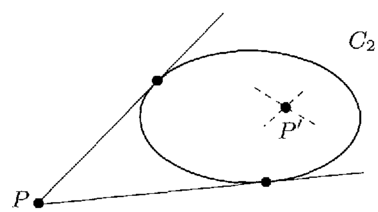

Figure 8: For points $P' \in \mathrm{Int}(C)$ one has two conjugate imaginary tangents. Klein viewed these, following Staudt, as representing two oriented involutions of the pencil of real lines passing through P'. By continuity, he regarded these as corresponding to imaginary points on two leaves of a Riemann surface that joined together along C. The result was a topological sphere, a surface of genus zero.

Nevertheless, in another paper from the mid-1870's, entitled "Über den Zusammenhang der Flächen," ([15], 479–480) he made some brief remarks that reveal how the two theories dovetailed together very nicely. Klein's main concern in this paper was to give a method for determining whether or not a surface was orientable and to emphasize that this was an *intrinsic* property independent of any embedding in space. As his principal tool, he introduced the notion of an *indicatrix,* a local pencil of oriented curves passing through a point P of the surface. This served to provide a local orientation—if by moving P anywhere around the surface this orientation remained preserved then the surface was orientable. Klein also emphasized the close connection between his *indicatrix* construction and von Staudt's

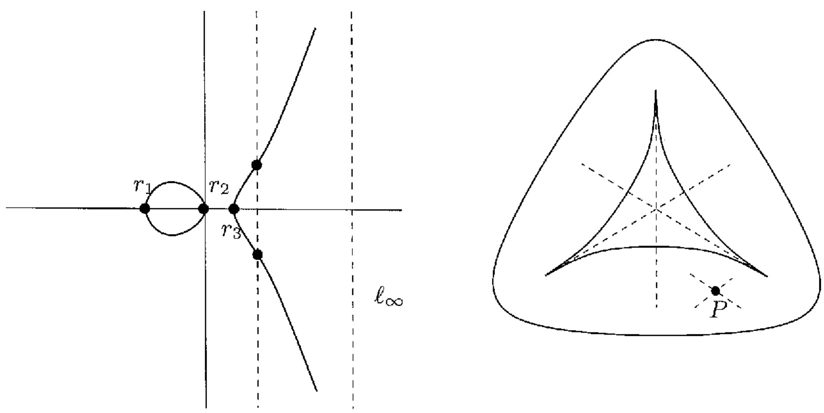

Figure 9: Klein's projective Riemann surface for a nonsingular cubic C with two real components. The three real inflection points (one of which lies at infinity) are transformed into cusps in passing to the dual curve C^*. This dualized curve has class 3, so that three tangents to C^* pass through each point in the plane. In the outer and inner regions of the plane three real tangents pass through an arbitrary point, whereas only one tangent can be found for points in the annular region. The "missing" imaginary tangents correspond to points on two leaves of a Riemann surface of genus one, a topological torus.

theory of imaginary lines as represented by oriented involutions on pencils of lines. In a projective setting, he noted that such an oriented involution I_P can be localized by constructing a small conic about P whose conjugate diameters correspond to the reciprocal lines of I_P. This yields a directrix that one can then move along curves of a projective surface.

Klein's key insight here lay in recognizing the compatibility of these ideas, borrowed from von Staudt, with his projective Riemann surfaces. The latter, he noted, are always orientable due to the fact that, by construction, the two possible orientations at a point P are realized as two distinct points that lie on different sheets of the surface.

Certainly much of this must be regarded as idiosyncratic mathematics, for it left no discernible mark on geometrical research after the turn of the century. Nevertheless, even after the publication of Hilbert's *Grundlagen der Geometrie* ([11]) in 1899, various theories for treating imaginary geometrical structures continued to fascinate a handful of geometers.[13] As the modern, axiomatic standpoint grew stronger, however, the ontological issues that originally motivated mathematicians to investigate possible relationships between real and imaginary structures eventually lost their urgency. Klein's journey may be regarded as one of the last to reach the frontiers of research in algebraic geometry mounted on arguments that he could convey in a picture. The reliance on *anschauliche* approaches, which Klein often promoted, eventually came under sharp attack.

In 1917 Eduard Study leveled a harsh assault on the sloppy, even fallacious arguments set forth by Lie and Klein in a review article published in the *Jahresberichte der deutschen Mathematiker-Vereinigung* ([41]). Study, who had habilitated in Leipzig during the mid 1880s under Klein's supervision, saw himself, and rightfully so, as the leading representative of German algebraic geometry in the tradition of Plücker and Clebsch. His was no wholesale critique of *Anschauung*, and to make his case for the importance of *anschauliche* arguments in geometry he even quoted Poincaré with strong approval. Yet Study had no patience with murky thinkers like Lie and Klein who, to his mind, refused to recognize the importance of systematic investigations undertaken with care for the details. He considered Lie's sphere geometry a particularly glaring case in point, but ten years earlier he also published a similarly devastating critique of certain results in Lie's theory of differential invariants ([40]).

In the case of Klein, Study portrayed him as having betrayed the principles of his own *Erlanger Programm*. Having accompanied Klein during his trip to Chicago in 1893, Study had witnessed up close how his former mentor skillfully exploited the opportunity to make propaganda for his *Erlanger Programm*. He also knew full well that Klein had done very little in pursuit of this program during the intervening twenty years. As a champion of modern invariant theory, Study saw his own work as fully in accord with the philosophy of Klein's *Erlanger Programm*—namely that geometry should be understood as the systematic study of those invariants associated with a given transformation group. Yet Study's sweeping criticisms of Klein's detrimental influence on geometry in Germany seem to have impressed very few of his contemporaries, despite the fact that much of what he asserted would have been difficult to refute. Even his leading pupils, Wilhelm Blaschke and

[13] See, for example, [32]. The dissertations of two Harvard geometers, J. L. Coolidge and William Graustein, also dealt with imaginaries in geometry.

Julian Lowell Coolidge, were among Klein's most avid admirers, probably contributing more to maintaining his reputation than they did Study's own. At any rate, we can see from this example, as I suggested at the outset, that very little is known about even the most famous protagonists of various approaches to geometry during the nineteenth century and beyond. Surely the time is ripe for deeper and more detailed historical investigations.

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