THE SCIENTIFIC LIFE AND WORKS OF H. C. ØRSTED

BY KIRSTINE MEYER.

The purpose of the present essay is to survey the scientific papers of *H. C. Ørsted* and to give an account of the conditions under which they appeared, as well as their relation to and influence upon contemporary physical science. It is hoped that in this way light will indirectly be thrown upon the personality of their eminent author.

Ørsted was born at Rudkøbing in 1777. His father was an apothecary. With his brother, *A. S. Ørsted*, who was his junior by one year, and who was later to become famous as a jurist, he received instruction of a very desultory and casual kind, but the very circumstances under which it was achieved will contribute to the understanding of his development. Their parents were so much occupied with the management of their large household and business that they placed the brothers for the greater part of the day during their early years, with a couple of »poor but very worthy citizens«,[1] a German wig-maker and his wife, who soon began to give them lessons in order to keep them employed. The wife taught them to read, the husband to speak German. Luther's catechism with *Pontoppidan's* explanations they learned by heart, they read and translated a German Bible, learned writing from German copies, and as much arithmetic as the wig-maker himself knew he taught his pupils, namely to add and to subtract.

The brothers learned easily and had excellent memories. Their desire for knowledge being only sparingly satisfied by the wig-maker and his wife, they sought other means of satisfying it. Their friends soon discovered how bright and eager to learn they were; this excited admiration and induced everybody to help them to acquive such knowledge as they themselves possessed: an older schoolfellow taught them multiplication, a friend of the family division. The burgomaster gave one brother lessons in English, the other in French. They received regular but »indifferent« instruction in Latin. Besides, »they seized with avidity all other means of gaining knowledge that presented themselves,«[2] and

[1] *H. C. Ørsted's* Autobiografi, (=Autobiogr.) *Kofod's* Konversationslexikon. Vol. 28. Kbhvn. 1828. P. 516. [2] Autobiogr. P. 517.

by reading acquired information in manifold and, for children, often strange domains. They invariably worked together so that what was learned by one was always imparted to the other. For every fresh piece of knowledge they gained their energies were stimulated and their ambition roused by the growing admiration of their friends and as, in the course of conversation, they often had an opportunity to give an account of their reading, a taste for authorship was formed. This inclination further showed itself in sermons that they wrote for their mutual edification. *H. C. Ørsted* also wrote poems. Their ability to work was at the same time more soberly employed in their father's business. Both brothers helped him in his pharmacy from their 10th and 11th year at the time that he had no assistant. The younger brother was allowed to give up this work, but *Hans Christian* continued it, and in this way gained a knowledge of chemistry and experimental work which served as a good foundation for his later studies. The wig-maker's teaching, too, may in one respect have predetermined his interests; the Key stone of this tuition was Scripture with scholastic and theological commentations, such as often induce a taste for systematic-philosophical interpretations. Already in childhood and early youth this kind of philosophical interest may be perceived in the brothers.

Thus influenced they arrived in Copenhagen in the spring of 1794 to finish their preparation for »the first academic examination« which they passed the same autumn with honours. They were accustomed to a small and humble community, to a life full of work in which learning had been their greatest pleasure and had given them their special position. The poet *Oehlenschläger* has recorded how this way of life was continued in the larger sphere. *Oehlenschläger* did not meet them until 1797. They were then leading a secluded life and always together, »in long yellow greatcoats they walked arm in arm.« »As in a dim monastic cell the *Ørsteds* sat here, grave, silent, at their studies.« »To all their fellow-students they shone resplendent like Dioscuri, and even ripe scholars soon noticed what was in them. In academical prize essays and the award of gold medals the fruits of their mind and industry appeared.«[1]

They led an unassuming life; were admitted to *Elers'* College;[2]

[1] *Oehlenschlägers* Erindringer. Vol. I. P. 136. Kbhvn. 1850.

[2] This college, which still provides free residence and a small scholarship for undergraduates and graduates of the University of Copenhagen, was founded in 1691 by *Jørgen Elers*.

dined gratuiously with an aunt, and soon began to act as tutors to candidates for the philosophical examination so that they were no burden to their father.

In their first year as undergraduates they attended *Riisbrigh's* lectures. *Riisbrigh* was the »exponent and approver of *Kant's* philosophy.« They also had other opportunities of hearing *Kant's* works expounded. They discovered at mathematical lectures that mathematics was not the puzzling mystery they had hitherto imagined. These and lectures on astronomy and physics attracted *H. C. Ørsted* to the study of science, while philosophy remained the chief interest of his brother who some years later »gave the critical philosophy in Denmark its first firm foundation.«[1] The intimate co-operation of the brothers continued, for even after they had to some degree chosen special fields, an interest in philosophy was engendered in *H. C. Ørsted* which was soon manifested in his writings and which remained with him throughout life.

H. C. Ørsted's first published work and first attempt at scientific authorship was a paper for the University Prize Competition in Esthetics for 1796 for which the prize was awarded him. His essay was published in »Minerva«.[2] His childhood's interest in poetry had developed and led to this first publication, and the same interest continued through his life and indirectly stamped the language of his scientific publications and speeches. Both in his written and oral style there is a certain elaborate elegance through which we feel his pleasure in moulding language according to his taste. This formal power and esthetic interest without doubt played a great part in his marked inclination for scientific communication. »I promised you in our last conversation to give you an account in letters of the systematic part of chemistry . . . . . I keep my promise with pleasure, both for your sake and that of science, which you know I find so much pleasure in communicating to others,«[3] he writes in 1798, and in the many lectures he gave with untiring energy in the course of his long life he had ample occasion to indulge in this »pleasure«.

Even in his first attempt at a work in the domains of science, a medical prize essay on the origin and use of the liquor amnii (1797),[4] his power of moulding language is very conspicuous,

[1] *N. M. Petersen:* Bidrag til den danske Literaturs Historie. Vol. Va. P. 139. Kbhvn. 1870.
[2] »Minerva«. Maj 1797. Kbhvn. [3] Chemical Letters 1798. This Edition (= Ed.) Vol. III. P. 3.
[4] Ed. Vol. I. P. 3.

and from the wording of the judgment[1] on this paper we feel that its style made a favourable impression on the board of examiners. The experimental part of the paper, a chemical investigation of the amniotic fluid, falls naturally into line with his other studies in this period, since in 1797 he took his pharmaceutical degree with high honours. The paper bears witness to wide reading and, as it was awarded the prize, its contents may be considered a testimony to the standard of contemporary science. After the treatment of these minor problems for the undergraduate, *Ørsted's* real, independent, scientific career began. From the very first it separated into two main currents, philosophical, and experimental; they divided his interest and his work; sometimes they ran side by side, and sometimes they united in the same work. One current might at times predominate over the other, but could never entirely suppress it. In the first period of his scientific life, when the philosophical current was predominant, he estimated the value of each experimental result mainly by its bearing on his favourite philosophical ideas. Still, in this period his experimental work had the effect of taking him back to sober ground again when Philosophy had tempted him too far into the realms of imagination; it was the study of chemistry that led him to experimental science, it was *Kant's* critical philosophy that led him into philosophical roads.

In 1798 a periodical was started entitled »Philosophisk Repertorium for Fædrelandets nyeste Litteratur«, the purpose of which was to uphold *Kant's* philosophy against its many assailants. In the course of its first year the *Ørsteds* became members of the editorial staff. The programme of the periodical contains a statement characteristic of many of *H. C. Ørsted's* reviews and critiques, namely, that the magazine will endeavour to provide »reading of independent interest« so that it may be »read with interest and advantage independently of the writings which it reviews and criticises.« In accordance with this programme *H. C. Ørsted's* many reviews, »chemical letters«, and the like, from these years, will be found to contain something more than mere accounts; through them we are able to see his own point of view towards science and philosophy and find independent remarks; therefore, in order to know his work and views it is not enough to read those of his writings which are

[1] Ed. Vol. I. P. 3.

directly designated as scientific works, we must also seek out occasional papers and semi-popular accounts. All these writings leave his study in finished style and bear the stamp of what dominates his ideas at the moment.

The Philosophical Repertorium had a sad fate and a brief existence. *H. C. Ørsted* wrote a paper for it which is not in the first volume but appeared separately in 1799 bearing the title of »Grundtræk af Naturmetaphysiken tildeels efter en ny Plan«[1] (Fundamental Features of Metaphysics, partly on a New Plan). The same subject is more elaborately treated in his thesis for the doctorate[2] from the same year. These works contain in substance a critical account of *Kant's* »Metaphysische Anfangsgründe der Naturwissenschaft« with some suggestions for an improved systematology; they have acquired no lasting value; for his contemporaries they may have been of importance from their clear statement of the fundamental metaphysical problem: What is the *a priori* base of science which is the necessary presupposition of all experience, and what laws governing matter and its motion may accordingly be *a priori* deduced? That these treatises met with appreciation is seen from the fact that they were amalgamated into one volume and with some additions published in German in 1802.[3] Their editor and adaptor, *Mendel*, states in the preface that his »als scharfsinniger Denker und Gelehrter rühmlichst bekannter Freund, Herr Doktor *Ørsted* in Copenhagen«, has communicated to him several new things for this edition and that he is still working at improvements in the same field. Of course we cannot see how much in the additions to the treatise is due to *Ørsted* and how much to *Mendel*, the latter, however, says in the preface that in a letter to him *Ørsted* has expressed a wish to be judged only according to the »architectonic« alterations he has suggested in *Kant's* metaphysics, not according to the separate propositions and rubrics. Hence it is possible that an »Anhang« in the German edition is due to *Mendel*, not to *Ørsted*. This »Anhang« shows how greatly the »critical philosophy« had become opposed to its name, an attempt being here made to give an *a priori* proof that there must exist motions in nature in which a particle performs periodic revolutions round a fixed body

[1] Ed. Vol. I. P. 33. [2] Ed. Vol. I. P. 83.

[3] Ideen zu einer neuen Architektonik der Naturmetaphysik, herausgegeben von *Mendel*. Berlin 1802.

under the influence of a central force of attraction from the latter, and under the influence of the impact of particles emanating from the central body and giving tangential force. Further, from the existence of this motion the *a priori* inference is drawn that space is filled with liquid! It is not improbable that the German school of Nature philosophy is responsible for so fanciful an »Anhang«. This school of philosophy had developed with *Kant's* metaphysics as the original starting point and — to use *Ostwald's* words[1] — »ravaged Germany like a plague in the first years of the 19th century.«

Still, *Ørsted* was on his guard against the most typical representatives of this school. In 1799 he writes about two of *Schelling's* works: »These two books no doubt deserve attention for the beautiful and great ideas we find in them, but on account of the not very rigorous method by which the author intermingles empirical propositions without sufficiently distinguishing them from *a priori* propositions the book is robbed of much of its value, especially as the empirical propositions adduced are often utterly false.«[2]

If we consider what became of importance to *Ørsted's* own development from his »Kantian period«, we mainly notice two maxims in his writings of the succeeding years: 1) For a law of nature to be absolutely valid it must have an *a priori* foundation. 2) From the conception of matter it follows that any theory of atoms is illogical. Further he acquired an inclination and a capacity for systematical exposition which characterises his more considerable works for the next 10 or 15 years.

In the meanwhile, in conjunction with these philosophical interests, *Ørsted* pursued his own special study of the physical sciences, doing not merely the duty work needful to pass an excellent pharmaceutical examination. Through the means at hand he no doubt procured as extensive information as possible both theoretically and practically. As regards the pursuit of his theoretical studies, his reviews, »chemical letters«, and notices of the publications of foreign chemists in various magazines from 1798 to 1800 bear witness to his interested work in this direction. Among the rewiews and notices published in 1800 one should be specially noted, viz. a review[3] of *Fourcroy's* Chemistry adapted by *Gadolin* under the title of In-

[1] *W. Ostwald* Vorträge u. Reden. Leipzig 1904. P. 368. [2] Ed. Vol. I. P. 77. [3] Ed. Vol. III. P. 51.

troduction to Chemistry. When speaking of the classification of the bodies in chemistry *Ørsted* for the first time introduces a view which later was elaborated by him and lead him to conceive the bases and acids as belonging to the same group or series, the characteristic reactions for acidity and basisity occurring in so many different degrees that it should be possible to arrange their compounds according to these degrees, e. g. according to decreasing acidity, increasing basisity. It was a time of ferment in the science of chemistry. »When as a boy he read books on chemistry none of which were quite modern, their whole base was the phlogistic system; as a young undergraduate he became acquainted with the antiphlogistic system and was quite fascinated by it; before he was 24, however, *Volta's* great discovery, *Ritter's* brilliant works, *Winterl's* bold edifice of principles, had induced the conviction in him that the antiphlogistic doctrine could not be valid.«[1] This conviction is expressed already in the latter part of »Grundtræk af Naturmetaphysiken« (Fundamental Features of Metaphysics).

As regards experimenting he was no doubt in the main restricted to the chemical training he received in the pharmacy in connection with his studies. The university of Copenhagen was at that time only badly equipped for experimental studies. It possessed no collection of physical instruments. There were certain amateurs who owned physical apparatus, and *Ørsted* may have been able to see and perform experiments in their laboratories. The largest private collection was in the possession of Overhofmarskalk *Hauch*, but, according to his own words, *Ørsted* had no access to it while an undergraduate. In those years, however, he became acquainted with Professor *Manthey* who held the chair of chemistry from 1793 and was also the owner of the »Lion Pharmacy« where *Ørsted* may have received his pharmaceutical training.[2] At any rate in 1807 Professor *Manthey* was the owner of a valuable collection of physical instruments, and he will probably have been in possession of such a collection already in *Ørsted's* undergraduate years. As *Manthey* took a great interest in *Ørsted* and aided him in every possible way, it is not improbable that he allowed *Ørsted* to use his

[1] Autobiogr. P. 526.

[2] This would seem to appear from *Hauch's* commemorative oration on *Ørsted*, but the circumstance speaks against it that the Lion Pharmacy was burnt down in 1795 and for the first time inspected by the authorities in 1799. *Rud. Jørgensen:* Løve Apotheket i Kjøbenhavn 1620—1908. P. 11—13.

apparatus and also in this way contributed to his scientific development.

The only partly experimental work published by *Ørsted* before 1800 is the prize essay on the liquor amnii[1] where the experimental part is of a chemical nature. Not before 1800 when »*Volta* had set up his pile as a landmark between the physics of the old and the new century«,[2] do we find traces in his writings that he is taking up experimental work in physics.

From 1800 to 1801, while *Manthey* was abroad, *Ørsted* was manager of the Lion Pharmacy, and from his letters to the former we see that he had no opportunity of experimenting with the *Voltaic* pile until the beginning of 1801. In a letter dated February 23rd[3] he tells *Manthey* that he has seen *Hauch* make experiments with a battery of 600 plates of silver and zinc, and gives an account of an experiment he himself has made with a battery of 60 plates of zinc and black-lead. He published some of his results in the 1st volume of »Nyt Bibliothek for Physik, Medicin og Oekonomi«[4] (New Library for Physics, Medicine, and Economics) and tells of others in two letters[5] to *Manthey* of February 28th and May 14th 1801. As far as we can see from these casual and not very detailed remarks, *Ørsted* here from the very beginning, in what may be considered as his first work on physics, exhibited some features that characterise all his works in this field of research. He readily gets ideas for experiments and apparatus, and he often obtains a striking and significant result, but he does not follow up the matter, and so it has happened more than once that his merits have been obscured by others who either used his results as the starting point of their own researches, or overlooked them, because he did not sufficiently elaborate them.

We shall now give an account of *Ørsted's* first experimental results.

As soon as the *Voltaic* pile had become known, *Nicholson*, *Carlisle* and *Ritter* found the chemical action of its current and saw the decomposition of water into oxygen and hydrogen. By way of explaining the observed phenomenon *Ritter* then advanced the theory that water was not a compound, but that water + negative

---

[1] Ed. Vol. I. P. II. [2] Autobiogr. P. 521.

[3] Breve fra og til *Hans Christian Ørsted* udgivne af *Mathilde Ørsted*. Vol. I. P. 11. Kbhvn. 1870. (=M. Ø.) [4] Ed. Vol. I. P. 106. [5] M. Ø. Vol. I. P. 13—14.

electricity produced hydrogen, and water + positive electricity became oxygen. This theory led *Ørsted* to the following argument: the forming of hydrogen by the decomposition of a metal in dilute acid may possibly be due to the generation of negative electricity through the dissolution of the metal. Hence we should expect that galvanism might be produced by the dissolution of metals. He put this idea to the test by constructing a u-shaped tube element with a lead amalgam in the bend and one branch filled with dilute sulphuric acid; a silver wire was plunged in the acid and an iron wire connected to the amalgam. From 7 of these cells a feeble battery was obtained. He now placed a piece of zinc in each branch filled with the sulphuric acid and found that the battery acted powerfully. Hence he imagined the reason to be that while the zinc was dissolving »galvanism« was produced, whereas the real reason is no doubt that a zinc amalgam pole is produced instead of a lead amalgam pole. Here *Ørsted* touched upon the fundamental question of the cause of »galvanism«, but he did not follow up the matter. He experimented with the small battery showing it to any one who professed an interest in it, and as he gave a description of it in a treatise in a German periodical[1] it drew attention to his name; on a journey in Germany the year after he often had to show experiments with it.

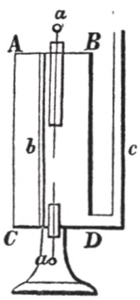
An observation in his treatise calls for further explanation.[2] He mentions his »*Voltaic* apparatus« made of plates of black-lead and zinc where the black-lead is not pure, and remarks that the thickness of the plates is not without importance. »Of this I was convinced by the fact that I obtained a greater effect with thick than with thin lead plates, and our *Abildgaard*, whose loss we must now regret, found no effect whatever from lead plates even thinner than those used by me.« If we assume that »lead plates« is an abbreviation for »black-lead« plates, the apparently strange observation becomes intelligible, the thin black-lead plates being no doubt so porous that there has been a layer of liquid between all the plates.

A very essential point was touched upon by *Ørsted* in these first experiments. He had no instrument to measure galvanism, so he constructed an apparatus for this purpose which he

[1] Ed. Vol. I. P. 106. [2] Ed. Vol. I. P. 108.

briefly refers to in the above-mentioned German periodical, and describes more fully in a letter[1] to *Manthey* of April 28th 1801.

»Its main parts are *ABCD,* a glass cylinder filled with water, *aa* 2 very thin gold wires cemented into glass tubes and able to be pushed backwards and forwards in leather without admitting the air to the interior of the cylinder, *b* a glass scale, *c* a glass tube which is not filled with water yet communicates with *ABCD.* The farther the gold wires are from one another the greater is the force required to evolve water,[2] the nearer, the less. With an exceedingly small distance between the points of the wires I got air by means of only four black-lead and lead[3] plates, although their surfaces were rather calcined. The tube *c*, to which also belongs a scale, serves to show how much air the galvanism has evolved in a certain specified time as the water in the same must of course rise in proportion. I think that by this means we shall be able to measure galvanism even more accurately than electricity.«



Another very important result was obtained from these first experiments. On May 14th 1801 *Ørsted* writes in a short letter to *Manthey*:[4] »Only this much I must tell you that I have succeeded in staining syrup of violets green by the negative and red by the positive galvanism«, and in a paper on »Fortsatte Forsøg med Galvanismen«[5] (Continued Experiments on Galvanism) in »Nyt Bibliothek for Physik« (New Library for Physics) for 1801 he makes the same statement and adds that »on shaking the colour disappears« just as when you mix a quantity of syrup of violets stained green by alkali with another that has obtained a red colour from an amount of acid proportional to it, which, as it is well known, is a consequence of the fact that alkali and acid saturate each other and neutralise each other's effect.«

Ørsted occasionally refers to one or two of the results of his galvanical experiments to which he evidently attaches some importance. In a general survey of the latest departures in physics in *Schlegel's* »Europa«[6] for 1803 he mentions that he was the first to show that the *Voltaic* pile acted in a space with rarified air, and moreover that »*Ørsted* and *Davy*« were simultaneous in finding that acids increase the conductivity of the *Voltaic* pile more than

[1] M. Ø. Vol. I. P. 13. [2] Probably a slip of the pen for »evolve air from the water.«
[3] Slip of the pen for Zn? [4] M. Ø. Vol. I. P. 14. [5] Ed. Vol. I. P. 110. [6] Ed. Vol. I. P. 111.

salts. In 1828 he calls attention in his autobiography to two important results of his galvanical experiments in 1801: »the great activity of the acids in the generation of galvanical electricity,« and the fact that »when the conductors of the *Voltaic* pile produce the same change in a coloured liquid as acid and alkali, the balance of colour returns as soon as all the parts are perfectly mixed, so that the opposite effects produced in the conductors are so proportioned as to neutralise each other exactly.«[1] It thus seems that in the course of years he had become more awake to the importance of the latter result than he was in earlier days.

When *Ørsted* did not enter more deeply into these matters, the cause is obvious. He was so overloaded with work that he could only take Sunday afternoons to »galvanise«, and in one of his letters he even complains that his experiments have come to a stop because they take too much time. In the year 1800 he had been appointed assistant lecturer in the medical faculty of the university without a salary, but with the obligation of lecturing to pharmaceutical students. As he was besides manager of the Lion Pharmacy, his duties were already onerous, but it appears from his letters to *Manthey* that in lectures and other work he did considerably more than his duty. It is of course only natural that a young man starting on his career has to take much business upon himself, but already at this stage of his life it becomes evident that *Ørsted* possessed certain qualities, valuable in themselves, but rendering him liable to accumulate a number of different tasks on his hands. His comprehensive interests, both of general and of a professional kind, to a certain degree robbed him of that strength of limitation which will take one so far in science, and more particularly in experimental science.

In the summer of 1801 *Ørsted* set out upon a journey abroad which lasted till the close of 1803. This journey was made possible to him by a grant from »*Cappel's* Travelling Legacy«, and he looked forward to it with great expectation. When in the year 1800 there was some talk of a professorship or a readership for him in the university, he wrote to *Manthey*: »According to what *Moldenhawer* says I am sure of advancement, but I hope to be able to go abroad nevertheless, I think I would rather resign my post than give up my journey.«[2] »His journey was made in a time of singular

[1] Autobiogr. P. 521. [2] M. Ø. Vol. I. P. 7.

ferment in the scientific world. The new philosophy had given the minds a new look-out and had aroused many great anticipations of a higher insight. Filled with enthusiasm for a farseeing future state in science he easily gained admittance everywhere at a time when there was so great a revolution within the sciences.«[1] Through letters to his home we may follow his route. First he travelled through western Germany to Oberweimar and thence to Berlin. He seized every opportunity of seeing and hearing about scientific matters; an unpublished diary, containing a lot of details of a chemical kind observed by him in the various places he was admitted to, shows that on the whole journey he conscientiously fulfilled the demands of the legacy »that the travelling studiosi shall pursue their chemical studies on their journey.« He visited factories, mines, and museums, attended lectures, worked in laboratories, and »galvanised.« At Göttingen he made a stay of about 10 days writing from there: »The first question asked everywhere is about galvanism. As everybody is curious to see the battery of glass tubes I have invented, I have had quite a small one made here of four glass tubes (in Copenhagen I used 30), and intend to carry it with me.«[2] By means of this battery he made many interesting acquaintances, and many people came to him to see it and copy it, but in spite of these advantages he thought he wasted too much time over it as he could not refrain from working with it when he was alone. At Göttingen he got a letter of introduction to *Ritter* and saw him on the 18th of September. »This man has made great discoveries of which only few are well known. Some of his discoveries have been published by others as their own, and therefore he is very reserved. I only succeeded in getting on friendly terms with him after some conversation.« On the 19th *Ritter* showed him »his most remarkable experiments.« »On the 20th he explained to me all the new ideas he intends to publish in due time. I found so much of genius and beauty in them that I must count this afternoon as one of the most beautiful of all my journey.« — On the 21st: »I have entered into a close friendship with Ritter — — — What I write on galvanism he will embody in his writings which are now of such importance that every chemist and physicist must read them. He is going to send me everything he writes if I send him my works

[1] Autobiogr. P. 521. [2] M. Ø. Vol. I. P. 21.

in return.«[1] Thus the foundation was laid of the great influence exerted by *Ritter's* work and theories on *Ørsted.*

From Weimar he went to Berlin where he remained about six months. Here he had ample occasion to pursue philosophical studies besides work of a physical and chemical kind. Already earlier the *Ørsteds* had been led by their interest in *Kant* to take an interest in *Fichte,* and *Hans Christian* was now glad to have an opportunity of hearing *Fichte* lecture and of making his personal acquaintance. In letters to his brother *Ørsted* gives an account both of lectures and conversations, and tells of discussions about *Fichte's* philosophy with young friends in which he defended *Fichte's* theories in debates by the hour. He also became more closely acquainted with the genuine Nature Philosophy; he heard *A. W. Schlegel's* lectures on mythology and on its influence on the poetical treatment of physics; he formed a friendship with *Friederich Schlegel* and writes in his letters about a discussion of *Schelling's* theories. »He wants to give us a complete philosophical system of physics, but without any knowledge of nature except from text-books and without possessing the same rigorousness of philosophical construction as *Kant*« ..... »These people all bring to market halting comparisons and lopsided physical theories, and then they grumble when others will not accept them. I always pester these people with *Steffens* who has seen nature with his own eyes and thought about her with his own brain.«[2] But he did not by far oppose all adherents of this school of philosophy. At a later stage of the journey he made the acquaintance of *Fr. Baader* »whose writings on Nature Philosophy are so beautiful that one would wish to make the acquaintance of the author, and often so obscure that one needs it to get the explanation.« »A man of such animation and fire is rare.« »He persistently urges that moral and physical nature are most closely connected, and that without such a connection physical science has no real value. In this he accords very closely with *Ritter* and I with both.«[3]

This philosophical influence in youth became of great importance to *Ørsted's* life work. An idea took root in his mind which he again inculcated in his disciples, and which one of them, Professor *Hansteen* at Christiania, expresses in the following manner in a letter to him in 1817: »The spirit that should emanate from the

[1] M. Ø. Vol. I. P. 24 and following. [2] M. Ø. Vol. I. P. 81—82. [3] M. Ø. Vol. I. P. 83.

whole being of the scientist should, it seems to me, be religio-poetico-philosophical; without this he does not know the end to which the sciences strive and can never be anything but a subordinate — — He who only looks with complacency at the stones he himself has fashioned, and has no sense of the beauty of the whole, he is an egotist who only wants to contemplate his own excellence in his own work.«[1]

It is from this influence in youth that a work like »Aanden i Naturen« (The Spirit in Nature) derives its origin; subjects like »Videnskaben som Religionsøvelse« (Science as a Religious Exercise), Det Skiønnes Naturlære (The Natural Philosophy of the Beautiful), and many others point directly to this source. Here we also find the fountain-head of *Ørsted's* faith in physical science as a popular educator — his belief in its connection with ethics — which made him throughout his life devote time and energy in various ways in working for the diffusion of the knowledge of nature and her laws.

In science this influence caused in him a certain predilection for forming comprehensive and vaguely formulated hypotheses, especially such as tended towards the thought of a »Unity in Nature.« Yet he saw the danger of this tendency in the field of physical science. His experimental training caused him to show some criticism on this point, but it was not sufficiently thorough to make him look critically upon such trained experimenters as *Ritter* and *Winterl* who built up their fantastic philosophical speculations on a foundation of inexact experiments or merely qualitative observations.

During his stay with *Ritter* at Oberweimar *Ørsted* refers to a book by *Winterl* which in many ways fitted in with *Ritter's* ideas, »a book full of great thoughts«, but so obscure both in language and exposition that it was not much known. In December 1801 *Ørsted* wrote to *Manthey* about it: »I read no book more diligently or with greater pleasure in my evening hours than *Winterl's* Prolusiones ad Chemiam Seculi Decimi Noni. At each fresh reading I find more harmony and genius in it. I only long to repeat some of his chief experiments, but the dreadful prejudice against it everywhere makes me somewhat cautious even in talking about it. I persuaded *Ritter* to read it, and had the pleasure of hearing his judgment coincide with mine.«[2] It evidently had *Ritter's* warm sympathy. In

[1] M. Ø. Vol. I. P. 321. [2] M. Ø. Vol. I. P. 30.

a letter from December 1801, which *Ørsted* must have received in Berlin, *Ritter* writes: »*Winterl* auf den Thron zu bringen ist ein zu wichtiges Geschäft, als dasz nicht jeder Beytrag dazu den innigsten Dank verdienen sollte.«[1] *Ørsted* made up his mind to offer such a contribution. »I am at present working at an exposition of *Winterl's* Prolusiones ad Chemiam Seculi Decimi Noni, a book rich in genius,«[2] he writes to his brother in February 1802. He had now drawn such general attention to it in conversations with chemists and physicists that he was invited to prepare an account of it for the Philomatic Society. »I am now also diligently experimenting on *Winterl's* Chemistry. — I have also formed a society for testing *Winterl's* system by experiment. It is true that I had myself already begun this, but as he has described several hundred experiments, it exceeds the power of one person to imitate them in a short time,«[3] he wrote at the same time to *Manthey* who read the book, but could not share *Ørsted's* enthusiasm for it.

By his philosophical interests *Ørsted* was predestined to accept *Winterl's* ideas, and the philosophical atmosphere in which he was living contributed naturally to render him susceptible to them. What particularly caught his interest was *Winterl's* assertion that the basis of heat and light, acids and bases, electricity and magnetism, was the same, namely the two electricities. In this way a unity and connection was given to all experience which appealed to *Ørsted's* taste for systematics, and which he believed in and founded the greater part of his writings on.

Winterl's fantastic chemistry contained many assertions which rendered it probable that *Schelling's* philosophy would extend a cordial welcome to it. Two mysterious substances, Andronia and Thelycke, played a great part in it. The former was of an acid nature and a component of all acids — this term to be taken in a much broader sense than usual; thus e. g. carbon and sulphur were acids. Thelycke was a substance found in everything that *Winterl* called bases. »Eine Andronia eine Thelycke, ein Princip der Acidität und Alkalität, welche einander neutralisieren oder zur Indifferenz bringen, gewähren die gesuchten Duplicitäten, die Konflicte und Indifferenzen wodurch das ganze Spiel des Schellingianismus besteht« writes the German chemist *Hermbstädt* in a review[4] of *Ør-*

[1] *Ritter's* still unedited letters to *Ø*. An edition of *Ørsted's* correspondence with scientists by *M. C. Harding* will soon appear. [2] M. Ø. Vol. I. P. 46. [3] M. Ø. Vol. I. P. 51. [4] Neue allgemeine deutsche Bibliothek. Vol. 88. 1804. P. 468.

sted's account of *Winterl's* book. For *Ørsted* not only undertook to give an account of *Winterl's* book in the Philomatic Society and — quite briefly — in *Schlegel's* Europa, but also to bring out an adaptation of it, which he had finished and procured a publisher for at the close of 1802. A comprehensive survey of the results arrived at in the book, in the shape of a letter to a friend, he sent to *Ritter* in order that he might get it published in *Gilbert's* »Annalen der Physik«, but *Ritter* thought it better that the letter should be published as the last chapter of the book, as he himself purposed to write something about it that might take the place of a review in a paper he was writing for *Gilbert.*

Hence the book was published under the title of »Materialien zu einer Chemie des neunzehnten Jahrhunderts«,[1] and thus it came about »that the young Danish traveller in a certain sense might be said to be the first to introduce the Hungarian chemist to the Germans.«[2]

Nearly all reviews of it unmercifully ran down *Winterl's* chemistry. It was acknowledged that it made a far better show in *Ørsted's* adaptation than in the original, but even the most friendly critics regretted that *Ørsted* had not employed his acumen in a more profitable task. In an extensive technical notice in *Trommsdorff's* Allg. chem. Bibliot. the reviewer has, however, been well aware of what — in spite of all — had roused *Ørsted's* interest. On this point he found himself enlightened by the final chapter, the above-mentioned letter. »Aus diesem Briefe scheint zu erhellen, dass vorzüglich die Einheit, welche in den Erfahrungen gebracht wird, wenn die Grundlagen der Wärme, des Lichts, der Säuren, der Basen, der Electricität und der Magnetismus einerlei sind, den Verfasser so sehr für die Theorie *Winterl's* eingenommen hat.«[3] *Hermbstädt* too, under whose guidance *Ørsted* had worked in Berlin and with whom he had been on friendly terms, treated the book very haughtily.

The book meeting with such a cool welcome among most of the experimenting chemists and physicists in the birth-place of Nature Philosophy, an even less cordial reception was only to be expected outside Germany. And we find, in fact, that a noted English chemist, *Chenevix* wrote crushing reviews of it in the three European main languages. To show the strain we quote the latter

[1] Ed. Vol. I. P. 133. [2] Autobiogr., P. 521.
[3] *Trommsd.* Allg. chem. Bibl. Erfurt 1804. P. 126.

part of the notice in Ann. de Chimie et de Physique. »Pour la gloire du dixhuitième siècle il est à espérer qu'il se hâtera de rejetter l'offrande de M. *Ørsted* et la chemie de M. *Winterl.*«[1] *Ørsted*, by the way, incidentally replied to this review a few years later, saying that in it opinions and statements had been attributed to him which he had not expressed.

While still at work on the »Materialien«, *Ørsted* left Berlin and joined *Ritter* at Jena. He was with him »day in and day out« for three weeks, and when he was not with *Ritter*, he was working at the book, which thus came into existence under the very eyes of *Ritter*. While *Ørsted* was at Berlin he had been kept informed about *Ritter's* work by letters. *Ritter* was at that time working under better conditions than at any other period of his life. He was supported by the Duke of Gotha and had the opportunity of making experiments with a *Voltaic* pile of 600 couples, and he fully availed himself of this opportunity. From one of his letters to *Ørsted*, dated February 1802, we learn of fresh and important results of his experiments, and more particularly of dry piles which he was the first to build and investigate, and of the joining of piles in series and in parallel. *Ørsted's* interest in and admiration of *Ritter's* work was probably increased by these communications, and through personal intercourse and co-operation these feelings were no doubt strengthened. Yet his confidence in *Ritter's* results seems at times to have been shaken — at any rate he wrote many years after: »Although the experiments in which *Ørsted* took part were not especially suited to give him full confidence in the results derived from them, he relied all the more on the whole series of repeated experiments formerly made by *Ritter*, and it was not until several years later that he convinced himself of their inaccuracy by the repeated experiments of himself and others.«[2] At this period, however, he was perhaps hardly conscious of his doubts. At any rate in the early part of the year 1803 he wrote a historical survey of the most recent developments in physics in *Schlegel's* »Europa«, under the mark O., which bears the stamp of his admiration for *Ritter* and *Winterl.*[3] »Der rege Eifer, die muthvolle Verachtung wissenschaftlicher Vorurtheile und der tiefe Sinn für das Höhere . . . . zeigt

[1] Ann. de chimie et de phys. Vol. 50, Paris XII. Reprinted with additions in *Gilb.* Ann. d. Phys. Vol. 20, 1805, P. 417.

[2] Autobiogr. P. 522. [3] Ed. Vol. I. P. 112.

uns den Anfang einer neuen Schöpfung«,[1] he writes, with special reference to the two scientists whose works and theories are the main subject of the treatise.

When *Ørsted* left Jena and proceeded to Paris by roundabout routes, his connection with *Ritter* was kept up by letters. We have *Ritter's* letters to *Ørsted* from these and the succeeding years, but not *Ørsted's* to *Ritter*. The letters plainly reveal to us *Ritter's* person, his methods of work, and his development. Already the letters from 1802—03 show us his excitable, imaginative mind besides bearing witness to his conspicuous powers as an experimentalist. Shortly after *Ørsted's* departure ($^{28}/_{10}$ 1802) he writes that he feels he ought to live with some clever man so as not to get into difficulties too often. He adds: »Es wird mir alle Tage ernstlicher um die Wissenschaft, u. ich fühle zu sehr, dass ich ausser ihr ein verlorener Mann bin.« In the letters fantastic speculations and ideas alternate with accounts of excellent observations and experiments and again with the description of quite imaginary experimental results. This year *Ritter* made his most significant discovery, finding electric polarisation and constructing the first accumulator — called storage column — of copper plates and water. It is characteristic that this is mentioned in a letter containing at the same time the information that a needle, one half of which was zinc and the other silver, when suspended like a compas needle tended to set along the magnetic meridian. Besides this the letter is full of a kind of astrological forecasts, and contains an account of a balance by which he will be able to see the position of the sun and the moon, and other statements of a similar wild and whimsical kind. It is easy to understand that a young man prepossessed in *Ritter's* favour could only with difficulty separate the tares from the wheat in works so profuse in ideas, but this was just the task which presented itself to *Ørsted* during his stay in Paris.

When *Ørsted* had gained some proficiency in the French language, he made several acquaintances, and was introduced in the Philomatic Society, where he gave an account, at one or two of the society's meetings, of *Ritter's* researches and results. After he had described *Ritter's* experiments with the Voltaic pile, *Biot* asked him

---

[1] Ed. Vol. I. P. 112.

to write to *Ritter* »that the sooner he announced his discoveries of the last few years the better, as he could scarcely fail to obtain the prize of the Institute (3000 Livres).«[1] »While he was first consul, *Napoleon*, through the French Institute, offered an annual prize of 3000 francs for the most important electrical or galvanical discovery which could be considered equal in significance to *Franklin's* or *Volta's*.«[2] *Ritter's* discovery of the storage column might well seem to merit the annual prize. He composed a paper on it in his usual abstruse style asking *Ørsted* to translate it. Verbatim this was impossible. *Ørsted* quite remodelled it into a French essay which *Ritter* afterwards declared he understood better than his own. Unfortunately *Ritter* had stated that through the discovery of the storage column he had been led to another discovery of far greater importance as he had succeeded in showing that the earth had two electric as well as two magnetic poles. He stated that the uncharged storage column, when placed in a vertical position, became + electric at the bottom, ÷ electric at the top, and that this charge was most powerful when the column formed an angle of 50^0—70^0 with the horizon. This would moreover explain why the before-mentioned needle of zinc and silver took up its particular position of equilibrium. On account of this discovery *Ritter* competed for a larger prize of 60000 francs. *Ørsted* tried to show all *Ritter's* experiments, but those relating to the discovery last mentioned were of course found incorrect. *Ritter* failed to secure the prize, but *Ørsted* gained much benefit and recognition from his endeavours to procure it for him.

Ritter's working methods are best characterised by a remark of him in a letter to *Ørsted* ($^1/_5$ 1804): »Du bist Lehrer u. Forscher zugleich, ich Forscher allein; du gehst gewissermassen ein in das Forschen; ich sehne mich nach den Resultaten.« During the work at Gotha, experimental conditions were favourable and *Ritter* was gratified by a series of beautiful results; later when working conditions were less favourable, imagination, came to the rescue and *Ritter* still obtained results. During his last years — he died in 1810 — imagination ruled absolute and the results were elating, serving to indemnify their author for the misfortunes of his life.

Part of the ideas and experiments, described by *Ritter* in his letters, show a connection with *Ørsted's* simultaneous or later re-

[1] M. Ø. Vol. I. P. 137. [2] Autobiogr. P. 524.

searches. As *Ørsted's* letters to *Ritter* are wanting, we cannot see who was primus motor in their relations or whether there was any causal connection at all. All that we can see — which is as much as we ought to expect — is that it only needs a remark from *Ørsted* to set *Ritter's* imagination aflame with »ideas«. In a letter from May 1803 *Ritter* says: »Du liebst Ideen; also aus Langeweile welche« — whereupon he sets forth a number of ideas, relating to a question about the *Lichtenberg* figures, put by *Ørsted* in a previous letter; these ideas have a certain connection with *Ørsted's* paper on »Elektriske Figurer og organiske Former«[1] (Electrical Figures and Organic Forms) published in 1805, the most fantastic thing, probably, which *Ørsted* ever wrote.

In the same letter of May 1803 *Ritter* writes about oscillations: »Seitdem ich so in die Geschichte vertieft bin, u. überall Perioden . . . . . erblicke, hat Oscillation für mich eine hohe Bedeutung, und die häufigen Versuche alles auf Schwingung zu reduciren, werden lehrreich. Aller Sinnesempfindung liegt Oscillation zum Grunde . . . In aller Zustandsänderung ist Oscillation begründet. Jeder chemische Process kommt vom Maximum der Oscillation durch allmäliges Verschwimmen derselben zum Product . . . Kurz überall, wo nur etwas geschieht, geschieht es auch nothwendig oscillatorisch.« . . .

As during the following years we find *Ørsted* occupied with the oscillations causing acoustical figures, the above passage by *Ritter* may possibly have been inspired by *Ørsted's* account of the first steps in these experiments, though of course the reverse may also have been the case, one of *Ritter's* many ideas may have found congenial soil in *Ørsted's* mind. *Ørsted's* thoughts also turned on the oscillatory propagation of electricity, and he first set forth his ideas on this subject in 1806. While *Ørsted* always keeps fairly within sight of the scientific hypothesis in his writings, *Ritter* soars into the realms of imagination and incidentally pronounces a remarkable prophecy. In his letter of May 1803 he contends that earthly things are dependent on periodical celestial phenomena, and finds that the years of maximum inclination of the ecliptic are also the years in which important electrical discoveries are made. He exemplifies this by the following list: —

[1] Ed. Vol. III.

Jahre des Max. der Schiefe der Ecliptik.	1745 1/3	Erfindung der *Kleist*schen Flasche 1745 (verschiedene).
	1764	Electrophor 1764 *(Wilcke).*
	1782 2/3	Condensator 1783 *(Volta).*
	1801 1/3	Volt-Säule 1800 *(Volta).*

»Du wirst also nicht eher auf eine neue Epoche, oder deren Anfang als im Jahre 1819 2/3 oder 1820 zu rechnen haben. Die erleben wir also wohl noch.« Strange indeed, that *Ørsted* was not only to see the prophecy fulfilled but was himself to fulfil it.

In this period it is often evident — in particular from the letters — how *Ritter* elaborates *Ørsted's* comparatively sober results and remarks. When *Ørsted*, by the aid of *Ritter,* had got his first paper on acoustical figures[1] published in *Voigt's* Magazine, *Ritter* added »eine lange Nachschrift . . . . die hoffentl. sagen wird, was Du eigentl. hast alles sagen wollen . . .«[2] The postscript contains quite a theory about the generation of electricity by oscillations in solid bodies, due to the stretching and bending which these oscillations give rise to.

Another letter[3] affords us an amusing glimpse of the fact that the tendency with which *Ørsted* had associated himself was not accepted without criticism in the German world of science. *Ørsted* had evidently written to *Ritter* to ask his advice about the publication of his paper on acoustical figures when it was finished. *Ritter* answers: »Mit dem Platz für deine vorgenommenen Abhandlung bin ich etwas verlegen . . . . *Gilbert* ist auf die schlechteste Art orthodox. Für meine Heterodoxien weiss ich noch immer nichts bessers, als den *Voigt.* Der liest's doch nicht, ehe er's in die Druckerey giebt, u. wenn er's auch läse, so glaubt er doch an die Dreyeinigkeit.«

Ritter had many conjectures on subjects which had formerly occupied *Ørsted's* thoughts and which play an important part in his later production, such as the classification or arrangement of the substances in groups and series. Finally magnetism was a constant subject of speculation to *Ritter*, more especially the connection between magnetism and other activities of nature, and thus *Ørsted* was early led to ponder over this matter.

We have seen how *Ørsted's* production during his stay abroad and the intimacies he formed gave him officially the stamp of being

[1] Ed. Vol. I, P. 261. [2] R. to Ø. 20/11 1804. [3] R. to Ø. 8/2 1805.

closely associated with the philosophical school of physical science. This was detrimental to him in his own country, hampering his career to some extent, and it delayed his recognition by experimentalists. As late as 1819 *Berzelius* said to *Zeise* that 15 years ago *Ørsted's* ideas were disfigured by extravagance but now he had emancipated himself. It is apparent from *Ørsted's* letters how he felt that the epithet »Naturphilosoph« did him harm. His friend *Manthey* tried to create the impression, though for some time without success, that *Ørsted's* interests were not only philosophical, but practical and experimental.

Ørsted returned to Denmark in January 1804 and expected and hoped to obtain the professorship in physics, the duties of which, since the last occupant's death, had been temporarily discharged by the professor of astronomy. But the warden of the university believed him to be more of a philosopher than a physicist and on that account would not nominate him. In the course of the year reports of the unfavourable critiques on the »Materialien« found their way home. There is, e. g., a letter from *Engelstoft*,[1] who was staying at Paris, to Professor *Nyerup*, in which he says: »A piece of literary news may interest you, but I do not exactly care for it to be said that I have sent it home. Dr. *Ørsted*, the chemist, had translated a German chemical book and written an awfully laudatory preface to it. This chemical book and its preface have been unmercifully criticised in an English journal by one of the leading English chemists who at the same time gives a plain exposition of the whole philosophy of *Schlegel* and compeers, that is, in so far as it can be explained in intelligible words, for the said book is such nonsense from one end to the other, entirely couched in the very latest, most mystical, terminology. The said English review has now been published in the Annales de Chimie with notes by the famous French chemist *Guyton-Morveau*, who has thus finally made the whole thing ridiculous to all the world. Whatever will *Ørsted* say! The worst of it is that there is no need either of profound chemical insight or great genius, but only mere common sense, to see with half an eye what nonsense it is. Amongst others the following two sentences, they say, are to be found in this same book so highly lauded by *Ørsted:* l'architecture est une musique congelée, and: les dieux de la mythologi ne sont que de cristallisations intellec-

[1] Udvalg af *Engelstoft's* Skrifter, Vol. 3, Kjøbhvn. 1862. P. 318.

tuelles (for the book also contains much theology, mythology, etc., according to what I learn).«

The tone, it must be admitted, is somewhat spiteful, and the same note was sounded elsewhere. Thus when *Ørsted* had reviewed »Elektricitetslære grundet paa Erfaringer og Forsøg«[1] (Textbook of Electricity founded on Experience and Experiments) by *Friederich Saxtorph*, the author replied by an anti-criticism[2] in an unpleasant strain, taxing *Ørsted* amongst other things with his propensity for Nature Philosophy and his predilection for *Ritter.*

Ørsted, however, did not own himself defeated. We see this from an invitation to a course of lectures issued by him in 1804.[3] In this he commended the connection of philosophy with physics, calling attention to the peculiar interest physics thereby acquires for the scientist and giving this as a ground for his invitation. Both this and later calls resulted in crowded halls. »Deine Vorlesungen möchte ich wohl mitgehört haben, obgleich wie mir *Willemoes* erzählte, ich kaum ins Auditorium gekonnt hätte,« wrote *Ritter* on the 4th of August 1804. *Ørsted's* pleasure in communicating his knowledge, his enthusiasm for his subject, in addition to the experience of lecturing and experimenting gained in such ample measure in his travels, thus helped to procure for him and his scientific aims an amount of sympathy and interest sufficient to counteract the opposition he encountered.

In the first year after his return three articles[4] contributed to the publications of the Scandinavian Literary Society showed *Ritter's* and *Winterl's* influence still dominant in him, and another paper, a criticism of the so-called eudiometry,[5] made it evident that he still regarded *Lavoisier's* school with disfavour. But in the same year the letter already alluded to shows him more soberly occupied with the acoustical figures, while a treatise in 1806, »Versuche veranlast durch einige Stelle in *Winterl's* Schriften,«[6] informs us that his belief in »Andronia« and »Thelycke« has suffered a serious shock through the negative result of his attempts to find Andronia. At any rate the theoretico-chemical paper of the same year entitled »Die Reihe der Säuren und Basen«[7] takes no notice of these sub-

---

[1] Ed. Vol. III.
[2] Tillæg til Kbhvns. lærde Efterr. 1805. P. 1—14. Answer. P. 14—16. (Ed. Vol. III.)
[3] Ed. Vol. III. [4] Ed. Vol. III. [5] Ed. Vol. I. P. 248.
[6] Ed. Vol. I. P. 277. [7] Ed. Vol. I. P. 289.

stances. In this paper, which is based on *Winterl's* idea that the two electricities are principles of acidity and basisity, *Ørsted* develops the view already previously advanced by him, that the two sorts of chemical compounds belong to the same group. Both this and a shorter paper[1] on the undulatory propagation of electricity, to which he himself attached much weight, are the precursors of a larger theoretico-chemical essay in 1812 to which we shall return later.

The large attendance at his lectures and his fertility as an author at last procured for him an extraordinary professorship in 1806.

In 1807 his production was continued with »Betragtninger over Kemiens Historie«[2] (Reflections on the History of Chemistry) which appeared both in Danish and German after a series of lectures delivered before a numerous audience in the beginning of the winter of 1805—06. About this paper he wrote in his autobiography that in spite of its many imperfections he regarded it with a certain predilection because of its intimate association with his scientific life. He describes the many alternating chemical theories he had lived to see and asks himself, »Is there a permanent truth in the midst of all this change?«[3] . . . . . As an answer to this question he tried to show »that every theory which had been adopted by philosophers involved a contemplation of the connection of things, and a certain knowledge of the laws of nature, that is, of the reason in nature. This element of truth, he thought, we rarely succeed in giving an expression so pure and free from alloy that it can pass from age to age without needing correction of form, but neither the individuals nor the ages can be held accountable for this, it must be explained by the higher laws governing the development of mental life on earth.«[4] How deeply rooted these opinions were in *Ørsted's* mind may be seen from the fact that this paper was reprinted with hardly any alterations in 1844.

In the years following 1807 *Ørsted's* confidence in *Winterl's* researches was evidently shaken, and a decrease of *Ritter's* influence may be traced, partly from the gradual decline of *Ørsted's* contribution to their correspondence — of which *Ritter* complains in the letters — and partly by the letters showing *Ritter's* mental collapse.

Although the *Winterl-Ritter* period of *Ørsted's* life harmed his prospects and led him into alien paths from which he was eventually

[1] Ed. Vol. I. P. 267. [2] Ed. Vol. I. P. 315. [3] Autobiogr. P. 527. [4] l. c. P. 527.

obliged to return, he never looked back upon it with regret or with bitterness against his prototypes. He remained *Ritter's* friend until the death of the latter in 1810, though not blind to the fact that he was a broken man during his last years. About *Winterl* and his system *Ørsted* wrote in 1828 »*Winterl* was a man of great ideas but without any acute perception of detail. His experiments, if we may so call them, are without worth, but his far-seeing mind had perceived the same connection in the mass of chemical knowledge at hand, as the galvanical discoveries showed other chemists. His system is really the same as the one now called the electro-chemical system, only that in part it is more comprehensive and in part has many excrescences. Some chemists, indeed, do not admit that the electro-chemical system existed before certain conclusive experiments by which it was, as it were, forced upon the attention of all experimentalists, but this laboratory opinion does not hold outside the workshop. The publication of a new thought is just as much an event as the publication of a new experiment, nay, the latter only obtains its importance, which may be very great or very small, through its relation to the world of thought.«[1] These remarks are characteristic, partly by showing the mature man's criticism of a youthful ideal, partly by their evidence as to the influences of youth.

The first important experimental work from the hand of *Ørsted* was among the publications of the Royal Society of Sciences for the year 1807 — published 1810 — and bore the title »Forsøg over Klangfigurer«[2] (Experiments on Acoustical Figures). It obtained the Society's silver medal on the 18th of February 1808 and was published in German in *Gehlen's* »Journal für die Chemie and Physik« in 1809. Its precursor was the before-mentioned letter from *Ørsted* to *Ritter* »*Chladnis* Klangfiguren in elektrischer Hinsicht betreffend« dated October 5th 1804 and published in *Voigt's* Magazine (1805).

The paper was based on several hundred experiments on acoustical figures produced on square plates of glass or metal, a few of them on circular plates. While *Chladni* used sand to make the acoustical figures visible, *Ørsted* employed lycopodium in most of his experiments, and with this finer powder the figures showed somewhat differently. *Ørsted's* reason for taking up this work ap-

[1] Autobiogr. P. 523. [2] Ed. Vol. II. P. 11.

pears from the letter of 1804. He expected to discover electrical effects due to the oscillations causing the acoustical figures, and he supposed that the accumulation of electricity in the different parts of the vibrating plates would be observable by the lycopodium becoming electric on being sifted out and thus presumably seeking just those parts of the plate which had the opposite electricity. *Ørsted* was of opinion that electricity in great quantity and of slight tension is not conducted; he inferred this from the electric charges arising from contact between different metals, and he therefore considered it possible that even on an oscillating metal plate there may be different electricities in different places. Hence, when he expected a generation of electricity from the vibrations, he was no doubt influenced by *Ritter's* and his own ideas about the universal importance of electricity in physical phenomena, and by *Ritter's* experiments on the relation of electricity to the sense organs.

As soon as *Ørsted* made the acoustical experiments with lycopodium, he became aware of certain phenomena which he supposed might afford an important insight into the mechanism of the production of a tone, and hence the main treatise gives an account of the many experiments carried out in order to clear up this matter. It deals with four different problems. First *Ørsted* examines the shape of the boundary lines between the vibrating and the quiescent parts of the plate. Through neat experiments and a series of careful measurements they are found to be hyperbola with the nodal lines for asymptotes, and it is then attempted to prove theoretically that the boundary lines in general must be conic sections and that, with the shape of plate here used, they may be expected to be just hyperbola. The second chief subject investigated is a series of minor motions in the plate which are revealed by the lycopodium, and which exist simultaneously with the large motions marked by the nodal lines. In particular it is remarkable that small heaps of lycopodium accumulate where the vibration is greatest. *Chladni* had observed that fine dust was apt to accumulate in these places[1] but did not go further into the matter. *Ørsted* studied and described these minor motions of the dust for different ways of evoking vibrations in the plate, viz: by knocking a point of the edge, by striking the whole side line, by applying a violin bow etc. On the results of these experiments he built up a

[1] Die Akustik von *E. Chladni.* Leipzig 1802 P. 120.

theory about the propagation of vibrations in solids professing to explain the propagation of the motion from the place where the bow is applied through the quiescent to the vibrating parts. The explanation is not very clear and is of no value, but in the letter of 1804 *Ørsted* sets forth an idea of importance in relation to the appearance of these minor oscillations, a thought which is repeated in the treatise though not so clearly expressed. He says that these minor motions show that every sonorous oscillation is composed of a number of minor oscillations. »Hence the nature of each tone seems to be more dependent on the relation between the subordinate oscillations and the main oscillation than on the mere number of the main oscillations. Each tone thus seems to be an organisation of oscillations.«[1]

These first two sections are the valuable part of the treatise and their importance is seen from the treatment to which especially *Savart* and *Faraday* submitted the same subjects about 20 years later. *Strehlke*[2] found the hyperbolic form of the dust lines in 1825, and a couple of years later *Savart*[3] investigated the behaviour of vibrating plates in the same way as *Ørsted* had done. He, too, saw that the lycopodium may set outside the usual nodal lines. He gave a detailed description of the facts in regard to circular plates, coming to the same result as *Ørsted* and explaining it in a similar way, it being his opinion that through this result we may demonstrate oscillations which offer information about the overtones that determine the timbre of the plate.

While *Savart* does not mention *Ørsted's* paper it was, on the other hand, referred to by *Faraday* who took up the question for definite determination in 1831.[4] He only mentions *Ørsted's* paper incidentally, however, his enquiry having been caused by *Savart's*.

Faraday cites *Savart's* above-mentioned result and says, »A secondary mode of division subordinate to the principal as to be always superposed by it, might have great influence in reasonings upon other points in the philosophy of vibrating plates; to prove its existence therefore is an important matter. But its existence being assumed and supported by such high authority as the name of *Savart*, to prove its non-existence supposing it without foundation is of

[1] Ed. Vol. I. P. 261. [2] *Poggend.* Ann. Vol. 4. 1825. P. 205.
[3] Ann. de Chimie et de Phys. Vol. 36. P. 187 & 257, Paris 1827.
[4] Philosophical Transactions. Vol. 121. P. 249. London 1831.

equal consequence.«[1] This was just *Faraday's* purpose. First he exactly describes a series of experiments with vibrating plates where lycopodium or some other light powder is used. Its liability to form accumulations of »heaps« in the vibrating places is investigated together with the motions in the small heaps. His observations here accord with *Ørsted's*. Next *Faraday* shows that the shape of the accumulations is altered by small paper screens on the plates, though these cannot change the vibrations. From a number of systematically arranged experiments he concludes that the accumulation of the light dust in certain places does not mean any secondary division of the plate according to overtones, but is only due to currents of air passing over the plate in motion. That this interpretation is the right one he demonstrates by producing vibrations of the plates in a vacuum, the acoustical figures then prove the same whether heavier or lighter powders are used.

The third chief point of *Ørsted's* enquiry deals with the possibility of producing electricity during the vibrations. He observed that directly, by the electrometer, such production could not be demonstrated, and he realised that the method of showing the distribution of the electricity by the electrified powder, which he had first trusted would take him to the goal, had also failed, since he found that different powders, though assuming opposite electricities when sprinkled out, set quite similarly. But he observed a phenomenon which he supposed to be caused by electric action. He states that the dust adhered most to the plate in the »dust lines«, i. e. the boundary lines between the quiescent and the vibrating parts. If an acoustical figure has been produced on a horizontal plate and this plate is turned face downwards and tapped lightly with the palm of the hand, the dust will fall off from all places except the dust lines. He thought this was due to an electric binding of the dust in these lines. To explain this generation of electricity he resorted to the same line of argument which he had applied in explaining the propagation of the vibrations, and he himself attached considerable weight to it. The main idea is best understood if we imagine a square disc stroked down the middle of one side and held at the corners, so that the nodal lines become diagonals. By these the disc is divided into 4 triangular surfaces. Each of these is imagined to be composed of parallel strings of decreasing

[1] Phil. Transact. Vol. 121. P. 249.

length from edge to middle. The motion is propagated, but with decreasing oscillation, from the farthest string to that nearest the middle, *Ørsted* now took it for granted that, if the velocity of the strings decreased on passing from the longer to the shorter strings, a certain internal velocity would increase. Through this intense internal motion he supposed electricity to be generated. »Might it not be possible that the external oscillatory motion, changed into a penetrating internal motion, passed also from a mere mechanical motion into a generation of force?« In such an utterance one may perhaps perceive a vague anticipation of the principle of the Conservation of Energy. On the basis of the conception here sketched he explained the adherence of the dust to the dust lines in a very elaborate way.

The fourth and last subject is philosophical. *Ritter's* views on the generation of electricity through sound vibrations are mentioned, as well as his theory that light which acts on the eye is due to vibrations just like sound which acts on the ear, only that the light vibrations are much quicker — slowest in the rays giving the impression of blue, quickest in the red rays. Finally there is an oratorical conclusion about the »profound incomprehensible reason of nature which speaks to us through the flow of music.«

One branch of *Ørsted's* philosophical production is closely connected with this conclusion. The first part of his »Bidrag til det Skjønnes Naturlære« (A Contribution to the Natural Philosophy of the Beautiful) is founded directly on it. It appeared in the Publications of the Scandinavian Society, vol. VII, 1808, under the title »Om Grunden til den Fornøjelse Tonerne frembringe« (On the Cause of the Pleasure Produced by Music). In this paper he praises the beauty of the acoustical figures and seeks the cause of this impression on the senses in the fact that they are an expression of the »reason in nature«. *Vilh. Andersen* has pointed out[1] how *Ørsted's* predilection for experiments on acoustical figures is reflected in his circle during this period. He must have shown these experiments to many people, probably praising their beauty. *Søren Kierkegaard* writes in a letter that *Ørsted's* face had always seemed to him like a sonorous figure to which nature had applied her bow in just the right way; »*Hauch* in his biography compared his life

[1] *Vilh. Andersen:* Tider og Typer af Dansk Aands Historie. Goethe II. P. 111. København 1916.

to a mighty sonorous figure, *Eckersberg* painted him with the glass plate in his hand.«[1]

In some occasional verses (written to a young student) *Ørsted* compared the study of the physical sciences to an acoustical figure.[2] *Oehlenschläger*, too, saw his experiments; in »Aladdin« he lets Noureddin as »Nature's Researcher« perform them.

Hence we see that »Forsøg over Klangfigurer« was of great significance to *Ørsted*. As already mentioned the treatise brought him the silver medal from the Society of Sciences and at the same time admission as a member of the Society in November 1808. In March 1809 he was elected corresponding member of the Academy of Sciences at Munich in consequence of his paper on sonorous figures in *Gehlen's* Journal.

The performance of so many experiments for definite purposes trained *Ørsted* in the art of experimenting and observation, and thus we see that his descriptions of experiments agree well with those of such skilled experimenters as *Savart* and *Faraday*. In home circles this work enhanced his reputation as an experimentalist and a natural philosopher of exceptional power, but its philosophical cast was detrimental to his treatise regarded as a work on physics and partly concealed its importance to those who took up the work later on. For it cannot be denied that the arguments employed in various places, especially to explain the generation of electricity, bear the stamp of being adapted so as to agree with a previously given result and of building not so much on mathematically or experimentally grounded facts as on hypotheses intuitively advanced. To French and English scientists, in particular, who were not infected with the phraseology of the German school of Nature Philosophy, the form must have been distasteful.

In *Gehlen's* Journal for 1808 there is a short letter from *Ørsted* to *Ritter* dated September 3rd 1808.[3] This letter opens with a remark about some damage caused to *Ørsted* through the fire of Copenhagen during the bombardment in 1807; a larger text-book on physics, the composition of which had been followed by *Ritter* with much helpful interest, was just going through the press when the printed part was burnt during the siege; the printing was however begun again in 1808. The letter further deals with some

[1] *Vilh. Andersen:* Tider og Typer. *H. C. Ørsted.*

[2] *H. C. Ørsted's* Saml. og efterladte Skrifter. Kbhvn. 1851. Vol. 4. P. 20.

[3] Ed. Vol. I. P. 344.

electrostatical experiments on which he had been engaged for a year and through which he had some time ago found the law, just made public by the name of »*Simon's* law«, and which states that »the effect of electricity decreases as the inverse distance, not as the inverse square of the distance.« He was now occupied with some consequences of this law and was trying to demonstrate some imperfections in *Coulomb's* electrometer. *Ritter* has added some remarks to the letter showing its real meaning and the purport of *Simon's* law. The question is about the deflections of an electrometer which has been connected to the plate of an electrophorus when the sole is charged, and the cake has various thicknesses. It is this deflection which is said to vary inversely as the distance i. e. as the thickness of the cake. *Ritter* claimed that on a visit to *Volta* this result had been shown and communicated to him by the latter, and that his own mention of it on his return had caused *Simon's* researches. For this reason *Ørsted* in his letter quoted *Volta's* name by the side of *Simon's* in connection with this result which he seemed to consider as opposed to *Coulomb's* law, which, however, as we know, is not the case.

In 1809 the printing of the above-mentioned text-book was finished and it obtained the title »Videnskaben om Naturens almindelige Love. Første Bind.« (The Science of the General Laws of Nature. First Volume.). It related to the science of mechanics. The second volume was to have dealt with other parts of physical science but was never finished. »The continuation, namely, the chemical part, was interrupted by a journey, and later stopped by the great advance made by Science every year, which continually pointed to considerable and very essential gaps which he partly sought to fill up by independent investigations,« writes *Ørsted* in 1828.[1] Another and equally essential reason will no doubt have been *Ørsted's* increasing engrossment in teaching and public duties. We have drafts for the continuation of the text-book, partly in booklets printed for his classes, partly in unfinished manuscripts, notes for lectures, fair copies of such by students, all of which are found among *Ørsted's* posthumous papers, but there was never time to recast the numerous drafts into one. We find many indications of *Ørsted's* busy life from this time onwards. In his writings we often find remarks about investigations he was engaged on which would soon be finished and the results of which he in-

[1] Autobiogr. P. 529.

tended to publish as early as possible. But the publication never took place, there were evidently many things to prevent it. Thus, in the letter already referred to, he says that his electrostatic experiments have occupied him for a year and will now soon be finished and published, but this was never done though the work was not relinquished. In 1814—15 he read a paper on researches of this kind before the Society of Sciences; he intended to complete them during the coming winter but did not manage to carry out his purpose. The same experiments are mentioned in a letter from him to *Schweigger's* Journal in the same year. They are concerned with the validity of *Coulomb's* law tried with *Coulomb's* torsion balance, and seemed to him to show that the law does not hold.

In the preface[1] to the text-book he states his intention of publishing annually a brief description of recent discoveries, as supplements to the book, so as to keep it up to date, and further that he is at work on a German translation »with the alterations which are necessary on transplanting a book from one literature to another« — none of these purposes was ever realised. The book opens with a characteristic »Nature Philosophy« introduction which was enlarged and reprinted in 1811 under the title »Indledning til den almindelige Naturlære«[2] (Introduction to General Natural Philosophy). The preface informs us precisely how onerous were *Ørsted's* duties as a teacher. They consisted not only in instructing students of various kinds in the first elements of his science but also involved the task of keeping advanced students informed of the progress of theoretical and experimental science. Through oral delivery then, he redeemed his promise of enabling the readers of his book to keep abreast of the latest departures in science with which he kept in touch in spite of his extensive duties.

From May 1812 till the summer of 1813 *Ørsted* was abroad, mostly in Berlin and Paris, and »derived considerable benefit« from his experience there. On this journey he managed to procure the time to prepare an important work for which he carried the material of several years with him. Among this was a paper on the theory of chemistry which he had read before the Society of Sciences in 1810—11. This paper he lent to Geheime-Statsraad *Niebuhr* after some conversations between them about the theories of chemical science. *Niebuhr* »very warmly advised *Ørsted* to publish

[1] Ed. Vol. III. [2] Ed. Vol. III.

this paper in German, and procured a publisher for it.«[1] *Ørsted* did not restrict himself to the publication of this only, but, out of several papers he had brought with him, in 10 weeks compiled a book of 18 sheets which he brought out under the title »Ansicht der chemischen Naturgesetze«[2] and which passed through the press by the close of 1812. It states *Ørsted's* chemical theory, the first germ of which he traced back to his undergraduate days; he describes how the same subjects attracted his attention in his early years and points out that a trace of his present opinions may be found in a review,[3] printed in 1800, written in 1799, of *Gadolin's* adaptation of *Fourcroy's* Chemistry, where he proposes the same classification of certain chemical compounds as in the present work. Already in the works following upon *Galvani's* and *Volta's* first discoveries *Ritter* had put forward the idea of the identity of chemical and electrical forces, and the same idea was suggested to *Winterl* by the ordinary chemical and electrical phenomena. This idea appealed to *Ørsted.* The Voltaic pile and the subsequent discovery of the chemical effects of the current rendered the correctness of the notion more probable. *Ørsted's* confidence in the scientists who had advanced it grew, and his interest in and work for the diffusion of the knowledge of *Ritter's* and *Winterl's* ideas leading to his production of the »Materialien« was inspired by this confidence.

We find the same thoughts in small papers from 1805, especially in »Nyere Undersøgelser over det Spørgsmaal: Hvad er Chemie?«[4] (Recent Investigations concerning the Question: What is Chemistry?) It contains remarks which represent the mentioned ideas as established facts. »What else are the chemical forces but the opposite electricities?« . . . . »The same forces manifest themselves in magnetism as in electricity. . . . . All forces in nature may be reduced to these two.« Further we have material for the book in papers from 1806: »Ueber die Art wie sich die Electricität fortpflanzt,«[5] »Die Reihe der Säuren und Basen,«[6] likewise in »Betrachtungen über die Geschichte der Chemie«,[7] in the paper submitted to the Society of Sciences in 1810—11 on the theory of chemistry which, however, was not printed, and in »Indledningen til den almindelige Naturlære«[8] 1811. Finally, in »Ansicht der chemischen Naturgesetze«,[9] we may observe traces of the *Kant*ian influence from *Ørsted's* early

[1] Autobiogr. P. 530. [2] Ed. Vol. II. P. 35. [3] Ed. Vol. III. P. 51. [4] Ed. Vol. III. P. 105. [5] Ed. Vol. I. P. 267. [6] Ed. Vol. I. P. 289. [7] Ed. Vol. I. P. 315. [8] Ed. Vol. III. [9] Ed. Vol. II. P. 35.

youth, partly in the systematics, and partly in the assertion of the fundamental principle, — »the dynamical conception« — that the existence of matter is dependent on two fundamental forces, a repelling and an attracting force.

For the rest, during the past years, *Ørsted* had emancipated himself from German philosophy and created his own. When at Berlin in 1812, he derived no pleasure from the meeting with *Schleiermacher*, and about *Fichte* he wrote: »I have not been to see him very often, for owing to the difference of our views, especially on nature, no very comprehensive communication can take place between us.«[1] *Hauch* in his Life of *Ørsted* quotes a remark of his from about 1810 showing a change in his views on the value of general ideas when the foundation of facts is wanting: »It is also my firm conviction . . . that a great fundamental unity permeates all nature, but just when we have become convinced of this, it is doubly necessary that we turn our attention to the world of the manifold where this truth will find its only corroboration. If we do not, unity itself becomes a barren and empty thought leading to no true insight.«[2] It was thus only natural that he should cast off the influence of *Ritter* and *Winterl.* He expressed his appreciation especially of *Ritter's* genius but added: »Malgré l'estime que l'auteur avait conçue du génie de *Ritter* et de *Winterl,* il a cependant senti la nécessité de s'éloigner, à plusieurs égards, de leurs opinions.«[3] He had worked out an independent theory by himself though profiting all the time by the great ideas of these scientists. »On trouvera aussi que, quels que soient ses raisonnemens, il ne les a jamais fondés sur des faits douteux qui ont souvent été trop facilement adoptés par ces deux physiciens, et qu'il a évité en grande partie l'obscurité qu'avait répandue sur leurs écrits une méthode trop compliquée.«[4]

From this statement it will be seen that »Ansicht der chemischen Naturgesetze« fills an interesting place in *Ørsted's* production, as it shows the result of about 18 years' development in his scientific life, a development the root and growth of which we have been able to trace.

When *Ørsted* came to Paris at the close of 1812 he received the offer of the publication of a French translation of his book by *Marcel de Serres.* He himself took part in the translation or adap-

[1] M. Ø. Vol. I. P. 298. [2] Saml. og efterl. Skrifter. Vol. 9. P. 120. Kbhvn. 1852.
[3] Ed. Vol. II. P. 175. [4] l. c. P. 176

tation which was necessary, on account of the great difference in the opinions that might be presupposed among French and German readers.«[1] The title of the book, which was not easily rendered in French, was altered to »Recherches sur l'identité des forces électriques et chimiques.« *Ørsted* considered this French edition in many respects superior to the German one.[2]

In the introduction *Ørsted* compares the state of chemical science to that of mechanics before *Galilei, Descartes, Huygens,* and *Newton*. Before the age of these men a great number of important facts were known, and even consecutively connected series of important facts, but the great principle of unity to which the modern science of mechanics owes its completeness, was wanting. Just so with chemistry at its present stage: a large body of facts had been collected, a series of affinities found, but no first cause of these affinities had been discovered. An attempt should now be made, he thought, to reduce all chemical effects to the primitive forces which produce them, so that chemical science could be based on a theory of force whence, by the aid of mathematics, the chemical phenomena could be deduced. The work now begun was to be a first step in this direction.

We see that it was an imposing task he had set himself, and from the closing remarks of the introduction we learn that only his conviction of its ultimate necessity had induced him to enter upon a proceeding which he could but hope to accomplish imperfectly. We may infer that once he was engaged upon the work his anticipations of its importance and his ambitions for it grew. In a letter[3] dated July 7th 1812 he writes to *Sophie Ørsted* that at the instance of *Niebuhr* he had begun the compilation, from several of his papers, of a small volume to be entitled »Versuche eines Physikers sich in seiner Wissenschaft zu orientieren.« This modest title was given up, we see, as the purpose expanded.

The first step that in his opinion ought to be taken consisted in a general classification of all inorganic substances according to their chemical nature. He divides them into three groups or series.

The first group contains the elements arranged in a list according to definite chemical characters. The possibility is discussed of finding certain physical characteristics according to which the elements might be grouped. This proving impossible, they are ar-

[1] Autobiogr. P. 530. [2] Ann. of Philosophy. Vol. 13. P. 369. London 1819. [3] M. Ø. Vol. I. P. 296.

ranged in successive order according to their liability to combustion: the markedly combustible substances, such as hydrogen, the metals, ammonium, arsenic etc., are arranged according to decreasing combustibility, those which are essentially fire-feeding, according to increasing »power of ignition«. Gold and the platinum metals form the transitional stage between the two groups. Hydrogen is placed at the top of the combustibles, while oxygen closes the row of the fire-feeders. Between oxygen and the platinum metals carbon and sulphur are placed. As a reason for this grouping it is stated that at low temperatures these substances are less oxidable than the metals, and their power of robbing the metallic oxides of their oxygen at higher temperatures is not due to their greater combustibility but to other circumstances; it is partly a consequence of the heat and partly due to the fact that the product formed is gaseous. A more detailed arrangement within the list is no given.

His second group or list contains the acids or bases. The concept of acids can only be defined by their relation to bases and vice versa, and the strength of the acid or base only measured by the quantity required to neutralise a certain quantity of a standard fluid of some kind. The list is made out according to decreasing acidity, increasing basisity. The further arrangement is not indicated here either.

The third group or list contains the salts.

Next, attention is directed to the chemical reactions within the first groups. If two of the members of the first list, a combustible and a fire-feeder, combine, a compound belonging to the second list is formed. The combination of the two substances takes its course until the forces emanating from both neutralise each other and cease to draw them together. On combining, the substances pass out of the first list and form a member of the 2nd list. When highly combustible bodies combine with a small amount of oxygen, bases are formed, and when slightly combustible substances take up much oxygen, they form acids. These two kinds of substances are likewise mutually attracted up to a certain limit of saturation and then on combination pass out of the 2nd list into the 3rd group, the members of which have not the power of so acting on each other that they can form members outside the group.

The chemical compounds which may be formed within the two first groups are supposed to arise through the agency of two forces drawing the substances together and wholly or partly neutralising each other when the union is effected. Such forces are called opposite. It is then shown how the physical properties of the substances vary according to their greater or smaller quantity of the two characteristic opposite forces »combustibility« and »power of ignition.«

Next it is shown that the opposite chemical forces can flow from one place to another in a »chemical circuit«. When two metals of different combustibility are plunged into water they will effect a separation between the combustible and the fire-feeding part of the water, so that oxygen will seek the most combustible metal while hydrogen will be repelled by it and carried towards the less combustible. If these two metals are connected by a conducting wire, the balance of the forces in this wire will be disturbed on account of the alteration in force at the terminals in the water, and the forces will then be propagated through the conductor in the attempt to re-establish the equilibrium. In these passages the forces are treated by turns as material and non-material; there is a vagueness in the use of the concept of force which, by the way, is felt also in *Ørsted's* later production.

Having concluded this investigation of the chemical classifications and the nature of the chemical forces, he turns to electricity in its various modes of action, and examines the nature of the electrical forces. He finds the following similarity between the electrical and chemical forces »that there are two which neutralise each other, that they are present in all bodies, that the greater part of them are in equilibrium and are only made to appear by the effect of external agencies.« Through the chemical action of electricity it is next seen that all substances with combustibility seek the negative electricity, and everything with power of ignition, the positive electricity. From these circumstances the inference is drawn that affinities are conditioned by the opposite electricities, that combustibility and positive electricity, power of ignition and negative electricity are allied.

When electricity is present in great quantity, but with very slight tension, it is not conducted, as seen in contact electricity. Hence it is assumed that in all bodies the electricity is arranged

G

+ ÷ + ÷ + etc. The internal repelling and attracting forces which are the necessary attributes of matter are furnished by the interaction of the electric charges, and the different properties of the substances are dependent on the different arrangement or layering of the electricities. If the electric equilibrium is disturbed in some way, for instance by the body being connected to a battery, this disturbance will be propagated in undulations through the body by a series of alterations in the distribution of the electricities, and a fresh state of equilibrium will be established under the development of heat. If the disturbance is great a series of considerable and rapid variations in the tensions in various places of the body will occur, and the result will be a series of discharges in the style of immeasurably small electric sparks, and in this case these rapid undulations are the cause of light.

It is interesting to see *Ørsted* evolving an undulatory theory of light at a period when the *Newton*ian emission theory had just made a last bold stand against a dangerous attack from the adherents of the *Huygens* undulation theory. When in the latter half of the 18th century *Euler* had sharply criticised the emission theory, pointing out the advantages of the *Huygens* theory and demonstrating how the difference in colours might be explained by it, *Th. Young* had gone still further and on the same grounds explained the *Newton*ian rings as an interference phenomenon and by means of them found the wave lengths of light of different colours. His work found no adherents, and when *Malus* discovered polarisation in 1810, *Young* could find no interpretation of this phenomenon by his theory, as, like *Huygens*, he only thought of oscillations in the direction of the ray. After this the emission theory again ruled supreme. *Ørsted* was the first to return to an undulatory theory; he retained the idea of oscillations in the direction of the ray, but added the notion of the electric nature of the oscillations.

Ørsted's purpose to give a new theory of heat and light on this basis, suffered, however, from the same sort of obscurity as we have already pointed out above. The concepts of matter and force were confused. He showed in detail how the various effects of heat followed from his theory, but as the foundation of the theory was vague, the explanations remained hazy. The light-theory appeared in more elaborate form a couple of years later.

After it had been shown that the fundamental forces associated with positive and negative electricity are the first causes of all chemical effects as also of heat and light, it remained to be investigated whether the phenomena of magnetism could also be deduced from them. *Ørsted's* reasoning is as follows:[1] —

There are many similarities between the mode of action of the electrical and magnetical forces, but hitherto no connection has been found between them, no trace of a difference in the influence of electric forces on a body when it is magnetic or unmagnetic. *Ørsted* declares that for the present he cannot demonstrate such a difference, but will point out one or two things which seem to indicate that the solution of the problem is not impossible. It seems to be a fundamental distinction between magnetism and electricity that magnetism is not conductible. This difference is not, however, profound; the electricity found on the isolating plate of a condenser is equally latent. From the fact that magnetism does not differ in its effects when the magnet is acted upon by an electric body, we cannot infer that there is no connection; the chemical effect of a galvanic battery remains uninfluenced if, by approaching electric bodies to the poles, their tension, measured by the electrometer, is altered, and yet the chemical effects are due to electricity. Finally, according to *Hansteen's* investigations on the magnetism of the earth, it seems possible to demonstrate a connection between the magnetic poles of the earth and the northern lights, and further, the magnetising power of iron is dependent on the temperature; now, luminous and heating effects being due to the fundamental electric forces, a connection may be traced. *Brugmans* and *Coulomb* having likewise shown that magnetism is present in all bodies, it seems a likely conjecture that the magnetic forces are as universal as the electric. It is then proposed that the experiment should be made whether electricity in one of its most latent forms could act on the magnetic bodies as such. — *Ørsted* referred to this proposal in 1820 when he found a connection between electrical forces and magnetism precisely by applying electricity in latent, i. e. galvanical, form.

It may perhaps surprise that *Ørsted* does not mention *Berzelius* and *Davy* among his precursors in propounding an electrochemical theory; in fact, their well-known theories had allready been advanced before 1812. In 1803 and 1806 *Berzelius* and *Hisinger* published a paper in German and Swedish respectively, communicating the

[1] Ed. Vol. II. P. 146—49.

results of electrolytic experiments, and *Berzelius* set forth the theory that the chemical affinities were due to the opposite electricities. *Davy* advanced the same idea in 1808, likewise in a paper dealing with new experiments. Both these scientists were atomists, each in his way imagining the opposite electricities bound up with the atoms, and both applied the electrochemical theory as a fruitful working hypothesis in further experimental investigations. When now *Ørsted* does not mention these two men among the founders of the electrochemical theory, but, on the contrary, strongly emphasises that already before the Voltaic pile an electrochemical theory had been evolved in connection with *Ritter's* and *Winterl's* works, it may perhaps be taken as a sign of a kind of claim to priority on *Ritter's* and *Winterl's* account. There is the ring of something similar in a remark of his 16 years later, which was quoted above, p. XXXVII. In the same place he writes: »The electrochemical system was bred in several brains even before *Winterl* and *Ritter,* but these two men developed it considerably, each in his characteristic way. No doubt their system was less complete than what is now called the electrochemical theory, but nevertheless, both in name and contents the latter is as onesided as the antiphlogistic theory whose great merits no impartial observer will deny, and this theory had, indeed, even more the merit of novelty than the electrochemical theory«.[1] *Ørsted* was moreover, up to and at the time of writing »Ansicht der chemischen Naturgesetze«, an opponent of the atomic doctrine and may for that reason have considered his »dynamic theory« so different from *Berzelius's* and *Davy's* opinions that he did not count them as his predecessors in regard to theory even though he mentions their experimental work with appreciation.

Ørsted had every reason to be satisfied with the reception accorded to his book. It »attracted great attention and gained great eclat in Germany«.[2] In France it attracted so much attention that a French edition of it was published. *Ørsted* himself states that shortly after its publication *van Mons* dedicated a translation of *Davy's* Chemistry to him and *Berzelius.* In England the book was unknown for several years owing to special circumstances. It was not mentioned until 1815 by *Th. Thomson* in a review in »Annals

[1] Autobiogr. P. 523.

[2] Annals of Philosophy. Vol. 5. P. 5. London 1815.

of Philosophy«. The first article in the periodical for that year opens with the following remarks: »After an almost total exclusion from the Continent for about seven years, all the kingdoms of Europe have been suddenly thrown open; and it has been in our power, by importing the different foreign journals to make ourselves acquainted with the various additions which the sciences have received during this eventful period.«[1] Then follows a report of the important events in the domain of chemical science, one of the items of which is a detailed account af *Ørsted's* book because of the great attention it had attracted. *Thomson* had not, however, had the original treatise at hand, but only an account of it, he therefore declared that some things in *Ørsted's* book which seemed to him »whimsical and absurd« might perhaps appear different to him if he could see the original. Four years later, in Annals of Philosophy (1819), he returned to the subject; *Ørsted* had sent him the French edition and *Thomson* now stated his intention of giving as good an analysis of the book as it was in his power: »The book is highly worthy the perusal of all those British chemists who aim at the improvement and the perfection of their science. It is rather surprising that a work of such originality and value should have remained for these four years quite unknown in this country«.[2] He also says that it will be a pleasure to him »to do justice to Professor *Ørsted* whose knowledge in the science of chemistry, and whose powers of arrangement and generalisation are very uncommon.«[3] *Thomson* then analyses the contents of the book in three comprehensive articles. He states his appreciation of the classifications and the chemical arguments, but declares that he does not understand the heat- and light-theory on account of that very vagueness in the definition of matter and force which we alluded to above.

If now we examine the importance and influence of *Ørsted's* work in regard to the advance of science, if we ask whether it achieved its purpose of taking the first step towards a state in which chemical results could be computed from a knowledge of the laws of the fundamental forces inherent in matter, we must admit that at any rate it is not demonstrable and could hardly be so according to the whole character of the book. It deals only qualitatively with the forces; there is hardly a figure in the book, no single indication of the extent of the

[1] Annals of Philosophy. Vol. 5. P. 1. 1815.
[2] l. c. Vol. 13. P. 369. 1819.
[3] l. c. P. 369.

forces or a quantitative law for what they are dependent on, in short, nothing on which a mathematical treatment could be founded. And yet the book may have been of great importance — it is stated that it attracted attention and achieved success, it may have stimulated thoughtful readers by its ideas and systematics and may thus have led to works in the desired direction, but an influence of this kind will always remain hidden.

When *Ørsted* visited Paris in 1823, and was received and fêted as the discoverer of electromagnetism by all the celebrities, he wrote home: ». . . I see very well that I may expect my electrochemical theory, which I published already long ago, to be understood in particular by the new scientific generation; though I must say that I have not found that the elder generation have entirely accepted it. . . . On many occasions I have perceived that it is almost impossible to make my theory intelligible to Frenchmen without at the same time explaining to them some features of Nature Philosophy«.[1]

Here again, Nature Philosophy was evidently detrimental to him and lessened the importance of his work by giving a touch of vagueness both to matter and form.

At some points he recast and supplemented his theories during the succeeding years. This was the case with the theory of the production and propagation of light which he returned to again and again. In 1815—16 he laid his »Theorie over Lyset«[2] (Theory of Light) before the Society of Sciences. Its presuppossition the identity of electrical and chemical forces already asserted in »Ansicht der chemischen Naturgesetze«; further, it is maintained that every propagation of electricity through a body begins with a polarisation of the medium through which the propagation is to take place, alternate layers of + and ÷ electricity forming and then discharging themselves by undulations, after which fresh polarisation takes place. The possibility of such a polarisation even in conductors is proved by the Voltaic contact potentials between different metals. The main assumption is that heat and light are generated by the union of the opposed electrical forces when this union takes place under resistance. The evolution of heat increases with the resistance and with the quantities of electricity, or, as he puts it, with the quantity of the »forces which act on the conduc-

[1] M. Ø. Vol. II. P. 54. [2] Ed. Vol. II. P. 433.

tor at any moment while the strength measured by the electrometer remains unaltered«.[1] »The galvanical apparatus, especially with large plates, therefore produces much more heat and light than the frictional machine or a battery charged by means of it«.[2]

The evolving of light by combustion is caused by positive electricity in the combustible substance uniting with negative electricity in the firefeeder. The union cannot take place until »the conduction is very perfect«, i. e. when the parts are brought into such close contact by ignition that the passage of electricity can take place. By the alternating polarisations and discharges set up by this, the same phenomena will arise in the medium surrounding the place of combustion, and the process will be continued as rays of light. Along these a kind of electrical polarisation will take place with a following undulatory discharge of the opposite electricities by a passage through the intermediate resisting layer as in a spark. »The mode of action of the forces in light is compared by the author to that which takes place in the electric spark«.[3] »The greatest velocity in the union of the opposite forces gives the invisible rays which appear in the prismatic spectrum next to the violet light. After these rays the violet rays have the greatest velocity of union and so on, according to the order of the colours, up to the red rays which have the smallest velocity. A still smaller velocity of union gives heat rays. The gradual merging of light and heat into each other, as well as all the accompanying phenomena, are easily explained on this theory.«[4] The small spark paths in which the op-

[1] Ed. Vol. II. P. 434.

[2] l. c. P. 434. *Ørsted* remarks with some bitterness in 1817 that this correct observation mentioned in »Ansicht... has been overlooked and forgotten. In *Schweigger's* Journal Vol. 20. 1817. P. 212, he writes: — »Ich sehe, dasz *Children* der Beschreibung seiner schönen galvanischen Versuche einige theoretische Bemerkungen beigefügt, welche mit der von mir früher aufgestellten Wärmetheorie gänzlich übereinkommen. Er ziehet nämlich aus seinen Versuchen den Schlusz, dasz die Leiter von den Durchdringen electrischer Kräfte in dem Grade heisz werden, wie sie Widerstand leisten. Ich habe dieses Naturgesetz schon lange gekannt, und in meinen Ansichten der chemischen Naturgesetze, wie auch in Ihrem Journal aufgestellt, und zwar nicht ohne Gründe, die überzeugen können. Ich habe hierauf eine allgemeine Theorie der Wärme aufgestellt, in welcher ich die Thatsachen auf eine ungezwungene Weise aus dem Grundsatz ableite Woher kommt es denn, dasz in den neuern Schriften so gar keine Rücksicht darauf genommen wird? Ich sehe überhaupt mit Verwunderung, dasz man mehrere rein theoretische Speculationen englischer und französischer Physiker in deutschen Schriften weitläufig auseinandersetzet, während man von den analogen Untersuchungen, welche ich in meinen Ansichten der chemischen Naturgesetze aufgestellt, ein gänzliches Stillschweigen beobachtet, selbst wenn sich daraus noch Berichtigungen holen lieszen für die von jenen Fremden später aufgestellten Sätze. Ich habe ziemlich viel Materialien, womit ich das Gegründete dieser Bemerkung beweisen kann

[3] Ed. Vol. II. P. 434. [4] l. c. P. 435.

posite electricities unite are then interpreted as »the elementary components of light«.[1] »The line between the diametrically opposed points in such an elementary component is called its axis. Its position relative to a reflecting or refracting surface will of course influence the further passage of the rays. Hence this theory seems to accord better than any other with the polarity in the light rays discovered in our day«.

This last explanation evidently did not satisfy *Ørsted*, in later writings we find remarks showing that he was still considering the possibility of finding a better one. When in 1820 he sent out a short account of his discovery of the magnetical effects of the electric current, the last lines dealt with this theme. The newly observed effects gave him the idea that the electrical »conflict«, the discharge between the opposite electricities, in a conductor, took place along a spiral line round the axis of the conductor, and the action on the magnet in the vicinity of the conductor was conceived as an indication that in the medium surrounding the conductor there was also a conflict which followed a spiral line with the windings almost at right angles to the axis of the wire. The action in the conductor during the passage of the electricity may by analogy be transferred to »the conflict« assumed to take place along a ray of light. *Ørsted* then writes: »In a book published seven years ago I pointed out that heat as well as light is an electrical conflict. From the additional observations recently obtained we may now venture to conclude that there is also a circulating motion in these effects. This I think may contribute much towards the explanation of the phenomena comprehended under the name of the polarisation of light«.[2]

Already in 1819 we note this idea of *Ørsted's* that the conflict in the conductor itself does not take place in a straight line along the axis. Among his papers we find an account of some experiments in which heavy currents were sent through metal wires, with the result that their surfaces were altered so that alternate bright and tarnished stripes appeared. »Do these run in serpentine windings«, *Ørsted* asks several times in the notes added. Through the new observation he made of the deflection of the magnet, he saw the continuation of this conflict outside the conductor.

In an account[3] of the electromagnetical discovery read before

[1] Ed. Vol. II. P. 435. [2] Ed. Vol. II. P. 218. [3] Ed. Vol. II. 447.

the Society of Sciences in 1820—21 he again went into the question of the nature of light and the cause of polarisation. He reminded his audience that he had advanced the hypothesis that the propagation of light must be assumed to take place by an undulatory disturbance and re-establishment of equilibrium between electrical forces in space. »The electromagnetical discoveries now seem to lead the way to a better insight into the nature of these undulations. Perhaps the mutual distance between the windings or circles might determine the colour of the rays, and the figure of these windings or circular lines one day serve to explain to us the so-called polarity of light«.[1]

He did not further follow up these ideas nor give them definite form, but it is evident that his imagination was constantly occupied with the problem of the nature of light and its relation to matter by emission and absorption. We see this clearly from some letters to his German friend *Weis,* written in 1828, which take the shape of quite a treatise concerning the internal constitution of bodies. »Briefwechsel über Atomistik und Dynamik« is the general title.[2] We find the same opinions expressed on a sheet among *Ørsted's* papers which bears the title »From a Lecture on Electricity. March 15th 1828«.

From the outset *Ørsted's* theory of light is the issue of his chemical theories of the internal forces of bodies, and it is again closely connected with the opinions he maintains in the above-mentioned letters. His statements denote a break with his early acceptance of *Kant's* ideas of matter, maintained by *Weis* in this correspondence. He does not, like the latter, stop at the attracting and repelling forces of matter as the primary and *a priori* foundation of science, but, taking a step further, he seeks the common cause of these, and thus of other forces of nature, in motion. In a treatise[3] written during this period in an English encyclopedia, we find in connection with some reflections on his »dynamico-chemical theory«, the following statement: »The dynamico-chemical theory must still remain very imperfect, until it is decided if the powers acting in magnetism, electricity, heat, light, and chemical affinities, are to be ascribed to vibratory, circulating, and other

[1] Ed. Vol. II. P. 453.

[2] *Harding's* (still inedited) collection of letters to and from *H. C. Ø.*

[3] Ed. Vol. II. P. 351.

internal motions or not«.[1] In the letters to *Weis* he enters more closely into the last problem.

His new opinion is that all bodies consist of inconceivably small, though not infinitely small, particles which are in an oscillating motion and are thus kept apart. The existence of this internal motion is grounded on the assumption of the oscillating nature of heat and light: »Ich stelle Licht und Wärme zusammen, weil ich immer mehr und mehr in der Meinung befestigt worden bin, dasz Licht und Wärme oscillatorisch sind und dass die Wärme von dem Lichte nur durch langsamere Oscillationen verschieden ist. Dasz alle Körper sich wechselseitig Wärmestrahlen zusenden und zurückwerfen ist anerkannt. Dasz diese Strahlung auch zwischen den Grundtheilen statt findet, geht aus den Thatsachen hervor, denn die Erscheinungen des Eindringens der Wärmestrahlen in die Körper, ihre Zurückhaltung und Wiederausstrahlung sind so beschaffen, dasz man nicht bei einem Hin- und Herstrahlen zwischen den Oberflächen stehen bleiben kann; es musz ein unaufhörliches Hin und Herstrahlen zwischen den Grundtheilen vorgehen. Da aber diese Grundtheile sehr klein sind, so werden sie an den Schwankungen der Wärmeoscillationen mit Theil nehmen, und so ist schon eine innere Bewegung dargethan. Zwischen den Weltkörpern ist die Strahlung unter der Form des Lichts am thätigsten. Das aber zu jeden Planeten ankommende Licht verwandelt sich darauf, wie bekannt, zur Wärme und gibt dadurch der innern Wechselwirkung neues Leben«. — »Wir haben denn hinreichende Gewiszheit von der innern Bewegung der Grundtheile; wir können uns aber kaum erwehren einen Kreislauf an ihnen anzunehmen«. »Es bewegt sich also in jedem Körper eine Welt von Grundtheilen die durch äuszere Kräfte mehr oder weniger beschränkt wird: die sich durch Sonderungen und Vereinigungen der Systeme umbilden können. Durch Wärmestrahlung können diese Theile in weitere Kreisläufe gebracht werden, durch Zusammendrückung oder Entstrahlung in engere«. — This theory is made clear as well in the draft for a lecture as in the German letter; in the latter the reasoning is the most generally valid and runs as follows.

»Der electrische Strohm hat um seine Achse einen magnetischen Kreislauf. . . . . Jede Zerlegung durch einen in gegebener Richtung laufenden, electrischen Strohm, wird von einem Kreislauf

[1] Ed. Vol. II. P. 398.

begleitet. Durch diesen electrischen Strohm von dem ich anderswo erwiesen habe, dasz er in abwechselnden + E und ÷ E fortschreitet, werden eine Reihe von Ladungen der Theile, in der Richtung des Strohms hervorgebracht, und Kreisläufe in darauf senkrechten Ebenen. In so weit die Theilchen von dieser Richtung ergriffen sind, haben sie eine Achse electrischer Ladung und einen Æquator magnetischen Kreislaufes. Da die chemischen Wirkungen, welche ohne regelmässigen, electrischen Strohm hervorgebracht werden, doch gewisz von derselben Thätigkeit herrühren, so existirt dieser Kreislauf in allen chemischen Wirkungen. Wenn aber die trennende und vereinende Thätigkeit ihr Ziel erreicht hat, werden doch jene Ladungen und Kreisläufe in der Anlage dableiben, eben so wie eine Stahlscheibe, durch deren Mittelpunkt eine magnetische Ladung durchgegangen ist noch magnetische Kreispolarität behält, welche am wahrscheinlichsten als eine äuszerst langsamer magnetischer Kreislauf anzusehen ist«.

Through this theory of the internal constitution of bodies we catch a glimpse of an elaboration of his opinions on the mechanism evolved in the emission and absorption of light and heat. If the velocity of the particles of a body in their circulating motions increases, heat-radiation will take place. If, conversely, heat rays penetrate into a body, their oscillations will influence the motion of its particles. Light and heat oscillations are assumed to occur »nothwendig oft in Materien . . . die feiner sind als alle uns bekannten, z. B. die in dem Weltraume verbreitete Materie. Man mag . . . diese Materien Æther oder Ætherarten nennen, oder wie man sonsten will« . . . In his previous framing of the theory *Ørsted* based it on the supposition that the action along a ray in the medium, the ether, is of the same nature as the oscillations, electric polarisations, and magnetic circulation, taking place in bodies during the emission of heat and light. The above-cited conception of the condition in matter may therefore offer a picture of a ray of light.

In these theories *Ørsted* anticipated certain ideas which became the basis of later theories in the same domain. He had notions analogous to the *Faraday-Maxwell*ian ideas about the propagation of light by means of undulatory electric polarisation of the light-propagating medium and a concomitant alternating magnetic field. He had a preconception of the modern ideas about atoms as a kind of solar systems with electrons in motion and corresponding magnetic fields. Thus he employs the following illustration: »Stelle Dir einen Sternennebel vor, worin wir nichts als einen hellen Flecken sehen, unerachtet er aus unzähligen Sonnen und andern Weltkörpern besteht; denke Dir diesen in aller Rücksicht so verkleinert, dasz er in Deiner Hand noch dieselbe scheinbare Grösze habe, wie er in jener ungeheuern Entfernung hatte; er wird Dir jetzt alle Erscheinungen eines Körpers geben, so dasz seine vorher unübersehbaren Entfernungen nun Poren sind, seine innern Bewegungsverrichtungen als physische und chemische Eigenschaften erscheinen. Seine Zusammendrückung würde nicht ohne Widerstand seyn, und die Annäherung der Theile, unzählige Licht- und Wärmestrahlen nöthigen, schneller ihren Lauf hin- und herwärts zwischen den Theilchen zu vollenden, also auch häufiger durch die Oberfläche zu entweichen, also Wärme und Lichtentbindung geben«. — —

Indeed this was all metaphorical and *Ørsted* found no critical mind to build a mathematical system on his ideas, but the very nature of these shows that he belonged to that class of scientists who have power of combination and imagination enough to form hypotheses. He never gave his theory of light in explicit form, and it became little known — it did not, like *Faraday's* electromagnetical ideas, become the starting point of new theories — but it remains to us as a testimony that *Ørsted* had what *Faraday* called »power ot penetrating the secrets of nature«.

Once or twice in his production *Ørsted* incidentally returned to the theory of the nature of light. Through *Fresnel's* researches it had gradually been established that light is due to oscillations of the ether at right angles to the direction of the ray. In 1829 *Ørsted* read before the Society of Sciences some »Betragtninger over Forholdet mellem Lyden, Lyset, Varmen og Electriciteten«.[1] (Remarks on the Relation between Sound, Light, Heat and Electricity). He did not mention the special presupposition of his own theory, but started from *Fresnel's* assumption that light is due to oscillations of the ether. Still, he did not entirely desert his own theory, or, at least, did not abandon what to him was the central idea: »For the rest he would not have it regarded as definitely settled that light consists in oscillations of the ether, he only wanted to show that,

[1] Ed. Vol. II. P. 489.

assuming this opinion, which has gained so much in probability in recent times, to be right, we must imagine the interdependency between electricity, galvanism, and magnetism to be just as uninterrupted as on the theory starting from the electrical forces.«[1] . . »In many poetico-philosophical writings from the last decades of his life, this fundamental conception plays a great part.«[2] Hence the central thing to him is the idea of the unity in the forces of nature; as it was the backbone of his early works when he was under the influence of Nature Philosophy so it remained with him throughout his production to the poetico-scientific works of his old age.

After the completion of the great theoretical works in 1812—13 *Ørsted* returned from abroad where these works had seen the light, and there now followed some laborious years in which only small time was left for scientific work, and in which his achievements were mostly of an experimental kind.

A theoretical paper verging on the practical is his proposal from these years for an altered chemical terminology, a nomenclature founded on old Scandinavian-Germanic words.[3] It was first brought forward at the University Reformation Festival in 1814 and published later.[4] Some of the designations proposed are used in Denmark to this day, such as Brint (= hydrogen), and Ilt (= oxygen), with the derived words at brinte (= to hydrogenise), brintelig (= hydrogenable), at ilte (= to oxidize), iltelig (= oxidable). The paper is an outcome of *Ørsted's* interest in »Sprogforædling«, ɔ: the purification of the language, and was followed by others of a similar kind.[5]

Ørsted himself has described his working conditions in the years about 1815:[6] »In these and the immediately following years his scientific researches were much restricted by his routine work. For several successive winters he lectured for five hours on most

---

[1] Ed. Vol. II. P. 482.

[2] Aanden i Naturen. Vol. I. og II. Kbhvn. 1850 and Saml. og efterl. Skrifter. Vol. I — IX. Kbhvn. 1851—1852.

[3] That the matter occupied his mind for several years and that he sought information from philologists is seen from the following translated letter from *Rask* to *Ørsted* (of Jan. 28. 1812.): —

. . . ›Against Ilt or Ildt may be said that no such ending or mode of derivation is found in the language, by which one noun is formed from another by adding a t, and by which the new noun obtains the sense of the principle or element of the former . . . All words in -t are formed either from adjectives or verbs and denote rather an abstract quality or effect than a concrete element. If I am not mistaken in this, the word can hardly, I suppose, pass for good Danish‹. *Rask* proposed the use of eld or eldr instead, with derivations.

[4] Ed. Vol. II. P. 178.

[5] Saml. og Efterl. Skrifter. Vol. IX. Kbhvn. 1851. P. 1.

[6] Autobiogr. P. 532.

days, in the summer for 2 or 3 hours«. On the 30th of October 1815 he wrote to *Berzelius:* »Even the little I might be able to accomplish I am prevented from carrying out on account of my unfavourable position in the university where an adverse and cunning manager knows how to destroy the advantages which the approval of my countrymen to some degree entitles me to, so that I am compelled to give so many lectures and take upon myself so much other business that I have only little time left for my own work. But enough of this. I dare say I shall get some offer from Germany by which I can tear myself away from this position«.[1] Not until 1817 was *Ørsted* made professor ordinarius.

Besides delivering the lectures *Ørsted* had much work to do in providing and arranging a collection of physical instruments for use during lectures. The foundation of this collection was laid in the years 1804—06[2] when a collection of physical apparatus was given to *Ørsted* through the fund »ad usus publicos«, and by public grants and subsidies from the King he soon brought it up to such dimensions that in 1816 when it was taken over by the university it was insured for 16000 Rdr.

A practical or partly practical and partly scientific matter took up much of *Ørsted's* time for a couple of years. By Royal order *Esmarch, Ørsted* and *Forchhammer* were commissioned to make an inspection of the minerals of Bornholm in 1818—19. The inspection took several months, and two explicit accounts[3] were published and involved some newspaper polemics. *Ørsted* was quite satisfied with the results. He wrote to *Zeise* on the 3rd of Nov. 1818: »The commission, of which I was a member, had the good fortune to discover the finest prospects of real coal, to show that the island harbours a great wealth of ironstone (it consists of carbonate of iron) to find a good lead ore, to open up prospects of a copper mine and to point out many useful applications of the produce of the country besides«.

On the 20th of January 1815 *Ørsted* was made secretary to the Society of Sciences and began to work sedulously to enhance the importance and reputation of the Society. This became to him personally an instigation to keep his knowledge and work on a level with contemporary science. Evidence hereof is found in the

---

[1] *Harding's* Collection of Letters.

[2] *E. A. Scharling:* Bidrag til at oplyse de Forhold under hvilke Chemien har været dyrket i Danmark. Kbhvn. 1857. P. 69. and Ed. Vol. III. The introduction

[3] Ed. Vol. III.

history of the Society of Sciences, edited by *C. Molbech* in 1843, where mention is made of the period from 1815: »Thus we may note that especially the Society's secretary, already from an early part of this period and up to the present time, has made report on a great many scientific notes and briefer communications not printed in the publications, the object of which proceeding was to prevent the passing of a meeting without scientific communications, for want of other contributions or papers read«.[1] Besides this, we find shorter treatises or communications from his hand in all the annual volumes but two, of the Transactions of the Society (Videnskabernes Selskabs Oversigter), from 1815 to 1850.

Among his numerous lectures *Ørsted* delivered a monthly lecture in which he gave an account to advanced students of recent works and progress in science. In trying over the experiments which were to accompany the lectures or communications to the Society of Sciences, he was led to occupy himself with these subjects. It holds especially for these years, but also for later years, that during such work he often found new results and partly followed them up, but was then engrossed by other work and therefore prevented from completing the investigations. Others then found the conclusive evidence he had been on his way to attaining or which he had attained but not published.

Already in early youth *Ørsted* took an interest in the construction of elements and the cause of »galvanism«. His u-tube battery from 1801 occupied him much.[2] In 1816—17 he again took up his work with elements, but this time for the purely practical purpose of inventing an effective and easily available cell. The result was the construction, in 1817, of a battery by him and *Esmarch* in conjunction.[3] The new idea in the construction was that the vessel itself was of copper and formed the positive pole of the cell; the zinc plate in each cell was fastened to a hoop which protruded from the copper vessel of the adjoining cell. The fluid was a mixture which *Ørsted* always used later on, it consisted of water + $^{1}/_{60}$ H_2S_4O + $^{1}/_{60}$ HNO_3 and would probably have the effect of giving the cell about the same electromotive force as a Daniel cell, as HNO_3 would dissolve a little of the copper, so that nearest to this would be found a layer of copper salt. The idea of this mixture

[1] Det kgl. danske Vidensk. Selskabs Historie ved *C. Molbech*. P. 457. Kbhvn. 1843.

[2] See p. XXI and XXIV.

[3] Ed. Vol. II. P. 206.

Ørsted probably got elsewhere. In 1815 Children published a paper in *Schweigger's* Journal on some heating experiments performed by means of a large battery with the same liquid, and *Ørsted* was acquainted with this paper.[1]

During the experiments with the newly constructed battery, the yield of which was tried according to the practice of the time by heating experiments with metal wires, *Ørsted* discovered that the heating of the wire increased when the cell was heated. For this reason some huge cells were constructed in which the fluid could be kept warm by embers. The battery demonstrated before the Society of Sciences consisted of 6 cells, each able to contain 18 quarts; it could make an iron wire of $^1/_{24}$ inch in diameter glow, and even fuse it. In spite of its great effect in this respect, the battery does not seem to have been much used; it was no doubt too large and unwieldy. The first battery, on the other hand, was much used. »With this apparatus *Ørsted* performed many very interesting experiments«,[2] *G. Forchhammer* tells us, »still I shall here only communicate one in which I assisted him in 1818, namely the exploding of mines by making a fine metal wire, which was passed through the powder, incandescent by means of a galvanic current from the apparatus. The experiments succeeded capitally but they became known only within a narrow circle, and not until later, when they were adopted in other countries, did they acquire importance, and have now obtained a varied application«.

When in 1816—17 *Ørsted* demonstrated experiments with the new galvanic batteries before the Society of Sciences, he mentioned an experiment which he and *Esmarch* had incidentally performed, »an experiment which certainly does not actually teach us anything new, but yet exhibits a familiar truth in a new shape«.[3] They had succeeded in producing light by an electric discharge in mercury vapour, and the »familiar truth« was this, that the electric spark consisted in »a violent incandescence of the matter which fills space«. The proceeding is described as follows: »They filled a u-shaped glass tube, one leg of which had a strong capillary constriction, with mercury, and boiled it in the tube. They then brought it into connection with the galvanic circuit and now saw sparks form in the narrow part of the tube, the mercury there alternately being

[1] See p. LV, Note. [2] *G. Forchhammer: Hans Chr. Ørsted.* Et Mindeskrift. Kbhvn. 1852
[3] Ed. Vol. II. P. 437.

made glowing, its parts separating on account of the evolved vapour but again combining upon the quickly succeeding liquefaction of this vapour«.[1]

The battery was also used for electrolytic experiments. It will be remembered that already in 1801 *Ørsted* found important results in this domain, that he constructed a kind of voltameter, and that he found that the quantities of acids and bases appearing at the two electrodes in an electrolyte were chemically equivalent. The researches he now took up did not lead to any result of importance, but were in various respects characteristic. In April of 1819 he had set up a battery of 20 cells with very slight internal resistance, and used it for his electrolytic experiments. Amongst other things he carried out electrolysis of potassium hydroxide with platinum wires for anode and tin or bismuth for cathode. On account of the very strong current he got a disintegration of the tin and bismuth cathodes and thought he had succeeded in forming compounds of these metals with hydrogen — tin- and bismuth hydrides. He made a statement of this before the Society of Sciences, it was not however printed, but an account of the experiments is found among his papers. —

The disintegration of an electrode was first observed by *Ritter* in 1808. When in 1807 *Davy* had produced potassium by electrolysis of fused potash, *Ritter* tried in 1808 whether the nature of the cathode metal had any influence on the isolation of the potassium. Then, on employing a tellurium cathode, he perceived a strange phenomenon. The potassium was not liberated at all, whereas the tellurium cathode crumbled away through an intermediate stage of a tellurium-potassium alloy. *Ritter* supposed that the powder was tellurium hydride, in which opinion *Davy*, who repeated the experiment, joined him. *Ørsted* was the first who saw the dispersion of tin and bismuth, but he made no attempt to pursue the matter further. It was dealt with later by *Magnus* (in 1829)[2] and by *Poggendorff* (in 1848),[3] but did not become of any greater importance until 1898 when the chemistry of colloids came into being. Here again then, *Ørsted* was at the beginning of a road which might have led him to results of interest, but he did not follow the hint which he had received through the peculiar phenomenon of cathodic dispersion.

[1] Ed. Vol. II. P. 437.

[2] *Poggend.* Ann. d. Physik Vol. XVII. P. 521. Leipzig 1829.

[3] l. c. Vol. 75. P. 349. 1848.

In 1820 however, he had quite a different experience. »This year was the happiest in *Ørsted's* scientific life«,[1] he says himself. It was the year of his great discovery, but he was also fortunate in carrying through another, smaller, investigation, in finding a new alkaloid in pepper[2] which he called piperine. Already in 1809 he had touched upon the question.[3] He saw that he got similar precipitates with tincture of galls in a solution of quinine and in an aqueous extract of pepper. Tincture of galls contains tannin which is a reagent for alkaloids, so it is evident that already then he saw, without pursuing the matter further, that both substances contain components with uniform properties. The term »alkaloid« was then unknown. It was not until 1817 that alkaloids, i. e. vegetable substances with alkalic properties were known. In 1817 *Sertürner* demonstrated a substance of this sort in opium and called it morphium, later morphine, in 1818 and 1819 two more were found and to these, then, *Ørsted* added piperine in 1820. The mode of procedure in producing the substance, as well as its properties, were described in a letter to *Schweigger* of February 15th which was published in his periodical. On February 18th *Ørsted* gave an account of his work before the Society of Sciences and finally, on the 14th of March he sent a letter about it to the Journal de Physique which was also published. He wrote to *Schweigger:* »Ich setze meine Versuche über diesen Gegenstand mit Eifer fort,« and gave an account of several problems engendered by these researches, the answer to which he intended to seek through new experiments. This purpose was not realised. At a lecture in April he saw for the first time the magnetic needle deflected by an electric current and hence the course of his researches in the immediate future was determined.

[1] Autobiogr. P. 536. [2] Ed. Vol. II. P. 212 and 444. [3] Ed. Vol. II. P. 212 note.

ØRSTED'S discovery of electromagnetism came as a surprise to everybody and apparently without any preparatory work. It was communicated in a paper which in the briefest form possible gave an account of the conditions under which the experiments were made, and their results. These are many and all correct, but no drawings, no indications of series of experiments, or sequences of series, indicate the way these results were reached. Now that we look at the matter historically, and know the significance of the discovery, certain questions inevitably arise. What was the process of evolution which led up to the discovery? What new idea gave the impulse? What account of experimental work were the published results based on? I hope to be able to show that it is possible to get *Ørsted's* own answer to these questions by the aid of his published writings and his posthumous papers.

In the Edinburgh Encyclopedia, ed. *Brewster*, vol. XVIII, 1830, under the head of »Thermoelectricity« is found an interesting article by *Ørsted* about the questions here touched upon, the article containing, despite its name, a sketch of the discovery and development of electromagnetism.[1] That this is to be found in so late a volume of the work and under this heading is simply due to the fact that the work had been under publication for 20 years so that the concept »electromagnetism« did not exist when the volume was printed in which it ought to have been dealt with. In his preface to the volume containing *Ørsted's* contribution — the last of the encyclopedia — *Brewster* regrets that the publication has taken much longer than originally expected, but this very delay, he says, has involved some advantages: »Had the work been completed at the time originally contemplated, it must have been deprived of many of the best articles which it contains, written by Individuals of the most distinguished eminence in science and literature. The return of peace to Europe gave a vigorous impulse to scientific inquiry; and new sciences were created, which were not even known by name at the commencement of the work. Two of these, namely, the Polarisation of Light,[2] and Electromagnetism,[3] have been fully treated in the latter volumes of the work, the last

[1] Ed. Vol. II. P. 351.
[2] »See Optics, Part I Chap. VI and VII«.
[3] »See our Article Thermo-Electricity, under which the science of Electro-magnetism is given.«

of these articles having been written by Professor *Ørsted*, the distinguished philosopher to whom that science owes its existence«.

In the historical introduction to the article in the Encyclopedia *Ørsted* gives a sketch both of the ideas on the subject which were current when he began to work at it, and of the way in which his new ideas developed; as the latter account accords with the impression we receive of this development from his published writings as described in the preceding pages, we may safely assume that it will hold good for 1820, even if it is his view of the matter of about 10 years later which is directly expressed in the article. Among his papers are a number of loose, unarranged sheets which, read in succession, will give us an answer to the question about the experimental work which was the base of the discovery.

We shall now first quote the part of the above-mentioned introduction which deals with his own work.

»Electromagnetism itself, was discovered in the year 1820, by »Professor *Hans Christian Oersted*, of the university of Copenhagen. »Throughout his literary career, he adhered to the opinion, that »the magnetical effects are produced by the same powers as the »electrical. He was not so much led to this, by the reasons com»monly alleged for this opinion, as by the philosophical principle, »that all phenomena are produced by the same original power. In »a treatise upon the chemical law of nature, published in Germany »in 1812, under the title *Ansichten der chemischen Naturgesetze,* and »translated into French, under the title of *Recherches sur l'identité* »*des forces électriques et chymiques*, 1813, he endeavoured to estab»lish a general chemical theory, in harmony with this principle. »In this work, he proved that not only chemical affinities, but also »heat and light are produced by the same two powers, which pro»bably might be only two different forms of one primordial power. »He stated also, that the magnetical effects were produced by the »same powers; but he was well aware, that nothing in the whole »work was less satisfactory, than the reasons he alleged for this. »His researches upon this subject, were still fruitless, until the year »1820. In the winter of 1819—20, he delivered a course of lectures »upon electricity, galvanism, and magnetism, before an audience »that had been previously acquainted with the principles of natural »philosophy. In composing the lecture, in which he was to treat »of the analogy between magnetism and electricity, he conjectured,

»that if it were possible to produce any magnetical effect by electricity, this could not be in the direction of the current, since this »had been so often tried in vain, but that it must be produced by a »lateral action. This was strictly connected with his other ideas; »for he did not consider the transmission of electricity through a »conductor as an uniform stream, but as a succession of interruptions and re-establishments of equilibrium, in such a manner, that »the electrical powers in the current were not in quiet equilibrium, »but in a state of continual conflict. As the luminous and heating »effect of the electrical current, goes out in all directions from a »conductor, which transmits a great quantity of electricity, so he »thought it possible that the magnetical effect could likewise eradiate. »The observations above recorded, of magnetical effects produced »by lightning, in steel-needles not immediately struck, confirmed »him in his opinion. He was nevertheless far from expecting a great »magnetical effect of the galvanical pile; and still he supposed that »a power, sufficient to make the conducting wire glowing, might »be required. The plan of the first experiment was, to make the »current of a little galvanic trough apparatus, commonly used in »his lectures, pass through a very thin platina wire, which was »placed over a compass covered with glass. The preparations for »the experiments were made, but some accident having hindered »him from trying it before the lecture, he intended to defer it to »another opportunity; yet during the lecture, the probability of its »success appeared stronger, so that he made the first experiment in »the presence of the audience. The magnetical needle, though included in a box, was disturbed; but as the effect was very feeble, »and must, before its law was discovered, seem very irregular, the »experiment made no strong impression on the audience. It may »appear strange, that the discoverer made no further experiments »upon the subject during three months; he himself finds it difficult »enough to conceive it; but the extreme feebleness and seeming »confusion of the phenomena in the first experiment, the remembrance of the numerous errors committed upon this subject by »earlier philosophers, and particularly by his friend *Ritter*, the claim »such a matter has to be treated with earnest attention, may have »determined him to delay his researches to a more convenient time. »In the month of July 1820, he again resumed the experiment, making use of a much more considerable galvanical apparatus. The

»success was now evident, yet the effects were still feeble in the »first repetitions of the experiment, because he employed only very »thin wires, supposing that the magnetical effect would not take »place, when heat and light were not produced by the galvanical »current; but he soon found that conductors of a greater diameter »give much more effect; and he then discovered, by continued ex»periments during a few days, the fundamental law of electromag»netism, viz. *that the magnetical effect of the electrical current has »a circular motion round it.*«

This historical survey is characteristic of *Ørsted* in its whole strain and argument, and exhibits in brief his early views on philosophical and physical science. It gives, however, more than that, it gives us information of peculiar interest concerning his discovery of electromagnetism: we get to understand that connection with his general scientific views of the universal significance of the electric forces which he himself so often referred to, and in few words it tells us at the same time what the new thought was which in April 1820 led from the old to the new order of things. This information is contained in the words: »As the luminous and heating effect of the electrical current, goes out in all directions from a conductor, which transmits a great quantity of electricity, so he thought it possible that the magnetical effect could likewise eradiate«, and also: »In composing the lecture, ... he conjectured, that if it were possible to produce any magnetical effect by electricity, this could not be in the direction of the current, since this had been so often tried in vain, but that it must be produced by a lateral action«. We are also told why *Ørsted* in his first experiments used a battery so large »that it will make a conducting wire glowing«.[1] It was owing to the new idea which had led him to try the experiments: »still he supposed that a power, sufficient to make the conducting wire glowing might be required. .... supposing that the magnetical effect would not take place, when heat and light were not produced by the galvanical current«. The new line of thought, or idea, is then as follows: The electrical »conflict« or »interbattle« in the conductor between the opposite electricities produces many effects, partly chemical in the conductor in the direction of the current, partly heat and light effects which radiate in all directions from the conductor; might it not be possible that the magnetical

[1] Ed. Vol. II. P. 215.

effect, so often sought in vain, accompanies these latter and is a special action of the same forces that are found in light and heat rays, just as such different things as wind and sound are due to analogous causes?

Of the very evening lecture in which the first glimpse of the discovery appeared before the eyes of *Ørsted* and his audience we have a brief account by *Hansteen* in a letter to *Faraday* (Dec. 30, 1857). *Hansteen* was *Ørsted's* pupil and was on friendly terms with him until his death, and *Ørsted* encouraged *Hansteen* in many ways and showed him great kindness. The letter which is written in English to *Faraday* does not quite correspond, however, to the picture of friendship revealed by his correspondence with *Ørsted*, the portion of the letter describing *Ørsted's* first experiment runs as follows: »*Ørsted* tried to place the wire of his galvanic battery perpendicular (at right angles) over the magnetic needle, but remarked no sensible motion. Once, after the end of his lecture, as he had used a strong galvanic battery to other experiments, he said, »Let us now once, as the battery is in activity, try to place the wire parallel with the needle«; as this was made, he was quite struck with perplexity by seeing the needle making a great oscillation (almost at right angles with the magnetic meridian). Then he said: »Let us now invert the direction of the current«, and the needle deviated in the contrary direction. Thus the great detection was made; and it has been said, not without reason, that »he tumbled over it by accident«. He had not before any more idea than any other person that the force should be *transversal*. But as *Lagrange* has said of *Newton* in a similar occasion »such accidents only meet persons who deserve them«.«[1]

Hansteen cannot have been an eye-witness of the discovery; it appears from his correspondence with *Ørsted* that he was not in Copenhagen at the time, so his account must be founded on the reports of others. *Hansteen's* remark that *Ørsted* »tumbled over the discovery by accident«, that it was due to a mere chance, is opposed to *Ørsted's* own statement both in the above-mentioned English paper and in other places. *Ørsted* has publicly protested[2] against a similar utterance from *Gilbert,* the editor of *Gilberts* Annalen der Physik, and this protest is all the more worth noticing

[1] Life and Letters of *Faraday* by *B. Jones.* London 1870. Vol. II. P. 389.

[2] Ed. Vol. II. P. 225.

as *Ørsted* did not generally remonstrate when any one tried to depreciate his merit with regard to the discovery of electromagnetism by asserting prior claims or in other ways.

After the succesful issue of the experiment at the lecture, *Ørsted* waited three months until his many duties left him a consecutive space of time to enquire more closely into the matter. He had been successful by following up the idea that the magnetic action of electricity accompanied heat and light rays from a wire which was made incandescent by the electric conflict, it was therefore only natural that for use in his further experiments he procured a battery specially suited for making wires glowing. Such was the battery constructed by him and *Esmarck* which, by the size of its cells, and the very small internal resistance consequent thereon was able to produce a sufficiently strong current to make even long, thin wires glow, and which could moreover, by the mixture of fluids employed, be kept constant for a considerable time. By means of this battery then, in the course of a few days in the month of July the fundamental laws for the direction of the electromagnetic force were found. The question is now: how well were these laws supported? Through what series of experiments was the result reached?

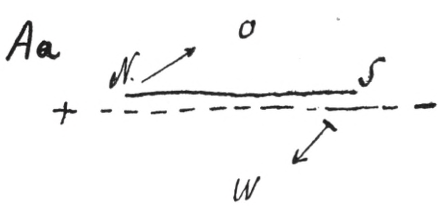
Among *Ørsted's* posthumous papers are found some sheets containing drawings and notes referring to electromagnetic experiments; some are dated July 1820, others are undated, but judging by their contents they must belong to the same time; some of the notes are found in duplicate, f. inst. a draft in the hand of *Ørsted*, a fair copy in somebody else's handwriting with comments by *Ørsted*, or vice versa. We also know from *Ørsted's* own account that there were witnesses when the experiments were carried out, it is therefore not improbable that one of the witnesses assisted in taking down the results of the experiments. These papers, which will now be given in the following,[1] exhibit the course of the experiments and thus show how carefully that substructure was laid, on which the results were built up.

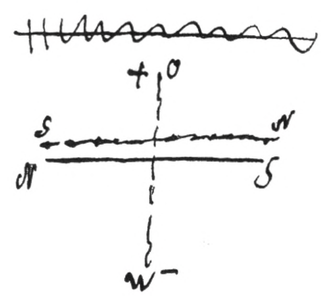
If we imagine ourselves in *Ørsted's* place at the beginning of the experiments we see that he had the following results to start from: he had seen that a conducting wire placed at right angles to the

[1] The sheets will be given under the name of supplements. The drawings and one of the sheets — a part of supplement IV, p. LXXXII—LXXXV — will be given in facsimile, in the others sheets the text will be printed to make there more readable.

magnetic needle had no effect, whereas a conducting wire parallel to the needle showed a lateral action and deflected it; his old notion about a connection between the forces of nature, between electric forces, chemical effects, heat, light, and magnetism, had led to this temporary result and thus assumed increased importance to his mind. He would then probably in the first place wish to try the effect of the electric current in other positions in relation to the needle, and especially to try the effect on each pole separately; but at the same time old thoughts and plans have no doubt crossed his mind, his friend *Ritter's* results with the galvanic pile, his friend *Hansteen's* corroboration of the fact that a magnet would act on a »silver tree« formation, the »arbor dianæ«,[1] the notion of a connection between heat and magnetism, may have obtruded themselves and claimed examination in future experimental investigations. The first sheet of *Ørsted's* electromagnetical papers communicated here shows this very point of view, and though it is undated, it may be taken for granted that it shows the very first working plans from the days in July. Its whole appearance has the character of a draft as shown in the following:

Supplement I



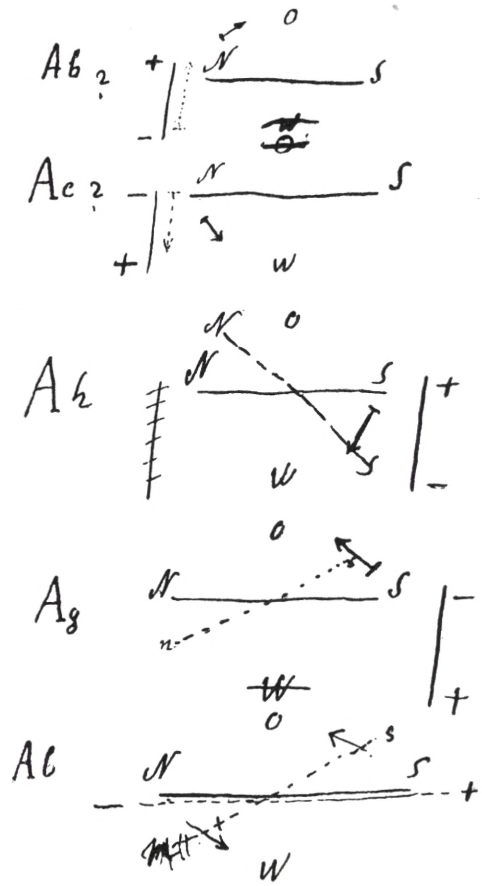


ligeledes maa prøves Traader i horizontal Stilling parallel med Naalen, paa østlig og vestlig Side.

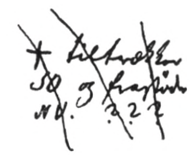
Omdreining ved Electrisk Udstrømning.
En sluttet el. Støtte bragt i samme Stilling mod Magneten som de galvaniserte Traader. — En usluttet ligesaa.
Electrisk Gnist i Therpentinolie nær ved Magnetnaalen.
En umagnetisk Naal bragt i samme Forhold som den magnet. — Blandt andet en Jerntraad.
En stor galvanisk Udladning i Nærheden af en chemisk Metaltrædannelse. —
Det samme forsøgt mod en Galvanisk.
En stor galvanisk Udladning i Nærheden af et sig opløsende Metal.
I Nærheden af en mættet Saltopløsning.

[1] Ed. Vol. II. P. 356.

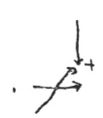
Supplement I



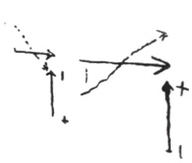
[1]

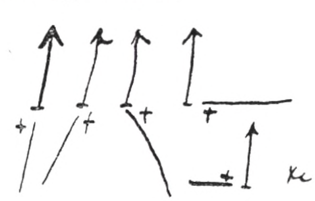


En pludselig Afkjølning i Galvanomagneten.
Varm Metaltraad uden Galvanisme bragt i Forhold til Magnetnaalen.
Hvilken Varmegrad er den meest Magnetiske?
Kan en galvaniseret Traad dreie sig mod Nord og Syd?
Skulde ikke en Pind af Træe kunne modtage Polaritet?



Der maa prøves Traader i horizontal Stilling og i den forlængede Magnetretning, saaledes





[1] If these four drawings are compared with the more detailed ones on p. LXXIX it will be seen that the conducting wire is supposed to be held vertically at the poles.

Under the mark *Aa* are first sketched the two experimental results on which the further experiments were to be based, viz. the action of a conductor placed above the magnetic needle in positions parallel to as well as at right angles to it. The needle is denoted by a fully drawn line and its poles by *N* and *S*, while the conducting wire is denoted by a dotted line with + where the positive electricity enters and ÷ where it leaves the conducting wire. Under *Aa* ought also to have been noted *Al*, which is found further down the page, and which indicates the action when the direction of the current parallel to the needle is from *S* to *N*, while *Aa* gives the action when the direction of the current is *N* to *S*. If *Hansteen's* account is correct, *Ørsted* tried both directions already at the lecture and saw that the needle was deflected in opposite directions in the two cases, as indicated in these drawings. Further, it is intimated that new positions of the conducting wire east and west of the needle must be tried, and then follows a series of plans for experiments for showing a further connection between galvanic, magnetic, and chemical effects of the kind mentioned in the English article as leading up to the discovery. Between these plans, under the marks *Ab Ac Ah Ag Al*, are placed 5 drafted drawings of which *Al* has been mentioned above; the other four indicate various experimental arrangements, and the results obtained, of a conducting wire placed vertically with the current flowing up or down near the north or south poles of a horizontal magnetic needle; these experiments have presumably been carried out to try the action of the current on each separate pole. The direction of the deflection of the pole affected is indicated by an arrow. In the two first drawings, *Ab* and *Ac*, the direction of the movement of the north pole has been incorrectly given if, as the drawing seems to imply, we are to imagine that the conductor is held in front of the north pole in the plane of the magnetic meridian; a note of interrogation has been placed against these drawings by the writer. The uncertainty indicated by this may perhaps be explained by the crossed-out remark. If the conductor in *Ab* is held south east of the north pole, this latter will move towards the east; if it is held north east of the pole, it will move towards the west; the cancelled remark shows that this has been observed, but the many notes of interrogation added, and the fact that it has been crossed out indicates that in these first preliminary experiments it was not yet distinctly perceived that, as long as the position of the wire has not been more sharply defined than by »vertically at the north pole«, the result will be uncertain, even if the direction of the current is indicated. It is this very expression which has been used about the position of-the conductor in these cases in a second sheet, here annexed, where we see formulated the results of the experiments sketched in the first supplement. It is shown how these results were first found with an incandescent platinum wire, but how the experiments were then carried out with non-incandescent wires of divers materials. Of this explanation we have both a draft written in *Ørsted's* hand on acid-stained paper, and an almost identical fair copy in another handwriting. It should be observed that the experiments only concern the action of the electric current on the magnetic needle, the other plans indicated in the first supplement have been laid aside.

Supplement II *En temmelig fiin Platintraad af 3—4 Tommers Længde glødet i vertikal Stilling ved Nordpolen frembragte naar*

den ÷ Leder var nederst Frastødning mod Øst }[1]
» + » » » » » Vest }

ved Sydpolen

» ÷ Leder » » » » Vest
» + » » » » » Øst

(i Kladden: »*Traaden glødet over Naalen det samme; men svagere*«)[2]

Glødede Traaden (i Kladden: *langs Naalen*) *i horizontal Stilling 1 Tomme over Magnetnaalen, saa frembragte den følgende Virkninger:*

1) Naar den laae i Magnetens Meridian + Leder mod Syd: Frastødning fra Syd til Øst
(i Kladden: *Sydenden stærkere Øst end før*)*—Leder mod Syd: Frastødning mod Vest*

2) ⊥ Magnetens Meridian næsten ingen Virkninger.

En temmelig tyk Metaltraad

af ubekjendt Komposition glødede ikke, men frembragte, vertikal og horizontal igjennem Meridianen samme Virkninger i samme Stillinger, som Platintraaden.

⊥ Meridianen ÷ Øst: ingen Virkning; drejede man den i samme horizontale Plan om en Axe gjennem Kompassets Midte, saa bevægede sig Sydenden mod Vest.

var + mod Øst, vandrede Naalen fra Syd til Øst over 180°, og vedligeholdt sin Stilling, saalænge Traaden under sluttet Kjede forblev i Stillingen (i Kladden: *Forsøg gjentaget samme Stilling kun i modsat Retning. NB. Mistænkeligt*).

Aftoges Glaspladen, viiste sig det samme. Siden efter under en svagere Virkning af Batteriet frembragtes ikke disse Frastødninger.

Stanniolstrimler frembragte under de samme Stillinger de samme Virkninger.

---

[1] Correspond to the doubtful drawings *A b* and *A c* in supplement I.

[2] This must signify that the wire is held vertically above the pole.

Et lodret Rør med caustisk Natron frembragte naar ÷ var nederst ved Sydpolen Supplement II
nogle Afvigninger mod Vest

omvendt frembragte modsatte Virkninger.

horizontal + mod Syd Afvigning mod Øst.

— ÷ *mod Syd Afvigning fra Syd til Vest.*

Gnist ved Nordenden frembragte Afvigelse fra Nord mod Øst.

En Jerntraad No. 13, 1$^1/_2$ Fod lang frembragte vertikal og horizontal gjennem Meridianen samme Virkninger som de øvrige ligesaa en Messingtraad af No. 12. Derimod ⊥ *frembragte selv under Forkortning til 3 Tommer ingen kjendelig Virkning saaledes som med Metaltraaden.*

Kladden lyder: *En Traad $^3/_4$ Al. Jern Magnetens Mer.*

+ *mod Syd. Frastødning fra Syd til Øst. omvendt — omvendt.*
Lodret paa Magneten
+ *mod Vest ingen Virkning*
÷ » » *ei heller*
Traaden forkort. 1 Fod ei heller
9 Tom. ei heller
6 Tom. —
endnu kortere
ingen.

Endvidere i Kladden: *Messingtraad*
+ *mod Syd Naalen gik Syd mod Øst*
+ *mod Nord omvendt*
+ *mod Øst næsten ingen*
+ *mod Vest.*
Experimentet med . . . . virkede ved svagere Virkning.

The results of the experiments given in the 2nd supplement suffer as yet from some uncertainty, the indications of the position of the conductor in relation to the magnetic needle being not always sufficiently accurate. It was mentioned above that »vertically at the north pole« is not a sufficient indication. In the cases mentioned in supplement II where the conducting wire is first held at right angles to the magnetic needle and is then moved in a horizontal plane above the magnetic needle, the direction of the movement is not indicated; a remark in the draft for this experiment shows that this deficiency has been felt.[1] This uncertainty has been done away with in the third sheet, supplement III, which will now be given. This sheet is dated the 15th of July and contains in systematic order, drawings and explanations of the experiments referring to the action of the conductor on the needle mentioned as performed or planned in the two papers first

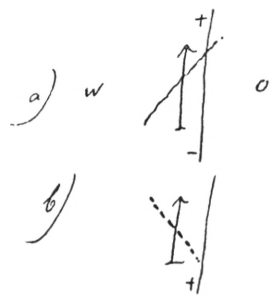
[1] See p. LXXVI. »NB. Suspicious«.

quoted. The sheet now referred to is a folio halfsheet, folded into two quartos, these have again been folded over lengthwise and thus divided in two. Notes and text on the right hand side are written in *Ørsted's* handwriting, those on the left perhaps in that of an assistant. The uncertain statements from supplements I and II of the action of the vertical conducting wire on the north pole have here been corrected under the designation »copy« p. LXXX, and it is implied that the conducting wire is held in the magnetic meridian.

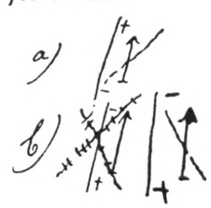
Supplement III

En Metaltraad parallel med Magnetnaalen.

A. Østen for samme



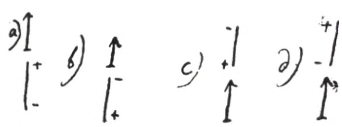
B. Vesten for samme



C. Over Magnetnaalen
a) med + over Nordpol
b) med + over Sydpol

langt stærkere end i A a og B a

D. Metaltraaden i den forlængede Magnetretning



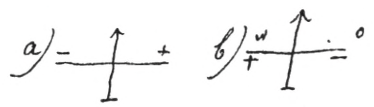
virker svagt

Traadens Art — *Naalens Længde*
— *Længde* — $2^1/_2$ *Tom*
— *Tykkelse*
Skærm af Glas hindrede ei
— *af Metal stærkere end ved* [?]
— *af Træe hindrer ikke*
— *af Papir*
— *af Glas og Met. hindrede ei*
I Forsøg. Platintraad $^1/_{100}$ *Lin.(?) 9 Tom*

Supplement III

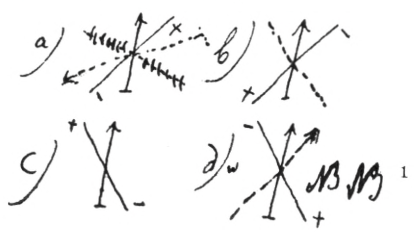
Metaltraaden gjorde Vinkel med Magnetnaalen.

E. Skar den i en ret Vinkel og gjennem Midtpunktet

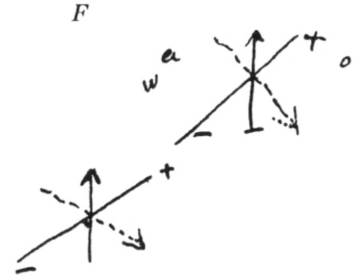


Ea
Eb } *ringe Bevægelse*

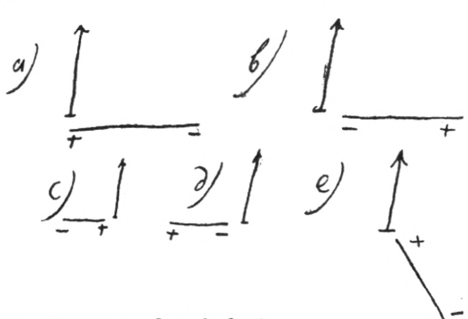
F. Skar den under en spids Vinkel


[1]

F

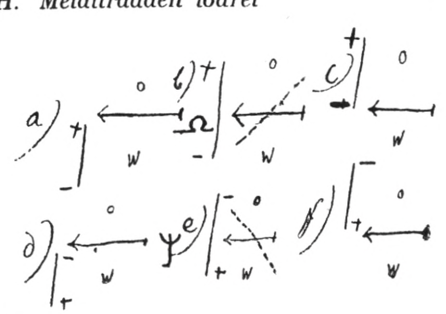


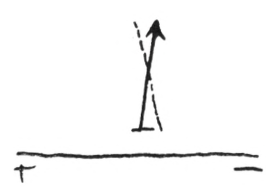
G. Skar den forlængede Magnetnaal



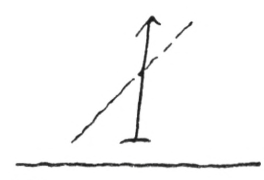
Afvigningen meget svag

H. Metaltraaden lodret



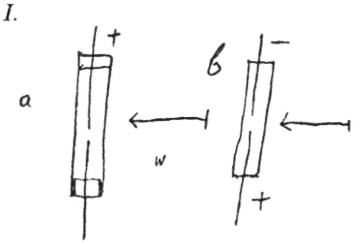


meget liden Vink.



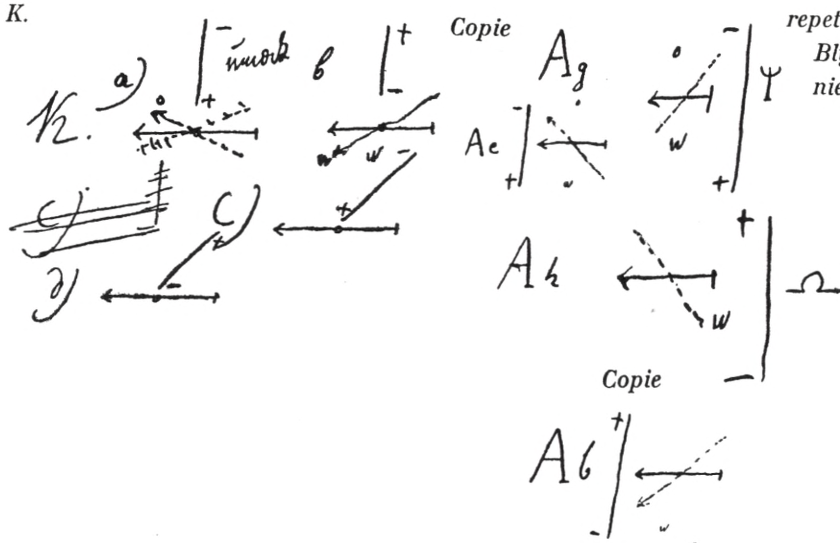
meget større Vink.

I.



[1] a) and d) are wrong. a) is corrected on the right hand part of the sheet.

Supplement III *K.*



repeteret med en Blystribe 7 Linier br.

d. 15de July[1]

med Blye gav samme Resultat som med Platintraad.
Enden af den negative Leder lagdes ∓ over Magnetnaalen og Udladningen skeede i Syd og Naalen gik mod N.W. Udladningen mod Nord — Naalen i NO.
Enden af den positive Leder lagdes over Naalen. Udladning ved Syd Naalen gik mod NO 50°.
Udladning ved Nord Naalen gik mod NW. 50°.
† *Enden af Lederen lagdes perpendiculair paa den magnetiske Meridian ingen Bevægelse ved Udladningen.*

Supplement III contains the result of the action of the conducting wire on the needle in two main cases: when the conducting wire is held in various different positions parallel to the needle, and when the conductor is held at various acute angles or in different ways under right angles to the needle. Finally it is mentioned that the action is transmitted through various substances. On the last page of the half-sheet we notice how carefully it is indicated whether it is the end of the positive or of the negative conductor which is placed above the needle; this no doubt implies that possibly one or the other of the electricities predominates during the action,[2] a thought which is already contradicted by these experimental results. Furthermore it is the first time that any indication af quantities is given in these notes, the size of the rotation angle being given. This is done largely in the next series of experimental data given in supplement IV.

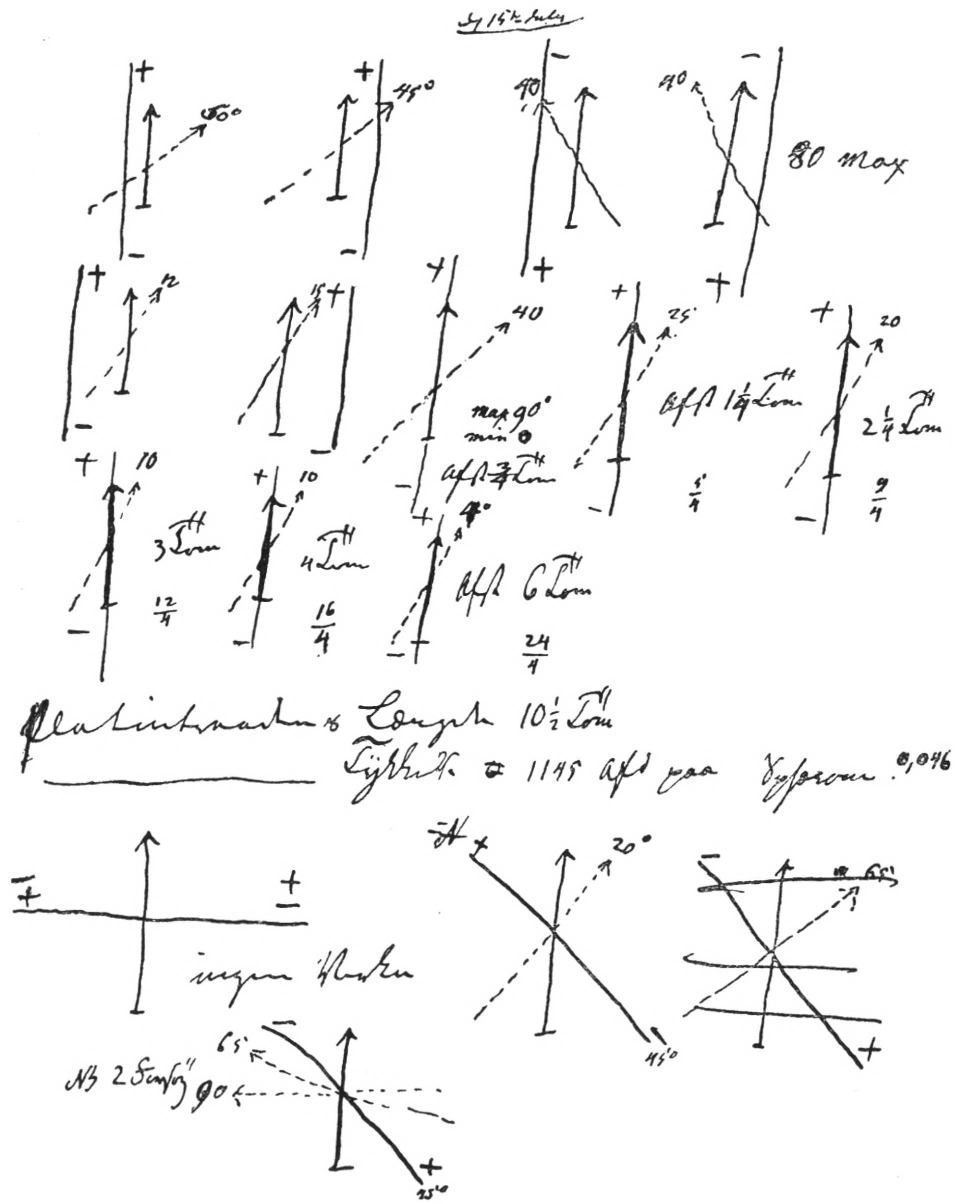
[1] The last page of the half-sheet. [2] The same idea P. LXXXVI. l. 1—4.

In this is given the contents of a larger sheet dated July 15th and three smaller ones which by their contents belong to it. These notes in the main give the material for the famous communication (the programme) of the 21st of July 1820. The contents are primarily a series of systematically arranged drawings giving the results of experiments with the conductor in all sorts of positions relative to the magnetic needle; if we compare this with the contents of the last mentioned sheet (supplement III), we find that something has been omitted and some new material added. The experiments corresponding to *D* and *G*, where the wire lay in the prolonged direction of the magnet, or where the conductor crossed the prolonged direction of the magnet, have been omitted; the reason for this omission is obvious; from the remarks accompanying the experiments we see that the results were uncertain or negative. New additions are: the results of experiments with the conductor below the magnet; observations on the dip of the needle when it is in such a position in relation to the conductor that the force from the latter is not horizontal, the dip has been specially observed when the conductor was parallel to the magnetic needle in the same horizontal plane; finally, an explicit account of experiments in which a vertical conductor was placed in various positions near each of the poles. Further it is proved, by observing the size of the angle when a conductor is held at various distances above the magnet, that the action of the conductor on the pole is diminished with the distance.

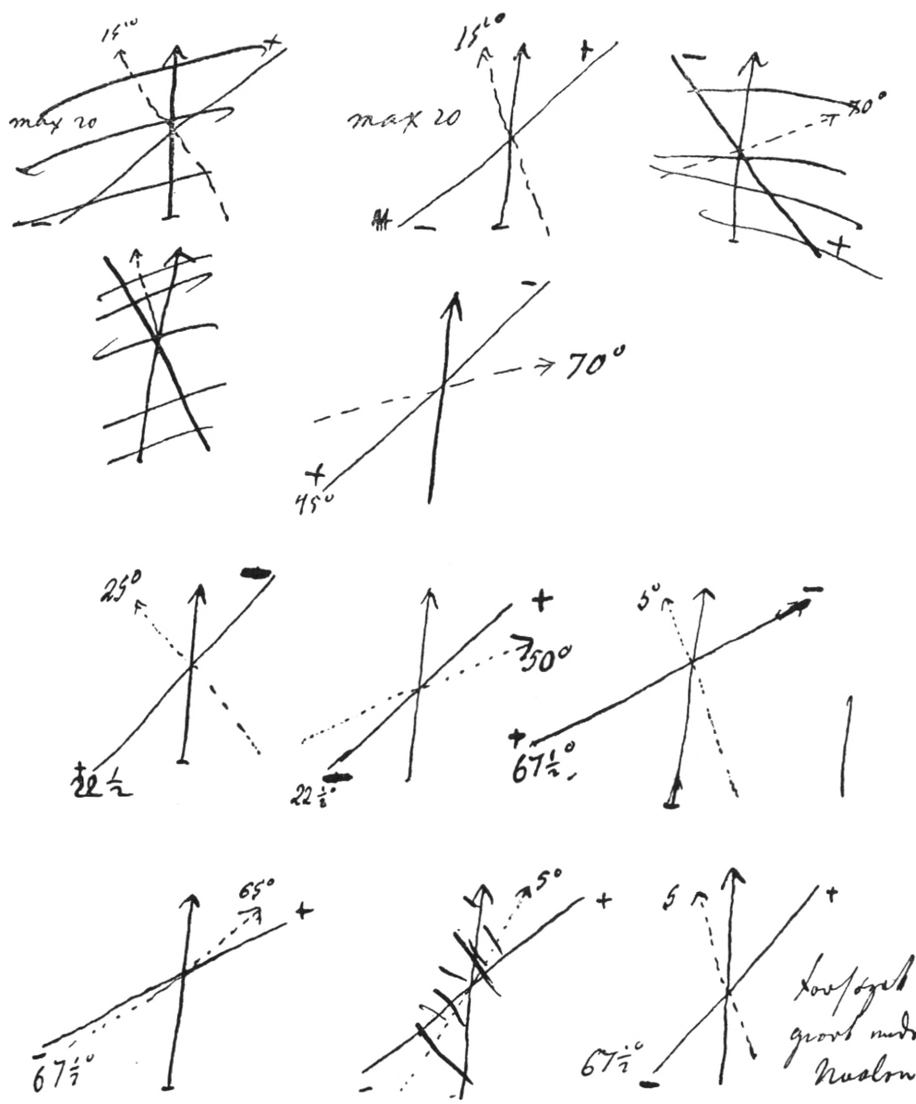
The second main part of the contents is a survey of the results of experiments with the »galvanical fork«. The latter is made of a conducting wire bent in a plane, and gives one turn of a solenoid with a north and a south side. Thus *Ørsted* saw already at this period that a closed electrical circuit acts like a magnet, but he has not sufficiently defined its position to the needle.

After these remarks follows supplement IV. Its first part — the larger sheet — is given in facsimile, in the first place in order to show the character of the drafts and *Ørsted's* handwriting which is easy to read in these pages, and in the second place to show the attempt to arrange the experimental results systematically.

Supplement IV

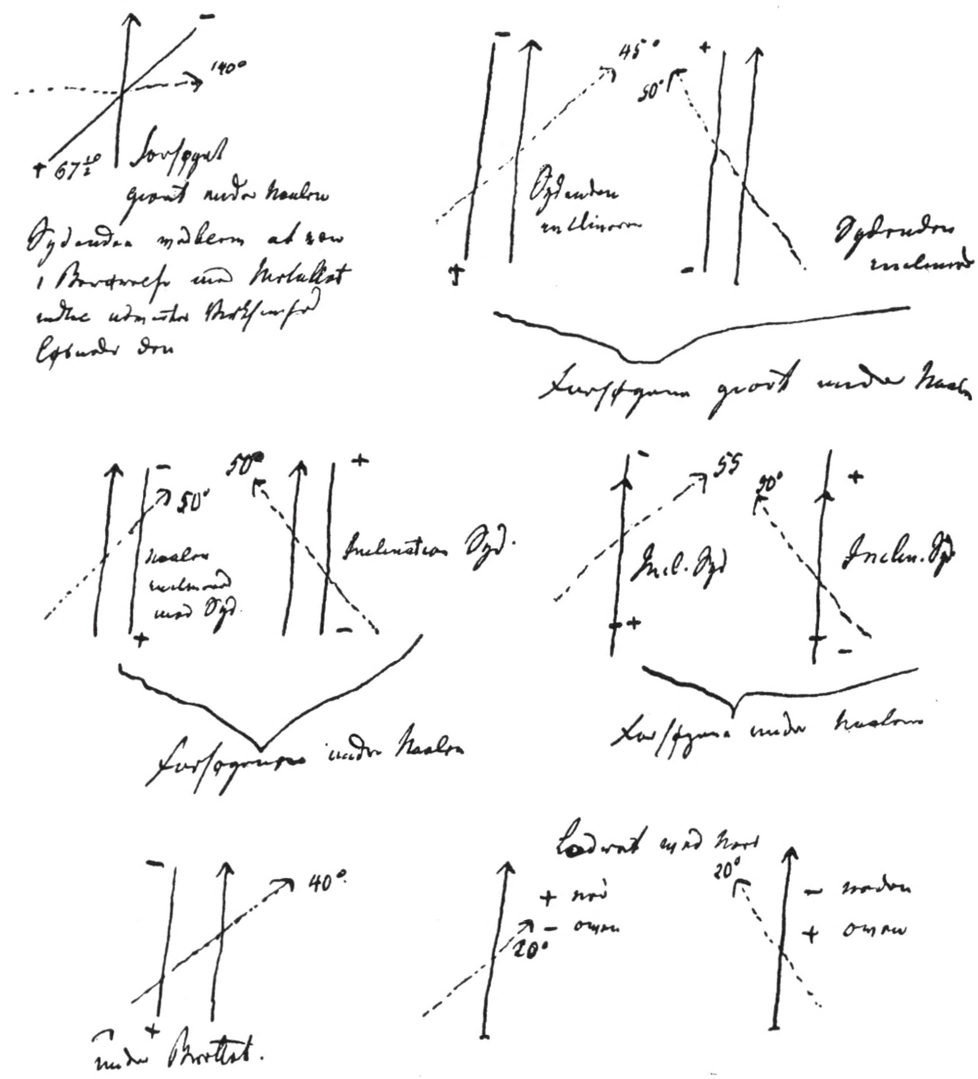


Supplement IV



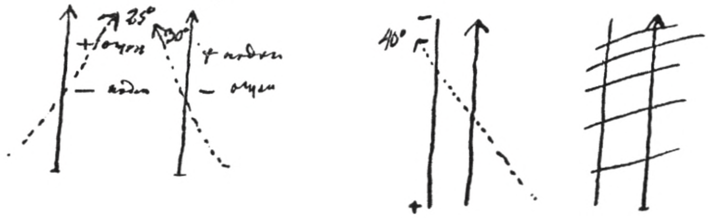
Some of the drawings on this page illustrate experiments with the conductor under the needle, others the reverse.

Supplement IV



Supplement IV

Lodret, med Syddenden



Traaden dyppet i Vandet ingen Virkning

Traaden i Berøring i Vandet med Nordenden paa begge Sider Tiltrækning

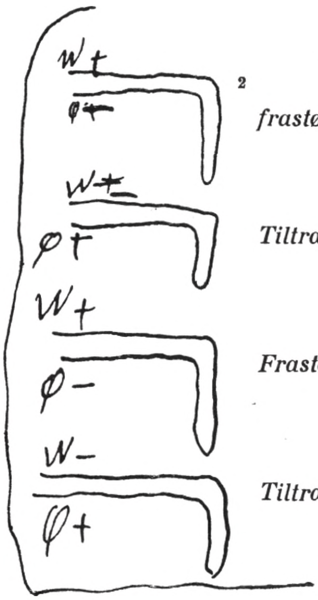
Traaden bøiet i Berøring i Vandet med Syddenden paa begge Sider Frastødning

Det samme Ende som Udladnings Ende over Vandet i lige Forsøg.

\+ Enden i Syd } begge i Berøring med Vandet paa Fra-
− Enden i Nord } stødning i NO og S.W.

Omvendt paa Omvendt

Supplement IV [1] *I Naalens Plan + ved Syd. Nedtrykn.*
— + ved Nord Nedtrykn.
Østen for Naalen
Vesten for Naalen nedtrykte det negative



[2] *frastøder ved Nord*

Tiltrækning ved Nord

Foran Polen

Frastødning ved Syd

Tiltrækning ved Syd

Mellem Pol og Midtpunkt det modsatte

[3] *Galvanisk Gaffel dens Plan lige for Sydpolen*

den vestlige Del ÷	*- - —*	*Frastødning* [4]
Planen østen for Sydpolen omtrent $^1/_2$ *Tomme*		
÷ nordlig	*— — —*	*Frastødning*
Planen østen for Nordpolen ÷ nordlig	*—*	*Tiltrækning*
		omvendt Frastødning
Planen lige for Nordpolen, ÷ østlig		*Frastødning*
» vesten » — + sydlig		*Frastødning*
Planen vesten for Sydpolen + nordlig		*Frastødning*
Planen over [5] *Sydpolen ÷ vestlig*		*Frastødning og Nedtrykning*
» over Nordpolen ÷ vestlig		*Frastødning og Nedtrykning*
» under Nordpolen ÷ vestlig		*Tiltrækning*
÷ østlig		*Frastødning mod Vest*
		ei mærkelig mod Øst
» under Sydpolen ÷ østlig		*Frastødning*

Traaden horizontal i en Plan ⊥ paa Magnetnaalen
ved Enden af Sydpolen ÷ vestlig *opad*
omvendt omvendt

[1] The first four lines and the drawings are found on a separate undated sheet.

[2] Drawings of the »galvanical fork«. W + means that the western vertical brauch receives electricity from + pole etc.; there is nowhere any indication as to whether the bend is upwards or downwards. [3] A separate sheet, undated.

[4] The bend is upwards, in all other cases downwards.

[5] The position is insufficiently defined in this case and in the following.

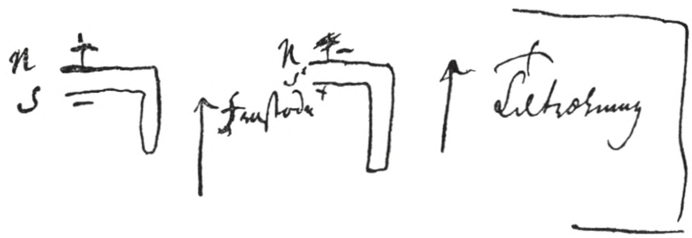
d. 21 Juli[1] **Supplement IV**

Traaden lodret mod Nord | ÷ *oven* |

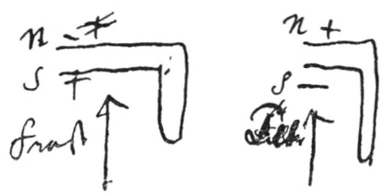
Paa begge Sider | *Foran Polen østlig Afvigelse*
Bagved Polen vestlig

Ved Sydpolen | + *oven* |

Paa begge Sider | *Foran Polen vestlig Afvigelse*
Bagved — østlig Afvigelse



Østen for Naalen



Negativ i Vest Opløftning.

On a sheet of paper dated July 19th 1820 (supplement V) a further series of experimental results is put down, of which some are mentioned in the programme, others not. The former are investigations with a negative result of the effect of the current on needles of wax, glass and brass, besides the observation that the electromagnetic action takes place unimpeded through screens of wood, glass, an electrophorus plate, porphyry, earthenware vessels with water etc. Among the experiments not mentioned in the programme one is of special interest. It is the observation that a magnet which is held in position by another magnet will nevertheless be moved out of its position by the current; it is perhaps the starting point of a later proposal by *Ørsted* to use a controlling magnet with the multiplier.

Further, some experiments are mentioned where the acting current is closed by a magnet laid over the glass plate of a compass so that the needle in the latter is affected both by the magnet and the current. The position of the magnet is not always so clearly defined that we can judge of the result:

[1] This date and the notes below are found on a small loose sheet. Since the results mentioned are given in the communication of July 21. 1820, the date probably only means that the sheet is to be considered as a draft for part of this communication.

Supplement V

D. 19de Juli 1820.

Ophængte Lak, Glas og Messingtraader afficeredes ikke.

En Magnetnaal der tillige fastholdtes i sin Stilling af en anden Magnet frastødtes dog.

Traaden holdt i samme Flade, hvori Magnetnaalen vandrer, gjør ingen Forandring.

Virkede gjennem Jern næsten umærkeligt.

Befugtet Papir ingen bestemt Virkning.

En Magnetnaal lagt ╪ over Pladen tiltrak Naalen. Den ovenoverliggende Magnetnaals Sydpol berørtes med den negative Leder, Nordpolen med den + Leder: Magnetnaalen, der var ophængt, gik med Syd mod Vest, omvendt omvendt.

Den overliggende Naal havde Nord ⊥ paa den underliggende. Sydenden af den overliggende berørtes med ÷ Leder: den ophængte Naal frastødtes mod Vest. ÷ paa Nord frastødtes mod Øst.

Magnetnaalen lagdes Vesten, ⊥ Nord mod den ophængte.

Nordenden berørtes med + Magnetnaalens Nordpol gik mod Vest.

Virkede gjennem 2 Tommer Træe, ligeledes 3 Tom. virkede gjennem Electrophorpladen, gjennem en Porphyrplade gjennem Vand og en Fajence Talerken.

The notes here quoted, and especially those dated July 15th, are so complete that it was only natural that *Ørsted* should consider the discovery ripe for publication, hence on the 21st of July it was communicated under the title »Experimenta circa effectum conflictus electrici in acum magneticam«.[1] This brief description in Latin was sent on the same day to learned bodies and scholars in all European countries. *Ørsted* saw the significance of his discovery.

As this discovery was of such a remarkable kind the communication first enumerates the distinguished men who witnessed the experiments, next follows a description of the battery employed.

In the preceding pages we have explained how *Ørsted* came to use a battery of such large dimensions, and why he considered it necessary that it should be able to make a metal wire glow.

In order that the communication may be brief he states that he will »pass over all those things which have led him to find out the facts of the matter«, and only mention those things which clearly prove them. Through *Ørsted's* notes it has been shown in the preceding pages what course he took to find out »the facts of the matter«.

The chief contents of the communication are as follows: The electric current — in *Ørsted's* language »the electrical conflict« or »interbattle« in the conductor — acts on a magnetic needle; the direction of the force is a lateral action from out the con-

[1] Ed. Vol. II. P. 214.

ductor, and it has been found in, one might almost say, every possible position of the conductor in relation to the magnet.

A simple rule has been given for finding the direction of the force.

It has been proved that the action of the force does not depend on the intermediate substance between the magnet and the conductor.

The magnitude of the force is found to depend on the distance from the magnet, the power of the battery, and the quality of the connecting conductor.

It has further been perceived that a planary current polygon attracts or repels the pole of a magnet.

Finally, quite briefly, attention has been called to the fact that the »electrical conflict« judging from these experiments is not restricted to the conductor but is communicated to the surrounding space and must be assumed to traverse circles whose planes are at right angles to the conductor.

Ørsted then refers to the opinions formerly advanced by him about light and heat being the results of an electric conflict, and points out how the conception of a circular movement in the medium surrounding the conflict will be of significance for the theory of the nature of light.

After these introductory remarks we shall now proceed to quote the communication in full after an English translation from 1820.[1]

Experiments on the Effect of a Current of Electricity on the Magnetic Needle.[2] *By John Christian Oersted, Knight of the Order of Danneborg, Professor of Natural Philosophy, and Secretary to the Royal Society of Copenhagen.*

»The first experiments respecting the subject which I mean at »present to explain, were made by me last winter, while lecturing »on electricity, galvanism, and magnetism, in the University. It »seemed demonstrated by these experiments that the magnetic »needle was moved from its position by the galvanic apparatus, »but that the galvanic circle must be complete, and not open,

[1] *Thomson's* Annals of Phil. XVI. P. 273—76. London 1820.

[2] »Translated from a printed account drawn up in Latin by the author, and transmitted by him to the Editor of the Annals of Philosophy.«

»which last method was tried in vain some years ago by very celebrated philosophers. But as these experiments were made with »a feeble apparatus, and were not, therefore, sufficiently conclusive, »considering the importance of the subject, I associated myself »with my friend *Esmarck* to repeat and extend them by means of »a very powerful galvanic battery, provided by us in common. Mr. »*Wleugel*, a Knight of the Order of Danneborg, and at the head of »the Pilots, was present at, and assisted in, the experiments. There »were present likewise Mr. *Hauch*, a man very well skilled in the »Natural Sciences, Mr. *Reinhardt*, Professor of Natural History, »Mr. *Jacobsen*,[1] Professor of Medicine, and that very skilful chemist, »Mr. *Zeise*, Doctor of Philosophy. I had often made experiments »by myself; but every fact which I had observed was repeated in »the presence of these gentlemen.

»The galvanic apparatus which we employed consists of 20 copper troughs, the length and height of each of which was 12 inches; »but the breadth scarcely exceeded $2\,{}^1/_2$ inches. Every trough is »supplied with two plates of copper, so bent that they could carry »a copper rod, which supports the zinc plate in the water of the »next trough. The water of the troughs contained ${}^1/_{60}$th of its weight »of sulphuric acid, and an equal quantity of nitric acid. The portion »of each zinc plate sunk in the water is a square whose side is about »10 inches in length. A smaller apparatus will answer provided it »be strong enough to heat a metallic wire red hot.

»The opposite ends of the galvanic battery were joined by a metallic wire, which, for shortness sake, we shall call the uniting »conductor, or the uniting wire. To the effect which takes »place in this conductor and in the surrounding space, we shall give »the name of the conflict of electricity.

»Let the straight part of this wire be placed horizontally above »the magnetic needle, properly suspended, and parallel to it. If »necessary, the uniting wire is bent so as to assume a proper position for the experiment. Things being in this state, the needle will »be moved, and the end of it next the negative side of the battery »will go westward.

»If the distance of the uniting wire does not exceed threequarters »of an inch from the needle, the declination of the needle makes »an angle of about 45°. If the distance is increased, the angle di-

[1] [ɔ: Jacobson].

»minishes proportionally. The declination likewise varies with the »power of the battery.

»The uniting wire may change its place, either towards the east »or west, provided it continue parallel to the needle, without any »other change of the effect than in respect to its quantity. Hence »the effect cannot be ascribed to attraction; for the same pole of »the magnetic needle, which approaches the uniting wire, while »placed on its east side, ought to recede from it when on the west »side, if these declinations depended on attractions and repulsions. »The uniting conductor may consist of several wires, or metallic »ribbons, connected together. The nature of the metal does not »alter the effect, but merely the quantity. Wires of platinum, gold, »silver, brass, iron, ribbons of lead and tin, a mass of mercury, »were employed with equal success. The conductor does not lose »its effect, though interrupted by water, unless the interruption »amounts to several inches in length.

»The effect of the uniting wire passes to the needle through »glass, metals, wood, water, resin, stoneware, stones; for it is not »taken away by interposing plates of glass, metal or wood. Even »glass, metal, and wood, interposed at once, do not destroy, and »indeed scarcely diminish the effect. The disc of the electrophorus, »plates of porphyry, a stone-ware vessel, even filled with water, »were interposed with the same result. We found the effects un»changed when the needle was included in a brass box filled with »water. It is needless to observe that the transmission of effects »through all these matters has never before been observed in elec»tricity and galvanism. The effects, therefore, which take place in »the conflict of electricity are very different from the effects of »either of the electricities.

»If the uniting wire be placed in a horizontal plane under the »magnetic needle, all the effects are the same as when it is above »the needle, only they are in an opposite direction; for the pole of »the magnetic needle next the negative end of the battery declines »to the east.

»That these facts may be the more easily retained, we may use »this formula — the pole above which the negative electricity »enters is turned to the west; under which, to the east.

»If the uniting wire is so turned in a horizontal plane as to form »a gradually increasing angle with the magnetic meridian, the de-

»clination of the needle increases, if the motion of the wire »is towards the place of the disturbed needle; but it diminishes »if the wire moves further from that place.

»When the uniting wire is situated in the same horizontal plane »in which the needle moves by means of the counterpoise, and »parallel to it, no declination is produced either to the east or »west; but an inclination takes place, so that the pole, next which »the negative electricity enters the wire, is depressed when the »wire is situated on the west side, and elevated when situated »on the east side.

»If the uniting wire be placed perpendicularly to the plane of »the magnetic meridian, whether above or below it, the needle »remains at rest, unless it be very near the pole; in that case the »pole is elevated when the entrance is from the west side of the »wire, and depressed, when from the east side.

»When the uniting wire is placed perpendicularly opposite to »the pole of the magnetic needle, and the upper extremity of the »wire receives the negative electricity, the pole is moved towards »the east; but when the wire is opposite to a point between the »pole and the middle of the needle, the pole is moved towards the »west. When the upper end of the wire receives positive electri»city, the phenomena are reversed.

»If the uniting wire is bent so as to form two legs parallel to »each other, it repels or attracts the magnetic poles according to »the different conditions of the case. Suppose the wire placed op»posite to either pole of the needle, so that the plane of the parallel »legs is perpendicular to the magnetic meridian, and let the eastern »leg be united with the negative end, the western leg with the po»sitive end of the battery: in that case the nearest pole will be re»pelled either to the east or west, according to the position of the »plane of the legs. The eastmost leg being united with the positive, »and the westmost with the negative side of the battery, the nearest »pole will be attracted. When the plane of the legs is placed per»pendicular to the place between the pole and the middle of the »needle, the same effects recur, but reversed.

»A brass needle, suspended like a magnetic needle, is not moved »by the effect of the uniting wire. Likewise needles of glass and »of gum lac remain unacted on.

»We may now make a few observations towards explaining »these phenomena.

»The electric conflict acts only on the magnetic particles of »matter. All non-magnetic bodies appear penetrable by the elec- »tric conflict, while magnetic bodies, or rather their magnetic par- »ticles, resist the passage of this conflict. Hence they can be moved »by the impetus of the contending powers.

»It is sufficiently evident from the preceding facts that the elec- »tric conflict is not confined to the conductor, but dispersed pretty »widely in the circumjacent space.

»From the preceding facts we may likewise collect that this »conflict performs circles; for without this condition, it seems im- »possible that the one part of the uniting wire, when placed below »the magnetic pole, should drive it towards the east, and when »placed above it towards the west; for it is the nature of a circle »that the motions in opposite parts should have an opposite direction. »Besides, a motion in circles, joined with a progressive motion, »according to the length of the conductor, ought to form a con- »choidal or spiral line; but this, unless I am mistaken, contributes »nothing to explain the phenomena hitherto observed.

»All the effects on the north pole above-mentioned are easily »understood by supposing that negative electricity moves in a spiral »line bent towards the right, and propels the north pole, but does »not act on the south pole. The effects on the south pole are ex- »plained in a similar manner, if we ascribe to positive electricity a »contrary motion and power of acting on the south pole, but not »upon the north. The agreement of this law with nature will be »better seen by a repetition of the experiments than by a long ex- »planation. The mode of judging of the experiments will be much »facilitated if the course of the electricities in the uniting wire be »pointed out by marks or figures.

»I shall merely add to the above that I have demonstrated in a »book published five years ago that heat and light consist of the »conflict of the electricities. From the observations now stated, »we may conclude that a circular motion likewise occurs in these »effects. This I think will contribute very much to illustrate the »phenomena to which the appellation of polarization of light has »been given.

Copenhagen, July 21, 1820.

JOHN CHRISTIAN OERSTED.

In the days following the publication the experiments were evidently continued. This appears from some sheets among *Ørsted's* papers marked EM. The most important are marked EM A and EM B and contain a series of experimental results numbered in succession 1—13. These refer in the first place to a series of experiments of a kind similar to those carried out in the preceding days. Their object is to show in additional ways that the electromagnetical action is independent of the interposition of other bodies between the wire transmitting the current and the poles; f. inst. it is shown that a mirror has no effect. Further it was examined whether there was any lateral electric action on a frog and on a gold leaf electroscope furnished with a point, and finally a fresh set of experimental arrangements of conducting wires and poles relative to each other serves as a further corroboration that the laws already found for the direction of the force hold good in all cases.

Finally there is the statement of a significant new fact: *Ørsted* has seen[1] that a single cell was sufficient to produce »a very marked effect« on the magnetic needle. He further shows[2] that this effect is found even if the conductor consists of potash, water or dilute sulphuric acid when this forms a broad layer, but not if the current is transmitted through water with wires as electrodes. EM A and EM B are to be seen in Supplement VI.

Supplement VI *A* *E. M.*

1) *Magnetnaalen og Lederen kunne befinde sig i samme Vandmasse, uden at Virkningen derfor ophører.*
2) *En Frøe i et Cylinderglas med Vand syntes ikke at føle Indtryk af Lederens Nærhed.*
3) *Guldbladelectrometret forandredes ikke ved at nærmes Lederen, eller berøre den.*
4) *Virkningen tabte sig ikke ved at gaae gjennem et Speil.*
5) *Luftens Fortyndning eller Fortætning i en Glasklokke, synes ei at virke paa en nær Magnetnaal.*
6) *Et Glasrør med to Messingtraade og Potaskeopløsning blev brugt som en Deel af Lederen mellem Z og K i en enkelt Kasse. Om man end gav Traadenes Spidser en nok saa ringe Afstand, naar de kun ei berørte hinanden, skede ingen Virkning paa Magnetnaalen. Dette Forsøg maa dog gjentages med bevægeligere Naal.*
7)[3] *En umagnetisk Naal opstilledes paa en Spids, hvorom den kunde dreie sig. Paa den ene Ende befæstedes lodret en Staaltraad, omtrent $1^1/_2$ Tom. lang, saaledes at*

[1] Experiment 12 under EM B. [2] EM B, 13.

[3] Drawings and notes to experiments 7, 8, 9, 10 are found on an enclosed sheet and are here reproduced on p. XCV; a somewhat improved arrangement of this position of the magnet is mentioned in *Ørsted's* paper in *Schweigger's* Journal für Chemie u. Physik, Vol. 33. (Ed. Vol. II. P. 228) and is recommended as particularly convenient to demonstrate the electromagnetical effect.

Nordenden var neden og Sydenden oven for Naalen. Den gaffelformige Bøining af Lederen virkede paa Polerne saaledes som man umiddelbart kunde forudsee det, af dens Virkning paa den vanlige Magnetnaal. Supplement VI

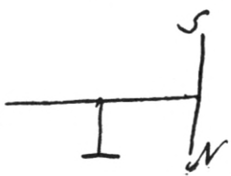
8) *Naar den nærmeste Deel af Lederen stilledes parallel med Magnettraaden, frembragtes ingen Bevægelse, som ligeledes kunde ventes, da den ene Pol altid vilde drives i den modsatte Retning af den anden, og Magnettraaden ei kunde dreies om sit Midtpunkt.*

9) *Lederen dannede en Vinkel af 45⁰ med Horizonten, i en Plan fra Øst til Vest. Retningen fra det Negative til det Positive gik fra oven til Neden:*

   a) Bag Sydenden af den lodrette Traad gav den Afvigning mod Øst.
   b) Foran samme: Afvigning mod Vest
   c) Bag Nordenden: Afvigning mod Vest
   d) Foran Nordenden: Afvigning mod Øst.

10) *Lederen parallel med den umagnetiske Naal, og beliggende i en horizontal Plan der skar Magnettraaden mellem Sydenden og Ligevægtspunktet. Retningen fra Negativt til Positivt var fra Syd til Nord. Afvigning mod Østen. Kræfternes Retning omvendt: Afvigning mod Vesten.*

[1]



÷ oven
i en Plan fra Ø til V
Lederen skraa
bag ved S mod Ø
for ved S lidt mod V.

+ oven
foran Afv. Ø
bag Afv. V.
— oven — *for N til Øst*
foran N — *bag N til Vest.*
Afv. Ø
bag Afv. V.

Parallel foran SN, ingen Forandring. Ved Siderne ei heller.

Lederen under S.
- *Østen for S ÷ fra Syden + fra N mod Østen*
- *Vesten for, ogsaa mod Østen*
- *÷ fra Nord + fra Syd. Vesten paa begge Sider.*

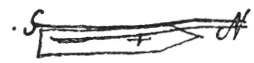
Lederen over S.
- *÷ fra Syd. Afv. mod V.*
- *+ fra Syd. Afv. mod Ø.*

Tiltrækninger og Frastødning af Gaffelen, som ved den horizontale Naal.

[1] Drawings and notes to E. M. A and B 7—11; they are found on a loose sheet enclosed with the sheets marked E. M.

Supplement VI *B.* *E. M.*

11) *Samme Forsøg, kun med den Forskjel at Lederen laae i en horizontal Plan over Magnettraadens Sydende.*



Retningen af Kræfterne S – + → N

Afvigning mod Vest.

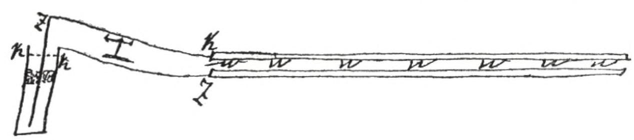
Retningen S + – → N : *Afvigning mod Øst.*

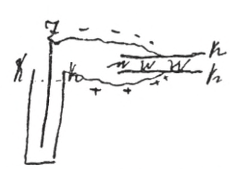
12) *En enkelt Zink- og Kobberplade til 2 □Tom give med Svovelsyre til flydende Leder en meget kjendelig Virkning paa Magnetnaalen.*

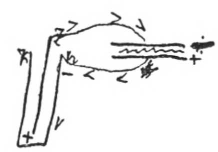
13)[1] *For at prøve om et enkelt Element (Kobberkasse, Zink og syret Vand) kunde give Virkning paa Magnetnaal, naar den faste Leder afbrødes ved en flydende, lagdes et Stykke befugtet Papir mellem to Messingplader af 28 Qvadrattommer, og Zinkpladen forbandtes med den ene, Kobberpladen med den anden. Naar den flydende Leder enten var Potaskevand eller Svovelsyrevand, med yderst lidet Syre, saa viste denne afbrudte Leder endnu Virkning paa Naalen. Lærret i Stedet for Papir gjorde ingen Forandring. Med Vand, som flydende Leder udrettedes intet, i det mindste intet Kjendeligt. To Metaltraade (i Stedet for Pladerne) i et Glasrør fyldt med Potaskeopløsning gave ei heller Virkning.*

---

[1] On two loose sheets are found the following drawings relating to this experiment.





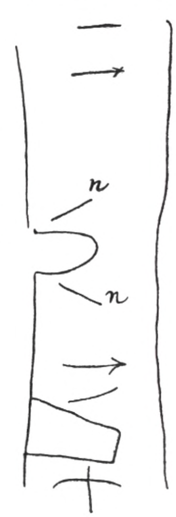


On the 3rd and 4th pages of the sheet marked E. M. B. is written in pencil:

En bred Leder

1) *Magnetnaalen derpaa*
2) *Derudenfor*
3) *Inclination.*

further some rough drafts of a broad conductor; the best of these drafts is reproduced here. These experiments are mentioned in a treatise by *Ørsted* in *Schweigger's* Journal. Vol. 32. 1821, (Ed. Vol. II. P. 231.) and are there used to refute one of *Berzelius'* theories on the cause of the electric action.



The experiments with the single cell were extended and the result was published soon after under the title »Neuere elektromagnetische Versuche«[1] in the July number of *Schweigger's* »Journal f. Chemie u. Physik«, the same number in which the first communication is printed, only some pages further on. This work, then, was evidently finished shortly after the first, and should be regarded as coherent with it as it forms an important supplement to it; as a rule, however, it has been overlooked and forgotten, its contents have even been ascribed to *Schweigger* by people who might otherwise be considered experts. It contains the following results: —

1) The effect of a conducting wire on the pole of a magnet depends on the quantity of electricity and not on its tension; all the effects of the experiments mentioned in the first communication are therefore made more powerful by using a large cell in stead of a battery of smaller cells, and also more powerful by using a cell with large plates in stead of a cell with small ones.

2) The reaction effect is found by showing that a suspended closed circuit is turned by a magnet.

3) It is established in a fresh way that a closed circuit has a north end and a south end just like a magnet.

As the treatise is so often overlooked it is reprinted here[2] in connection with the first communication in order to afford a complete view of *Ørsted's* first experimental electromagnetical papers.

New Electromagnetic Experiments. By Prof. Oersted.

»Since the publication of my first experiments on the magnetic »action of the galvanic battery, I have multiplied my researches »on that subject as much as a multitude of other important avoca»tions put it in my power.

»The magnetic effects do not seem to depend upon the intensity »of the electricity, but solely on its quantity. The discharge of a »strong electric battery transmitted through a metallic wire pro»duces no alteration in the position of the magnetic needle. A se-

---

[1] Ed. Vol II. P. 219.

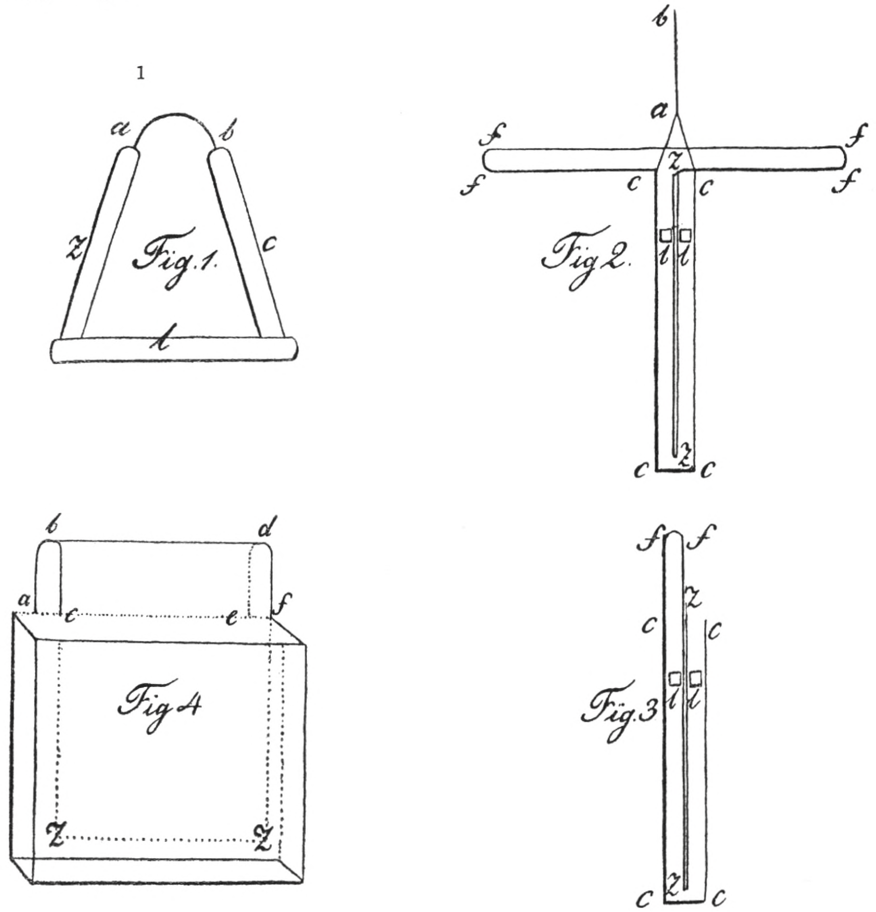
[2] After an English translation in *Thomson's* Annals of Philosophy XVI. London 1820. P. 375—377.

»ries of interrupted electric sparks acts upon the needle by the »ordinary electric attractions and repulsions, but as far as can be »perceived, the sparks produce no electromagnetic effect. A gal»vanic pile composed of 100 discs of two inches square each metal, »and of paper moistened with salt water to serve as a fluid con»ductor, is likewise destitute of sensible effect upon the needle. On »the other hand we obtain the effect by a single galvanic arc of »zinc and copper having for a conductor a liquid possessed of »great conducting power; for example, of one part sulphuric acid, »as much of nitric acid, and 60 parts of water. We may even »double the quantity of water without much diminishing the effect. »If the surface of the two metals is small, the effect is likewise small. »But it augments in proportion as we augment the surfaces. A »plate of zinc, of six inches square, plunged into a vessel of copper »containing the liquid conductor of which I have spoken, produces »a considerable effect. But an arrangement of this kind in which »the zinc plate has a surface of 100 inches square acts upon the »needle with such force that the effect is very sensible at the dis»tance of three feet, even when the needle is not very moveable. »I have not observed greater effects from a galvanic apparatus com»posed of 40 similar troughs; indeed the effect seemed less great. »If this observation, which I have not investigated expressly, is just, »I shall be of opinion that the small diminution of the conducting »power produced by increasing the number of the elements of the »apparatus weakens its electrochemical effect.

»To compare the effect of a single galvanic arc with that of an »apparatus composed of several arcs or elements, let us make an »observation. Let fig. 1, represent a galvanic arc composed of a »piece of zinc *z*, of copper *c*, of a metallic wire *a b*, and of a liquid »conductor *l*. The zinc always communicates a portion of its posi»tive electricity to the water as the copper does of its negative elec»tricity. This would occasion an accumulation of negative electri»city in the upper part of the zinc, and of positive electricity in the »upper part of the copper, unless the communication *ab* re-estab»lished the equilibrium by affording a free passage for the negative »electricity from *z* to *c*, and for the positive electricity from *c* to *z*. »We see then that the wire *ab* receives the negative electricity of »the zinc, and the positive electricity of the copper, while a wire »that constitutes the communication of the two poles of a pile, or

»of another compound galvanic apparatus, receives the positive »electricity of the zinc pole, and the negative electricity of the »copper pole.

[1]



»By attending to this distinction, we may, with a single galvanic »arc, repeat all the experiments which I had at first made with a »compound galvanic apparatus. Employing a single galvanic arc »gives this great advantage, that it enables us to repeat the experi»ments with little preparation and expense. But it presents another »advantage still more considerable; namely, that we may establish »a galvanic arc sufficiently powerful for the electromagnetic expe»riments, and yet sufficiently light to be suspended to a small me»tallic wire, in such a manner that the small apparatus may be »made to turn round the prolonged axis of the wire. We may in »this way examine the action which a magnet exerts on the galvanic

[1] These diagrams are somewhat larger than in the English treatise; they are reproduced after *Schweigger's* Journ. f. Ch. und Physik (Ed. Vol. II. P. 219).

»arc. As a body cannot put another in motion without being moved »in its turn, when it possesses the requisite mobility, it is easy to »foresee that the galvanic arc must be moved by the magnet.

»I made use of different arrangements of the simple galvanic »apparatus to examine the motion impressed on it by the magnet. »One of these arrangements is represented in fig. 2, which repre»sents a perpendicular section of it in the direction of the breadth. »*cccc* is a trough of copper, three inches high, four inches long, and »half an inch broad. These dimensions doubtless may be varied »to infinity. It is only necessary to observe that the breadth ought »not to be great, and thus the trough should be made of plates as »thin as possible. *zz* is a plate of zinc. *ll* are two pieces of cork »which keep the plate in its position. *cffffz* is a brass wire, of a »quarter of a line at least in diameter. *ab* is a brass wire as fine »as possible, so as to be able to bear the weight of the apparatus. »*cac* is a linen thread uniting the wire to the apparatus. The »trough contains the liquid conductor. The conducting wire of »this apparatus will attract the north pole of the needle when it is »placed on the left side of the plane *cffffz*, considered in the di»rection *fz*. On the same side the south pole will be repelled. On »the other side of this plane, the north pole will be repelled, and »the south pole attracted. That this effect may take place, we must »not place the needle above *ff*, nor below *fz* or *fc*. If instead »of presenting a small moveable needle to the conducting wire we »present near one of the extremities *ff* one of the poles of an ener»getic magnet, the attraction or repulsion indicated by the needle »will put the galvanic apparatus in motion, and will turn it round »the prolonged axis of *ab*.

»If instead of the conducting wire we take a large ribbon of »copper of the same breadth as the plate of zinc, the effect differs »from that which we have just mentioned only in being much »feebler. On the other side we increase the effect a little by making »the conductor very short. Fig. 3 represents the perpendicular »section of this arrangement in the direction of the breadth of the »trough. Fig. 4 exhibits the same arrangement in perspective. It is »obvious that *acbdef* represents the conducting plate, and *czzf* »the plate of zinc. In this arrangement the north pole of the needle »will be attracted towards the plane of *abc*, and the south pole will »be repelled from the same plane. *edf* will have contrary effects.

»Here we have an apparatus whose extremities act like the poles »of the needle. But it must be acknowledged that only the faces »of the two extremities, and not the intermediate parts, have this »analogy.

»We may likewise make a moveable galvanic apparatus of two »plates, one of copper and one of zinc, twisted into a spiral, and »suspended in the fluid conductor. This apparatus is more move- »able; but more precautions are necessary not to be deceived when »we make experiments with it.

»I have not yet found a method of making a galvanic apparatus »capable of directing itself towards the poles of the earth. For »this object it would be necessary to possess apparatus much »more moveable.«

It appears from these two papers from 1820 that *Ørsted* has not confined himself, as it has often been represented later on, to show that there is a force which acts between an electric current and a magnet, but he has furnished the whole foundation for the knowledge of the action of this force through systematic experimental research.

From the moment that *Ørsted's* discovery became known it created an enormous sensation. The results communicated were so astounding that they were received with a certain distrust, but they were stated with such accuracy that it could hardly be permitted to entertain any doubts. The treatise itself was thus a strong inducement to put the results to a test; as *Ørsted* had given no real description of his way of proceeding, each scientist made the experiments in his own way, and many of those who repeated them were thus led to consider themselves and others *Ørsted's* equals in this field of research, and published their work as new-discoveries even if their results were in reality to be found in *Ørsted's* communication. The language of the latter also impeded the understanding and was responsible for the defective comprehension of its contents. Even in 1820 scholars were no longer very intimately acquainted with Latin, and in the course of a short time the treatise was translated into all the chief languages; into Danish by *Dyssel* in 1820.[1] Two important new-discoveries followed quickly after

[1] In *Rahbek's* Hesperus, 4th Number, 1820, p. 312—27. The translation also comprises »Neuere elektromagnetische Untersuchungen«. Ed. Vol. III.

the discovery had become known in France. On the 18th of Sept. *Arago* made the communication in the French Academy that an electric current can magnetise iron, and on the 25th of Sept. *Ampère* was able to communicate in the same place that parallel electric currents act on each other.

All were soon imbued with the excessive importance of the discovery and the reviews were filled with electromagnetical treatises. In 1821 *Schweigger* began a new series of his »Journal für Physik und Chemie« on the ground that *Ørsted's* experiments were the most interesting which had been made in the domain of magnetism for the last century, and that a new era would open with them.[1] *Ampère* saw the matter in the same light[2] »M. *Ørsted* . . . . a pour jamais attaché son nom à une nouvelle époque. . . . . Ce savant professeur danois a ouvert, par cette grande découverte, une nouvelle carrière aux recherches des physiciens.« In 1821 the German physicist *Erman* wrote in a work on *Ørsted's* magnetism:[3] »Von der überschwenglichen Wichtigkeit der *Örsted*schen Entdeckung kein Wort: diese Sache oder keine in der Welt spricht von selbst.«

In England, too, the discovery received full credit. In 1821 *Faraday* wrote a historical survey of the evolution of electromagnetism up to April 1821.[4] Though only 8 months had passed since *Ørsted's* communication, so much had already been written that *Faraday* found it very difficult to make head or tail of the many works, of what had been done and by whom »in consequence of their great variety, the number of theories advanced in them, their confused dates, and other circumstances«.[5] He then undertook to go through systematically »with great labour and fatigue« everything that had appeared in journals and other places. He gives full credit to *Ørsted's* work; about the first communication he writes: »It is full of important matter, and contains, in few words, the results of a great number of observations; and with his second paper,

[1] *Schweiggers* Jahrbuch der Chemie und Physik Bd. 16. S. 13. Halle 1826. *Schweigger* »Ueber Elektromagnetismus«: »So vorbereitet übrigens die Entdeckung des Elektromagnetismus war: so musz man doch gestehen, dasz, als er wirklich durch *Oersted's* glänzende Entdeckung, zur Erscheinung und in unsere Gewalt kam, man sich hierdurch in eine ganz neue, bisher in solcher Art von niemanden auch nur geahnete, Welt versetzt fühlte. Es schien mir daher schicklich, in dem Jahrbuche für Chemie und Physik einen Hauptabschnitt zu beginnen« . . . .

[2] Journal de phys. Vol. 94. P. 61. Paris 1822.

[3] Umrisse zu den physischen Verhältnissen des von Herrn Professor *Örsted* entdeckten elektro-chemischen Magnetismus. Skizzirt von *P. Erman*. Berlin 1821. P. 1.

[4] *Thomson's* Annals of Philosophy. New series. London 1821. Vol. II. P. 195 & 274. Vol. III. P. 107. [5] l. c. P. 195.

comprises a very large part of the facts that are as yet known relating to the subject.«[1] He acknowledges *Ørsted's* discovery to be not only a lucky chance but the fruit of a deliberate search: »Mr. *Ørsted* . . . has, for many years, been engaged in inquiries respecting the identity of chemical, electrical, and magnetic forces; . . . his constancy in the pursuit of his subject, both by reasoning and experiment, was well rewarded in the winter of 1819 by the discovery of a fact of which not a single person beside himself had the slightest suspicion; but which, when once known, instantly drew the attention of all those who were at all able to appreciate its importance and value.«[2]

In spite of this unanimous appreciation of the significance of the discovery, *Ørsted's* merits in the matter and the value of his work gradually became obscured. The point of view which was little by little generally adopted was this: *Ørsted* had by chance discovered the fact that an electric current may deflect a magnetic needle, but all the closer investigation of the matter had been made by others. *Faraday's* perception of the connection of the discovery with *Ørsted's* earlier views, and his recognition of the weighty contents of the papers from 1820 was only shared by few, or was at any rate forgotten.

The reason why this point of view gained ground must be sought in various causes. Of no slight significance was the fact that this opinion was held by a man who had the means and the power to communicate it to others, viz. *Gilbert*, the editor of »Annalen der Physik«. *Gilbert* considered it the principal mission of his life to give his wide circle of readers an idea of the progress of science by means of his »Freie Bearbeitungen« of new scientific papers published in his periodical. To judge from his reports of electromagnetical researches he was not equal to this task; but although some of his contemporaries looked upon his activities in this domain with criticism,[3] he certainly exerted considerable influence in this way.

Gilbert translated *Ørsted's* communication and published it in vol. 66 of his review[4] in the October number of 1820, together with an account of experiments made in order to confirm its results, all accompanied by critical or explanatory comments.

[1] l. c. P. 196. [2] l. c. P. 195.

[3] *Hansteen* in a letter to *Ørsted* in 1819 complains that his only scientific nourishment is the »Zerrbilder« which »*Gilbertchen*« compiles under the title of »Freie Bearbeitungen«.

[4] *Gilbert's* Annalen der Physik. Bd. 66. Berlin 1820. P. 291.

He declared the discovery to be a mere accident: »Was alles Forschen und Bemühen nicht hatte geben wollen, das brachte ein Zufall Hrn. Professor *Oersted* in Kopenhagen.« — He complained that the communication was written in Latin, and in many places found it so obscure that misapprehension could hardly be avoided; his remarks show, however, that this is not due to *Ørsted's* language, but to *Gilbert's* deficient conception. He has f. inst. misunderstood the experiments by *Ørsted* which produce inclination,[1] and does not understand the use of the galvanical fork, though the description is clear enough.

From the beginning he met *Ørsted* with distrust, but had more confidence in his friends: »Von Hrn. *Oersted's* Versuchen wuszte ich anfangs nur von hören sagen. So bald mein Misztrauen durch die Ansicht der Ankündigung, durch die Namen *Hauch, Jacobsen* u. a. als Mitarbeiter und Zeugen, und durch die Genfer Versuche entfernt war«[2] . . . he repeated the experiments himself.

He also disparaged the merit of *Ørsted* by assigning to repeaters and witnesses a great deal of the credit for the experiments, and especially by only casually touching upon the experiments in *Ørsted's* second treatise, and in such a way that the honour of priority was not with certainty ascribed to him: »Die merkwürdigen Versuche, auf welche die Entdeckung des Hrn. *Oersted*, ihn und seine Kopenhagnerfreunde, so wie H. H. *Pictet* und *De la Rive* in Genf, und Hrn. *Arago* in Paris geführt hat . . . . vor allen andern ab Hrn. *Ampère* . . . . würden nur wenigen zugänglich seyn, wenn sie in der That so mächtiger galvanisch-electrischer Apparate bedürften, als die Urheber dieser Versuche geglaubt zu haben scheinen. . . . . Zu diesen Versuchen sind sie aber völlig überflüssig, und ein aus einem einzigen Par Electromotore neuerer Einrichtung bestehender Apparat reicht hin«.[3] Here, we see, he does not mention that »der Urheber« himself had discovered this, and yet, 5 pages further on,[4] the observation is made that *Ørsted* had perceived it, which shows that the paper must have been known to *Gilbert*. *Ørsted's*

[1] The French translation in Bibl. universelle des sciences. Genève 1820. P. 274—84, has the same misunderstanding and another in the description of the galvanical battery; in Giornale di fisica, chimica e storia naturale da. *L. Brugnatelli,* III, Pavia 1820. P. 174—78, an Italian translation after the communication in Bibl. univers. has the same mistake.

[2] Annalen d. Phys. Bd. 66. P. 294.

[3] *Gilbert's* Annalen d. Phys. Vol. 66. Berlin 1820. P. 332.

[4] l. c. P. 337.

description of the discovery of the reaction-effect he has not understood.

That *Gilbert's* way of stating the case was not without significant consequences is seen from his successors.

Pfaff, Professor at Kiel published a book in 1824 with the title: »Der Elektromagnetismus, eine historisch kritische Darstellung«, in which he speaks about *Ørsted's* »lucky find«, where he plainly utters a doubt that, as asserted by *Ørsted,* there was a connection between this »find« and *Ørsted's* earlier views. In surveying *Ørsted's* experiments he did not mention that *Ørsted* had discovered the action of a magnet on a circuit, but stated that *Schweigger* was the first who tried to find such an action.

In *Gilbert's* Annalen, Index, 1826, p. 221, under the section Electromagnetism the following reference is found: »Vol. 66. 350 A. *Schweigger* bemerkte zuerst dasz ein einfacher Elektromotor aus Zink und Kupfer stärker wirkt als eine galvanische Batterie . . . .« It is of interest to compare with this a note by *Schweigger* on *Ørsted's* paper No. 2 in his journal, referring to the passage where *Ørsted* states that a single cell may be employed. The paper must have come into *Schweigger's* hands shortly after the first one, as it is found in the same number. The note runs as follows: — »Vergl. die lateinische Abhandl. S. 275 dieses Hefts. Es ist diesz eine der bedeutendsten unter den neuern physikalischen Entdeckungen, die für die Wissenschaft von groszen Folgen seyn wird. Durch gegenwärtige nähere Erläuterungen werden die Leser in den Stand gesetzt, die Versuche auf eine einfache Weise zu wiederholen und sich von der Wichtigkeit der *Oersted*schen Entdeckung selbst zu überzeugen. Seit *Galvani's* erstem Versuch ist vielleicht kein wichtigerer für die Lehre der Elektricität und des Chemismus angestellt worden als der *Oersted*sche.«

It is indeed curious that is has been possible to ascribe to *Schweigger* the discovery that the single cell can be used, in spite of his own words. If we look up the page in vol. 66 of Annalen der Physik referred to in the index, the discovery is not mentioned there at all; the reference is a mistake; it can only be an effect of the atmosphere of the *Gilbert* papers in the same number.

The effect spreads further: *Fechner,* Elementar-Lehrbuch des Elektromagnetismus, Leipzig 1830, only quotes *Ørsted's* Latin paper, does not mention that he has found the reaction effect, and writes: »*Oersted* selbst scheint sich, nachdem er dem ersten Anstosz gegeben, wenig mehr mit dem Elektromagnetismus beschäftigt zu haben«.[1] From a note on the first page it is seen that *Gilbert's* Annalen and its Index are the sources of reference employed, and among the original treatises none by *Ørsted* except No. 1 is mentioned.

In *Ostwald's* Edition of Classics No. 63, Electromagnetism, only *Ørsted's* first paper in *Gilbert's* translation is reprinted; two of *Gilbert's* mistakes have been corrected in the notes, and *Ørsted's* second paper is mentioned but not reprinted, whereas an elaborate paper by *Seebeck* is reprinted which only gives repetitions in a ponderous form of *Ørsted's* experiments.

In 1849 an attempt was made in the German press to rob *Ørsted* of the honour of the discovery of electromagnetism. He was informed of it by a letter[2] from *Reedtz* who was then staying in Berlin. On the 26th of August the latter stated that an article had appeared a fortnight earlier, in »Constitutionelle Zeitung« in which it was asserted that it was *Reedtz* and not *Ørsted* who had accidentally become aware of »the chief phenomenon of electromagnetism«. It was not until some time after the appearance of the article that *Reedtz's* attention was directed to it, but then by several people f. inst. by *A. von Humboldt*. As the statements in the article were definite and detailed, *Humboldt* and *Reedtz* agreed that the latter should publicly refute them. *Reedtz* sent both articles to *Ørsted* expressing the hope that he had not already seen this attack and wondered that *Reedtz* had not replied to it. If the article should have found its way into Danish papers *Reedtz* asks *Ørsted* — not for his own sake, as that is superfluous, but for *Reedtz's* — to insert the reply. The draft for *Ørsted's* answer is written on *Reedtz's* letter. The matter itself evidently made no great impression on *Ørsted*, at any rate his letter contains nothing to indicate it, but only an assurance that he would never dream of thinking *Reedtz* capable of appropriating to himself the honour of the discovery »but I owe you my thanks for having disproved this insidious appropriation and at the same time expressing sentiments so kindly and creditable to me.«

As a contrast to this it may be mentioned that a few years before, on a festive occasion, *Ørsted* had been the object of an ovation in Berlin at which he was called *Columbus-Ørsted*, because like

[1] *Fechner:* Elementar-Lehrbuch des Electromagnetismus. P. 124. Leipzig 1830.

[2] *Ørsted's* unedited letters. B. U. H. Kbhvn.

Columbus he had looked towards his goal for so many years and at last had the good fortune to reach it.[1]

The said attempt to rob *Ørsted* entirely of the honour of the discovery of electromagnetism did not remain the only one.

For the sake of completeness we mention a German pamphlet from 1874, maintaining that *Schweigger*, not *Ørsted*, was the discoverer of electromagnetism. The same assertion has been set forth by *F. Richarz* in 1915[2] with reference to a treatise written by *Schweigger* in 1808[3]; the whole is a mistake; the treatise only deals with a special form of *Coulombs* torsion balance. All that is needed to refute these assertions is to refer to *Schweigger's* own notes on *Ørsted's* papers in 1820.

While, then, the development in Germany tended to depreciate *Ørsted's* merit, in France it was somewhat different, it was not disparaged but forgotten on account of *Ampère's* researches and theories which created the greatest sensation and interest. The cause of this is not difficult to see. *Ørsted* certainly had several ingenious ideas, but as a general rule his theoretical views were neither clearly nor consistently worked out.

Already in the communication of the 21st of July some ideas about the electromagnetical mode of action were briefly set forth, they were simply and plainly formulated. *Ørsted* figures to himself that »the electrical conflict« is not restricted to the conductor, but also takes place in the space outside it, and that the two electricities which in each place are alternately united and separated, in the conductor as well as outside it, move in spiral lines (turned from right to left) whose individual windings are almost circles. The negative electricity is presumed during its flow to act propulsively on the north pole, the positive electricity in the same way on the south pole, the magnetic particles being impenetrable to the conflict, whereas all unmagnetic ones are penetrable; in this way the movement of the magnetic needle becomes a sort of conveying movement. Here, then, for the first time was set forth the idea of a special condition of the medium surrounding the conductor, of a circular field of force around it.

These ideas, comparatively clearly expressed, were not maintained, however, in the succeeding papers, among which there

[1] Letter from *Ørsted* to his wife 1843. B. U. H.

[2] Die Kultur der Gegenwart: Physik. P. 273. Leipzig 1915.

[3] *Schweigger:* »Ueber die Benutzung der magnetischen Kraft bei der Messung der elektrischen«. *Gehlens* Journal für Chemie u. Physik Bd. VII. 1808.

are two of substantially the same contents. One is found in the Videnskabernes Selskabs Oversigter for 1821,[1] the other in *Schweigger's* Jahrbuch 1821.[2] The latter is the most explicit. Two essential changes had taken place in *Ørsted's* notions. He now supposed that both electricities acted on each pole, the negative electricity repelling the north pole, the positive attracting it, and vice versa with the south pole. The idea of a simple conveying movement of flowing electricity at those points of the medium where the poles are, cannot be reconciled with this. Furthermore, among his assumptions he only emphasises »that the course of the electric forces in the conductor is a spiral line«, but not that it is so outside the conductor; in some cases he assumes this, in others not. He imagines that the »electrical forces« leave the conductor in the direction of tangents to the surface of the conductor. Starting from this conception he seeks to explain the mutual action of parallel currents and here includes the presupposition that the »forces« outside the conductor continue in straight lines, that they meet and act on one another like opposite electricities.

So *Ørsted's* theories had the same fate as hitherto, they were too vague to bear mathematical treatment, hence the value of the first ideas was not perceived. His theoretical writings even conduced to divert the attention from the really valuable work given by him in the two experimental treatises, even if nobody could rob him of the honour of having first seen the magnet move under the influence of the electric current.

Whenever he returned to his theory later, in lectures and smaller treatises, he made use of it in its first, simple form, where the medium surrounding a conductor is supposed to be polarised, thus it was this form he made use of in his argument in a lecture he delivered at a Meeting of Scientists at Berlin in Sept. 1828 where he spoke »ganz frei eine halbe Stunde lang«, and took a survey of his theory and its significance; the same was the case where he explained the relation of the theory to the induced currents,[3] and it is found again in a manuscript for lectures on electromagnetism in 1845, but the treatise in *Schweigger's* Journal for 1821 has caused the fundamental notion to become obscured, the notion of the circular field of force surrounding a conductor, produced by the po-

[1] Ed. Vol. II. P. 447. [2] Ed. Vol. II. P. 223. [3] Ed. Vol. II. P. 484.

larisation of the medium, the precursor of *Faraday's* ideas of a similar kind.

After the experimental work on electromagnetism in 1820, and the theoretical account of it in the spring of 1821, *Ørsted* in all essentials stopped his researches on this subject and turned his attention to other important domains. »In the immediately succeeding years he did indeed continue his electromagnetic experiments, but these rather led to a further corroboration of certain ideas than they made any fresh addition to science«, he writes in 1828.[1] Only occasionally when the works of others attracted his attention or the theories of others on electromagnetism seemed absurd to him, did he revert to the subject. »Yet he found when using the electromagnetic multiplier, discovered by *Schweigger*, that when two bodies are immersed at different times in a fluid, an electric current is set up« he writes in continuation of the passage quoted above. This enquiry[2] which he thus — curiously enough — calls attention to after the lapse of seven years, was carried out in 1821, and was induced by similar observations made by others; it is more the outcome of his interest in the causes of the production of a current than in electromagnetism, but during these years *Ørsted* was inclined to class an experiment as electromagnetic if it only employed the magnetic action of the current.

A small paper[3] in the form of a letter to *Schweigger* dated 9/9 1821 was likewise caused by the works of others. It is partly of a theoretical nature and defends and explains his spiral theory against scepticism and misapprehension. The paper also contains the description of an experiment, occasioned by an account by *Yelin,* according to which a conductor returning upon itself was said to behave like a piece of unmagnetic iron to a magnet. *Ørsted* now carried something of the same sort into effect by suspending an elliptic circuit perpendicularly and in such a position that it was able to revolve round the minor axis. When he lets a current enter and pass out at the extremeties of the axis, the two halves of the conductor will be traversed by currents in the same direction, i. e. get opposite poles, so that f. inst. the two sides of the plane may very well be attracted or repelled by the same magnetic pole. He adds after the description: »Man darf wohl àber hoffen, dasz Hr. *von Yelin,* wenn er sich nicht getäuscht hat, uns die Bedingungen

[1] Autobiogr. P. 538. [2] Ed. Vol. II. P. 251. [3] Ed. Vol. II. P. 246.

des Versuchs näher angeben wird«.[1] He evidently distrusted *Yelin's* powers of observation and experimental skill.[2]

Ørsted, however, formed schemes in 1821, for dealing with series of subjects of partly electromagnetic nature and with more comprehensive results in view, mainly with the intention of showing a connection between different forces of nature, but the schemes were not realised. Among his papers we find two sheets — reproduced below in Supplement VII — dated the 1st of April and the 6th of May 1821 which show that the plans have

At forsøge
optegnet 1ste Apr. 1821

Supplement VII 1) *At smelte ved Galv. Jerntraad, indsluttede i Harpix, i Therpentinolie, i Brindluft. Man eftersee om de derved vorde saa skjøre, som naar de smeltes ved samme Middel i Luften.*

2) *At give Metaltraade, især Jerntraade en glødende Udladning, saaledes at Qviksølv udgjør en Deel af Buen, og virker i denne Tilstand paa Jernet.*

3) *Kunde man ei frembringe en Efterligning af Polarlyset, ved en magnetisk Condensator af stor Virksomhed? — Vort Apparat tykke Ledningstraade.*

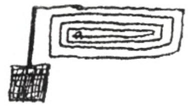
4) *At prøve hvorvidt man kan magnetisere med Condensatoren. Skulde man ikke kunde anvende denne Magnetisering til sand Forhøielse af vore Magnetkræfter.*

5) *Kan ei en Metaltraad, ved at behandles som en Jerntraad man galvanisk magnetiserer, faae den Egenskab at tiltrække Jernfiilspaan, saa længe den galvaniske Indvirkning varer?*

6) *Have de til Kugler halvhensmeltede Jerntraade ingen magnetisk, eller galvanomagnetisk Virkning?*

7) *Ere Tonesvingningerne ikke ledsagede med galvanomagnetiske Virkninger?*

8) *Man burde danne en magnetisk Condensator paa følgende Maade:*
Man drage paa fiint Papir med Gummivand følgende Figur



dække denne med eet Guldblad, og lade det tørre. Man kunde siden afgnide det løse Guld, og man havde da en Condensator af Guldblad. Fra a maatte gaae helst bag ved Papiret en Tinstribe, som forbandt en meget fiin Zinkplade med a. Papir og Fugtighed der imellem vilde give en saare letbevægelig Condensator. Det forstaaer sig at den maa hænge i Silkeormespind. — Sølvblade vilde være endnu bedre. — Mon Blyantstræk vilde være brugbare? Maaske bedre en Vædske af Blyant og Gummivand.

9) *Metallernes Brintning, og maaske Adskillelse burde forsøges med den størst mulige Kraft.*

10) *En staaende Leder maa paa alle Kanter tiltrække Jernfiilspaan med lige Lethed.*

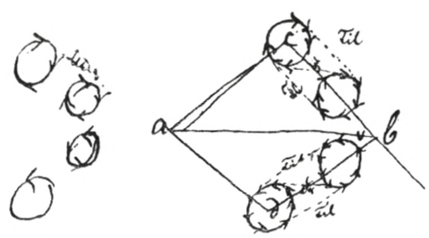
11) *Har en Jerntraad i Smeltningsøjeblikket ingen sær Virkning paa Magnetnaalen?*

[1] Ed. Vol. II. P. 250. [2] P. CXIII.

E M

til at prøve (skrevet d. 6 May 1821)

1) *Skulde en galvanisk Leder ikke udøve mindste Tiltrækning paa et ugalvanisk?*
2) *Man kunde maaske gjendrive Berzelius's Theorie om galvanomagnetismus ved at vise at en Jerntraad, der havde udgjort en halv Ring om en tyk galvanisk Leder havde to modsatte Poler.*
3) *Man kunde bringe en Jerntraad paa tvers i en flydende Leder i Kjæden, saavel paa dens Overflade, saa at den udgjorde en Deel af Omkredsen, som i Midten, saa at den udgjorde en Diameter.*
4) *Man kunde prøve om bøiede Ledere, hvis Gjennemsnit var:*

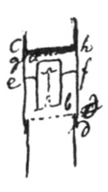


$bc = bd = a$, $ab = \delta$
$\delta^2 = 2a^2 + 2 \cos v . a^2$
$= 2a^2 (1 + \cos v)$
Er $v = 120^0$; saa er $\delta^2 = a^2$, og $\delta = a$ alt øvrigt lige ere Frastødningerne af vedkommende Punkter = Tiltrækn.

frastødte hinanden.

men da Tiltrækningerne her ere dobbelte saa maa δ^2 være $= 4a^2 (1 + \cos v)$, og $\cos v$ være $= 3/4$, naar δ^2 skulde være $= a^2$

5) *En magnetiseret og med Vox overtrukken Staaltraad lægges paa Qviksølv, og dette bruges som negativ Leder i en Kjæde.*
6) *Samme Forsøg uden at Naalen overdrages med Vox.*
7) *En Leder udskjæres saaledes at en Staalstrimmel eller en anden Metalstrimmel kan gjøres til en transversal Deel af Lederen, og let udtages; vorder denne derved magnetisk?*
8) *En fiin Magnetnaal opstilles inde i en huul Leder, for at see om den ei drejer sig. Den hule Leder kan have følgende Indretning.*



Et Glasrør ab, hvori en Magnetnaal, omgives af et Glasrør cd, som neden er lukket med en indkittet Leder, og fyldt med Qviksølv til ef, med en gjennemsigtig Leder til gh, og i øvrigt sluttet med en fast Leder.

NB. Herved kan Omdreining ei tilveiebringes, fordie Virkningerne ved a og b stræbe at give Naalen modsatte Retninger.

men en Naal som abc, ophængt ved Silkeormespindet da synes at maatte opfylde Fordringen.

existed; in these papers a series of subjects are set down in numerical order »to be tried« and »to be attempted«. The majority of subjects from the 1st of April deals with the connection between electrical, magnetical, chemical and heat effects. Some of the questions stand in close relation to subjects which had formerly excited his interest. Put off is evidently not given up, as one might be tempted to believe when *Ørsted* in his writings dealt with a subject and did not subsequently return to it. In No. 7 we thus find the question: »Are not sound vibrations accompanied by galvanomagnetic effects?« It is evidently his theory concerning the acoustical figures which he thinks may perhaps obtain new light or significance through the more recent discoveries. No. 9: »The hydrogenation of the metals and perhaps their separation ought to be tried with the greatest possible energy« refers to his experiments on cathodic dispersion concerning which he held the opinion that alloys between hydrogen and bismuth and between hydrogen and tin were formed. Such questions as No. 6: »Have iron wires half-fused to balls no magnetic or galvanomagnetic action?«, or No. 11: »Has an iron wire at the moment of fusion no special effect on the magnetic needle?«, belong to the notions about a connection between light, heat, and magnetic action which led to the discovery of the action of the current on the magnetic needle. Again, No. 8 contains directions how to construct a more easily adjustable circuit than the one employed at the discovery of the reaction effect from magnet on circuit.

All these great subjects were not, however, followed up, did not perhaps get any further than on paper; another great investigation took up *Ørsted's* time in 1821, namely experiments on the compressibility of water.

The paper of the 6th of May and No. 10 of the 1st of April specially aimed at refuting *Berzelius'* theory of the magnetic state of a conductor traversed by an electric current. In order to explain the action of the conducting wire on a magnet, *Berzelius* assumes that four poles are set up in the conductor which f. inst., in a conductor with a square section, are distributed in such a way that there are two north poles at the extremities of one diagonal and two south poles at the extremities of the other. About a year later *Ørsted* described another experiment he had made of a character somewhat different from those here proposed, but with the same purpose, it was published in several reviews.[1] Its purpose is to show that a long perpendicular conducting wire is surrounded by a circular field, as its effect on a small magnetic needle in its vicinity is not altered through the conductor making a revolution of 360^0 about its own axis.

From the autumn of 1822 to the summer of 1823 *Ørsted* was in Germany, France and England. It is a characteristic circumstance that he now profited in quite a different way from his experience and acquired a view of the state of the sciences in those countries quite different from the one he took in his youth. While in his first, extensive, tour he felt himself in close contact with the German scientists and their working methods, and took up a critical position with regard to the French men of science and did not feel at home in Paris, the reverse is now the case. He gives a detailed account of this in letters to his home. On the 9th of January he writes to *A. S. Ørsted* from Munich: »In poetry and philosophy I have not noticed that any new shining light has arisen in Germany in recent years. Nor does experimental science fare very well. Berlin has its excellent men in this branch of learning: *Seebeck*, *Erman*, *Mitscherlich*, *Heinrich Rose;* but from Berlin to Munich, on a journey of about 360 miles during which I have passed through three university towns, I have not found one fairly reliable chemist or experimenting physicist. *Schweigger* at Halle has brains and is a well grounded scholar, but also a reed shaken with the wind. His experiments are not of much importance. *Meinecke*, his coeditor of the chemical and physical journal, has brought to light no experiments of his own . . . . *Trommsdorf* at Erfurt makes new books every year by copying well-known German works. *Kastner* at Erlangen writes thick volumes compiled with much toil but without all judgment. *Yelin* at Munich makes indifferent experiments and lies much. But I have found much that was instructive with *Frauenhofer* at Munich, so that I have been able to occupy myself with benefit there for about a fortnight«.[2]

On the 23rd of February he writes to his wife from Paris: »The stay here grows more and more interesting to me every day, the acquaintances I have made grow every day more cordial and intimate; the benefit I can derive scientifically is thus all the greater. *Chevreul*, *Biot*, *Fresnel* and *Pouillet* are the men I particularly meet often,

[1] Ed. Vol. II. P. 265. [2] M. Ø. Vol. II. P. 41.

still I also very frequently see the other scholars either by visiting them or meeting them at parties. *Pouillet* is not yet famous outside Paris; but he cannot fail to gain an honourable name for himself.... Comprehensive science and not only skill in a single branch is now their watchword. »If you will mount a ray of light you may now be sure of finding something new«, said *Pouillet,* »but if you cannot attach it to the whole, what you find is but of small value«. He has written a mathematical paper on electromagnetism which is quite in the spirit I wish. . . . If in Germany I am often tempted to protest against Nature Philosophy when I see how it is misapplied, in France I feel so much the more called upon to defend it, or rather I feel a fundamental difference in scientific thought which I should not have imagined to be so great if I had not so often felt its vital presence. Still I am far from falling out with the French on account of this dissimilarity. I now know better how to appreciate their merit than before and am therefore on better terms with them«.[1] With special interest we read what *Ørsted* writes about *Ampère*: »On the 4th I dined with *Ampère* where I met *Arago, Fresnel, Chevreul, Dulong* and others. The conversation mostly turned upon scientific matters and I had a long discussion with *Ampère* about magnetism. He is a very unskillful debater and neither understands clearly how to comprehend the arguments of others nor how to set forth his own, nevertheless he is a profound thinker.[2] .... On the 10th I was at *Ampère's* by appointment to see his experiments. He had invited not a few. . . . He had three considerable galvanic apparatus ready, his instruments for showing the experiments are very complex, but what happened? Hardly any of his experiments succeeded. .... He is dreadfully confused and is equally unskillful as an experimenter and as a debater«.[3] — One wonders at this description of a man whose electromagnetical works are models of clear exposition, and whose experiments have been made use of all the world over to demonstrate the mutual action of the current and the magnet right up to our own day. — On the 25th of April *Ørsted* writes to his wife of one of his most remarkable experiences: »*Ampère* who has worked so much with my discovery and has founded a very elaborate theory on it, was greatly annoyed that I still keep to mine which is extremely simple. In order to have a conversation with me about this in the company

[1] M. Ø. Vol. II. P. 53. [2] M. Ø. Vol. II. P. 53. [3] l. c. P. 51.

of several scholars, he invited me to a dinner-party where *Fourier, Dulong, Chevreul, Frederik Cuvier, Savary* and *Montferrand* were present too. The two latter are young disciples of *Ampère's*. After the meal the conversation began and lasted for nearly three hours. I quite succeeded in proving that my theory accounts for all the phenomena, and what was most remarkable, I had to prove to *Fourier* that my theory was older than *Ampère's* which was, however, easy, seeing that I have already given it in my first publication. Even *Ampère's* two disciples declared that my theory was able to explain all the phenomena. They declare that so will *Ampère's*, and as his theory is nothing but the reverse of mine, he having removed the circuits of forces, discovered by me, from the conductor to the magnet, it will no doubt be difficult to find any entirely decisive objection to his theory, but I do not care for that either«.[1]

Ørsted continued to regard *Ampère's* theory with scepticism. In 1829—30 he thought he had found an »experimentum crucis« against it.[2] He placed a conductor in such a position in relation to a moveable filiform magnet that it was at right angles to the planes in which the currents in the magnet circled, thinking that, according to *Ampère's* theory, all influence should then be excluded while a movement of the magnet was observed. Curiously enough, among his papers is found an elaborate mathematical explanation of how, according to *Ampère's* law for the force between two arbitrary elements of a current, precisely that very movement of the magnetic needle might be expected which is actually seen. The explanation is not signed but dated the 1st of March 1833 and is not written in *Ørsted's* handwriting, but must without doubt have made him perceive his mistake which was obviously due to a deficient knowledge of the mathematical law for the force between two current-elements which *Ampère* had deduced from his experiments. When later on, f. inst. in the draft for a text-book found among his papers, compiled chiefly in 1845—46, he mentions *Ampère's* theory, he regards it, as in the Paris letter, as being of equal claim with, but not more acceptable than his own.

The stay in Paris became a busy time for *Ørsted*. A discovery

---

[1] M. Ø. Vol. II. P. 65. [2] Ed. Vol. II. P. 479.

made by *Seebeck* during an attempt to examine cells with different pole plates by means of the multiplier, and with which he had acquainted *Ørsted* when he passed through Berlin, was responsible for this. Through his experiments *Seebeck* had come to the conclusion that he might produce a current with a cell that contained only metals, and in order to investigate the matter more closely he laid a bismuth disc on the top of a copper disc and connected this latter to one end of the windings of a multiplier, while pressing the other end of the wire from the multiplier against the bismuth plate with his finger; he then got a deflection of the multiplier. When one end of the wire was fastened to the bismuth plate and the other was pressed with the finger against the copper disc, the deflection was in the opposite direction. If he replaced the bismuth disc by antimony, the deflections were reversed. Could it be the moisture of the finger which caused the generation of a current? No, a piece of damp paper laid between the finger and the place of contact weakened the effect or caused it to disappear. If a glass rod was pressed against the disc or, on the whole, an isolating rod instead of the finger, the effect disappeared; if a metal rod was employed no effect was produced unless the hand was near the place of contact.

It then gradually dawned on *Seebeck* that it was the heating of the junction that caused the current. *Ørsted* calls this observation »the most beautiful of the discoveries which have as yet grown out of mine«.[1] When he arrived at Paris he gave a report of *Seebeck's* work and results[2] in »Annales de chimie et de physique« and took up experimental work himself in order to extend *Seebeck's* researches. He writes about this to his wife: »The two metals that give the best effect in these experiments are bismuth and antimony. A piece made of these two metals, soldered together, has then the same effect as zinc and copper in a galvanical element. The heat, if I may so express myself, does the work of the moisture. It was thus a natural conjecture that of many pieces of antimony, bismuth, antimony, bismuth, continually alternating, it would be possible to solder together a ring giving galvanomagnetical effect when every second junction was heated, and in this way we should obtain a *Seebeck* battery in analogy with the Voltaic pile of galvanical elements. *Seebeck* seems to have had another theory about this. However, I have ex-

[1] M. Ø. Vol. II. P. 59. (4/4 1823). [2] Ed. Vol. II. P. 263.

perimented with the matter and found the conjecture correct. I believe that this discovery will be of far-reaching consequence. The laws for this new effect are, I suppose, in reality the same as for the galvanical battery; yet they look so different that I have been obliged to spend a great deal of my time during the last fortnight in discovering and defining them. . . . It is only yesterday that I have in some measure come to an end with the matter«.[1] In a letter of a somewhat later date to prince *Christian* he states that he has made the experiments »in conjunction with *Fourier*, the secretary of the mathematical department of the Institute«.[2] He laid the results before the Academie des Sciences on the 31st of March and published a paper[3] on the subject in Annales de Chimie et de Physique where he had formerly reported on *Schweigger's* multiplier and *Seebeck's* preliminary experiments.[4] He proposed the name »thermo-electric« for these currents a name which, as we know, has since been adopted everywhere.

We learn from his letters that the experiments on which the paper was founded had taken him 3 weeks, a space of time which is evidently much too short for the performance of the work; thus *Ørsted* himself points out a fundamental flaw in the experiments, but there has been no time to remedy it. The work is of interest, both by what has been gained through it, and by what does not plainly appear; in some of its results it is the precursor of *Ohm's* law and by its defects it shows how great was the feat of the actual discovery of this law.

The task *Ørsted* set himself was thus to examine whether the joining of several thermo-couples to a battery would lead to a similar increase of the effects traced in and by the connecting conductor as that which is observed by the joining of common galvanical cells. There were, however, two of these effects which were not at all traceable either with one or several thermo-couples; neither heat effects nor chemical effects could be demonstrated, neither did electrometrical measurements give the slightest results; only the action on the needle of the compass or the multiplier could be used for the investigation of this matter, and as a measure for the magnitude of the electromagnetical force was used the deflection of the magnetic needle under the influence of the current. *Ørsted*

[1] M. Ø. Vol. II. P. 59. [2] M. Ø. Vol. II. P. 70. (5/5 1823). [3] Ed. Vol. II. P. 272.
[4] Ed. Vol. II. P. 266.

thought this no very good measure: »Nous parlons ici toujours des déviations mesurées par les angles, et non pas de la grandeur réelle des effets«.[1] After some further reflections he adds: »on pourrait représenter les effets par les tangentes des déviations«,[2] but he does not use it, he only reckons with the deflections as a measure of the effect.

The chief defect of the experiments is that the difference in temperature between the junctions in the couples employed is not kept constant. Either one set of junctions is heated by the flame of a candle, or it is cooled with ice, or both agencies are used simultaneously so that one set is heated with a flame, the other cooled with ice. The latter arrangement which should be able to give a fairly constant difference in temperature is only used once, the two others are those most generally used; therefore in most cases an electromotive force was employed which varied greatly with the time. It is all the more remarkable that nothing was done to alter this circumstance as *Ørsted* was conscious of the defect and observed himself that in order to avoid that levelling out of the difference in temperature which will take place, especially in couples with short metal bars, one set of junctions should be connected with a constant source of heat, the other set with a constant source of cold. On his return from his journey he constructed a battery of 8 couples consisting of vessels one half of which was of antimony the other of bismuth. Through every second of these he passed warm water, through the others, cold. He demonstrated it before the Society of Sciences[3] and the Society proposed the construction of a battery of 50 couples; but nothing further is communicated about experiments with these batteries, and, in his treatise on thermo-electricity in the Edinburgh Encyclopedia which has already been mentioned, he recommends another construction,[4] so that it may be concluded either that his own did not answer to expectations, or that he had not the time or opportunity of testing it further.

In Paris the experiments were thus made with couples of inconstant electromotive force, and their effect was measured by a quantity which was not proportional to the strength of the current: the figures given and the conclusions drawn from them were therefore in general of no value. Only two results of importance were

[1] Ed. Vol. II. P. 277. [2] l. c. P. 277. [3] Ed. Vol. II. P. 462. [4] Ed. Vol. II. P. 391.

obtained. One was produced in experiments with a »complex thermoelectrical circuit« closed without external resistance and is stated thus: »l'effet d'un circuit ne change pas, lorsque la longueur de la circonférence augmente dans la même proportion que le nombre des élémens«.[1] This observation is correct as it means that the ratio between electromotive force and resistance is constant.

Next, the same circuit was used, one side being interrupted and the current transmitted through the windings of a multiplier. The deflection of the magnetic needle was very slight although the compass showed a great deflection if one side of the circuit with the couple shortcircuited was laid over the needle, here then was seen the importance of the external resistance for the strength of the current. Next, the current from thermoelectrical batteries varying in their number of elements was transmitted through the windings of the multiplier and the second chief result is seen: »que l'intensité des forces s'accroît dans le circuit avec le nombre de ses élémens, précisément comme cela a lieu dans la pile de *Volta*«.[2] As by »l'intensité« he meant »action électro-métrique« it is hereby expressed that the electromotive force increases proportionally to the number of elements.

These results are not mentioned in the introductory summary, it closes with the following passage: »Nous pouvons encore ajouter, que l'effet du circuit électromagnétique complexe, est beaucoup moindre que la somme des effets isolés que pouvaient produire les mêmes élémens employés à former des circuits simples«.[3] This proposition is correct if in the two cases, both with one and with several couples, the same external resistance, different from zero, is used, and if the internal resistance increases proportionally to the number of couples, but from the way in which the experiments were made which are described immediately after, and on which the proposition is presumably founded, it should be differently expressed. In these experiments the sum of internal and external resistance is constant whether one or more couples are used — at any rate if the alteration in resistance owing to heating is disregarded — hence the strength of the current should increase proportionally to the number of couples. When this does not take place it is no doubt due to the fact that the electromotive force will

[1] Ed. Vol. II. 4. Experience. P. 276. [2] l. c. 7. Experience. P. 279.
[3] Ed. Vol. II. P. 274.

not be proportional to this number, as the difference of temperature between the junctions decreases gradually on account of conduction when several junctions in the same circuit are heated. *Ohm* mentions[1] the proposition last quoted, and thinks that the error lies in the fact that the strength of the current has been measured by the deflection of a magnetic needle; even if it is measured by the tangent to the deflection, too small a value for the strength of the current will be obtained, and the divergence from proportionality between this and the number of thermo-couples is too great to be explained otherwise than by the lack of constancy in the couples.

Next the current from a thermo-couple was compared with the current from a small Zn-Ag cell with water as the liquid conductor. He found that by shortcircuit the liquid cell gives a much smaller deflection of a compass needle than the thermo-couple, whereas the magnetic needle of a multiplier is much more influenced by a current from a liquid cell; from this it is in the first place correctly inferred that »l'intensité des forces dans ce dernier circuit est beaucoup plus forte que dans l'autre«,[2] but next it is concluded that »le circuit thermo-électrique contient les forces électriques en quantité beaucoup plus grande qu'aucun circuit hydro-électrique de grandeur égale«.[3] »Forces électriques« in *Ørsted's* language is the same as »quantité des forces électriques« or quantity of electricity; and the idea here expressed, that an element possesses not only a definite electromotive force but also a definite quantity of electricity, combined with the want of understanding of the importance of the internal resistance, prevented the drawing up of a fruitful summary of the experiments. Not until three years later did *Ohm* succeed in interpreting similar experimental results rightly, when he had the opportunity of using a constant thermobattery.

At Utrecht *G. Moll* with some other scientists had also taken up *Seebeck's* thermo-couples for investigation and communicated their results;[4] these brought nothing new but led to an experiment which gave an apparently paradoxical result; the ends of a copper wire were connected and part of it rolled up in a spiral and immersed in acidulated water while the wire above the water

---

[1] *Schweigger's* Jahrbuch der Physik und Chemie. Vol. XVI. Nürnberg 1826. P. 127 et seq.
[2] Ed. Vol. II. 6e Experience. P. 278. [3] l. c. P. 278.
[4] The Edinburgh Philosophical Journal 1823. Vol. 9. P. 167.

was passed over a compass needle which, as had been expected, was not deflected; if now the copper spiral under the water was touched with a zinc bar, the magnetic needle showed by its motion that a current was started in the closed copper circuit. *Ørsted* took up this matter for investigation and published the results of his experiments,[1] he explained the phenomenon by a division of the current and varied the experiment in many neat ways to show that his view was right — here again he occupied himself with a subject which was taken up for more complete investigation by *Ohm* in 1826—27.

Ørsted returned to Copenhagen in the autumn of 1823, and it may be observed from short communications in Videnskabernes Selskabs Oversigter (Proceedings of the Society of Sciences) that now and again he worked at the things which had specially attracted his interest during his journey; it has already been mentioned that he tried to construct a constant thermobattery and that he sought to explain the problem which *Moll's* experiment had given rise to. His visit to *Fraunhofer* had directed his attention to spectrum-analysis: »This winter I have occupied myself a good deal in practising the latest optical experiments«, he writes in March 1824 to *Berzelius*. In the Society of Sciences he communicated a small observation in this domain.[2] He had seen that the spectrum from a metal wire, made incandescent by the electric current, was unbroken »without all light or dark lines, so that herein it proves different from all other varieties of light which have hitherto been examined«. From this remark it would seem as if the continuous spectrum was not known at the time.

From the following years there are only few signs that *Ørsted* occupied himself with electric researches, though he would occasionally, in connection with lectures or practical work, return to this domain from other things which occupied his thoughts more constantly. In 1825—26 he proposed[3] that the multiplier — which now according to *Nobili's* suggestion was constructed with a double needle — should be fitted with a controlling magnet so that its sensitiveness could be varied.[4] »It is an excellent apparatus for investigations on the electro-chemical effect of bodies, their conductivity etc. I also think of employing it for me-

[1] Ed. Vol. II. P. 282. [2] Ed. Vol. II. P. 463. [3] Ed. Vol. II. P. 471.
[4] Ed. Vol. II. P. 367.

tereological purposes« he wrote to *Hansteen* on the 28th of April 1826. In 1826—27 he suggested using the multiplier for testing silver,[1] as two pieces of silver of different fineness, placed in the same liquid, would give a current which could be demonstrated by the multiplier. He procured a number of strips of silver of given equi-distant degrees of fineness and by means of these he could find between what limits of fineness a given sample lay. Using the sample as one plate in a cell where the strips of silver with the known fineness alternately formed the other plate, he passed the current from the cell thus formed through the windings of a multiplier. He states that if diluted muriatic acid be used as the liquid conductor, a difference in »copper content« of $^1/_{100}$, or even less, may be ascertained. If another liquid is used with the same pieces of silver, an increased deflection of the multiplier will often be observed owing to other impurities; if the liquid conductor is a solution of potash and this effect appears, it may be inferred that the sample contains brass or arsen. *Ørsted* gives some more details of the procedure, but remarks: »Only a very detailed description will enable a metal worker with no knowledge of the art of experimenting to employ this method«.[2] He mentions this little investigation in his autobiography in 1828. During the years succeeding 1820—21 he was much occupied with »national undertakings« partly aiming at the practical application of physical science, and partly at its use in the service of popular education. These endeavours show themselves in his remarks on this work and in another simultaneous one, on an improved ringing of tower-bells:[3] »It cannot, I suppose, be considered superfluous that these and similar features are noted, as it is one of the objects of perfect science to act beneficially in the community, and with us it is not yet superfluous that examples hereof are brought to notice, since the more men trust science the more good will it do. They must be permeated by the conviction that the embellishment and elevation of human life is dependent on the Arts and Sciences, next to the true fear of God, and that these even act for the benefit of those who do not possess them«.[4]

---

[1] Ed. Vol. II. P. 473 & 337.
[2] Ed. Vol. II. P. 474.
[3] Ed. Vol. II. P. 474.
[4] Autobiogr. P. 541.

Once more, in the domain of electricity, *Ørsted* touched upon subjects of fundamental importance. In 1828—29 some experiments on magnetisation by means of the electric current[1] caused him to occupy himself with the question of remanent magnetism and the laws for the generation of heat in a conductor, but, as often before, »other business« diverted his efforts from an interesting beginning.

In other experimental domains we find important researches from the years succeeding 1820. It has already been mentioned that he particularly occupied himself with the laws for the compressibility of fluids and gases, but before entering more closely into this subject we shall give an account of some work in the domain of chemistry which has been important to science.

On February the 18th 1825 he made the communication to the Society of Sciences[2] that he »had succeeded in procuring a compound of chlorine and the combustible part or metal of alumina from which he hoped to be able to produce the metal by means of hydrogen«. On the 4th and 25th of March it is recorded: »Professor *Ørsted* read a paper on aluminium chloride and the method of obtaining aluminium from it«.[3] The method employed in these processes is more closely described in a paper in the Proceedings of the Society of Sciences for the same year.[4] Over a mixture of carbon and pure alumina heated to incandescence in a china tube was passed dry chlorine. »The alumina being thus able to rid itself of its oxygen, its combustible element combined with the chlorine and thereby formed a volatile compound which was easily caught in a receiver, which of course must be furnished with a drainage tube for the non-absorbed chlorine and the carbon dioxide formed«. Next, the chemical properties of the aluminium chloride are described and it is then added: »Heated quickly with potassium amalgam it suffers a decomposition whereby potassium chloride and aluminium amalgam are formed. In contact with air this amalgam is decomposed with great velocity and by distillation without the air being admitted it gives a lump of metal approaching tin in colour and lustre«. Here then *Ørsted* maintains that he has produced aluminium, and in April he shows a sample of the metal in the Society of Sciences.[5] He also produced silicon chloride by the

[1] Ed. Vol. II. P. 478. [2] Minutes of the Society of Sc. No. 2892 $^{18}/_{2}$ 1825.
[3] l. c. No. 2898. [4] Ed. Vol. II. P. 464. [5] Minutes of the Soc. $^{8}/_{4}$ and $^{22}/_{4}$ 1825. No. 2899.

same method as aluminium chloride, and from this he hoped to be able to produce »the combustible element of silica«, but the method was a difficult one on account of the volatility of the chlorine compound. He then adds concerning aluminium: »For the rest the author has found remarkable properties both in the obtained amalgam and in the metal, which do not allow him to consider the experiments as final but in all probality promise rich results«.

On the 9th of October 1825 he wrote a letter to *Schweigger* concerning this matter which the latter published in his Journal.[1] A note about it in the Norwegian Magasin for Naturvidenskaberne[2] taken from a letter from *Ørsted* to *Hansteen* of March 21st 1825 was reprinted in *Poggendorff's* Annalen der Physik.[3] There is this difference between the two communications that the one from March only mentions the production of the chlorine compound and expresses the hope of obtaining the metal from it, whereas the letter of the 9th of October says that *Ørsted* had succeeded in producing both the chlorine compound and the metal »argilium«. Among *Ørsted's* papers is found a more detailed description[4] of how the metal is found, and it is seen that his way of working has been right.[5] In spite of this *Ørsted* has not been credited with the honour of producing aluminium, it is always ascribed to *Wöhler*. His paper on this subject is from 1827 and bears the title: »Ueber das Aluminium«. He records that he read about *Ørsted's* experiments on the production of aluminium chloride, how he repeated them and indeed got the chlorine compound, but on distillation with potassium amalgam got a grey molten metal mass left, which by augmented heat superdistilled as pure potassium. *Ørsted* in his paper in the »Proceedings« having written that he would further pursue the matter, *Wöhler* communicated with him, and *Ørsted* encouraged him to take up the matter as his own work with it had stopped. *Wöhler* then produced aluminium by heating of the chloride with potassium. It will be seen that it was with this matter as with so many others,

---

[1] Ed. Vol. II. P. 297. [2] Mag. f. Naturv. Vol. V. P. 176—177. Christiania 1825.

[3] *Poggend.* Ann. d. Phys. Vol. V. P. 132. Leipzig 1825. [4] Ed. Vol. II. P. 467.

[5] Some unpublished investigations carried out 1920 by *J. Fogh* in the chemical laboratory of the Royal Veterinary and Agricultural College of Copenhagen have proved that it is possible to produce aluminium from aluminium chloride and kalium amalgam when following *Ørsted's* directions. When *Wöhler*, as mentioned below, did not succeed in producing aluminium in this way, it must presumably be due to the fact that he used too much kalium amalgam in proportion to the aluminium chloride.

Ørsted did not get time to follow up his ideas and researches to their final most conspicuous results. —

Yet his work in this field has been of importance to chemical science. »Scientific chemists at that time knew many methods of producing chlorine compounds of the elements, but the one indicated by *Ørsted* was new, and it has been of great importance because it has taught us to produce chlorides of many elements which otherwise we should not have been able to produce at all or only with great difficulty and expense. It can only be employed with any essential advantage where the chlorine compounds are volatile . . . . other scientific chemists have since employed the same method to produce chlorine compounds from many other elements«, writes *Forchhammer* in 1852. These words have held good until our day.

After this account of *Ørsted's* electromagnetical and other work we shall now turn to a survey of his achievements in quite a different field of research. —

In Videnskabernes Selskabs Oversigter (Proceedings of the Society of Sciences) for 1817—18 a brief notice from the hand of *Ørsted* about his experiments on the compressibility of water[1] was published, and here we meet for the first time the subject at which he worked most tenaciously for the rest of his life. Though only a small part of the vast researches has been published, it may be said that in this domain his achievement was of enduring value, his method and experimental apparatus having become the prototype and base of later researches.

The above-mentioned notice only gives a brief summary of what *Ørsted* brought before the Society. Among his posthumous papers, however, we find a more detailed account which answers point by point to the summary and so gives us more exact information of what he actually said. He begins with a historical survey from which it is seen that it was almost virgin soil on which he had been expending his labours. The first works he mentions date from the 17th century during which both *Bacon*, Academia del Cimento and *Boyle* made attempts to determine whether water was compressible at all. Their results could not, however, be said to be beyond doubt. It was not until 1761 that *Canton*[2] showed that the volume of water could be diminished by pressure, and measured the extent of the compression. He thought, too, that he was right in concluding from his experiments which were, it is true, only few, that the reduction in volume was proportional to the compressing forces. *Canton's* mode of proceeding was as follows: he had water in a thermometer tube and observed that the water rose in the tube when he placed the apparatus under the receiver of an airpump, and fell when he again let in the air. *Ørsted* calls attention to the fact that a slight change of temperature will have considerable influence on the observed value of the alteration in volume, and he is of opinion that a change in temperature will easily take place with the method adopted, as heat is evolved or absorbed when air is compressed or rarified.

The experiments which interested *Ørsted* most at this time, and of which an account was published in 1779,[3] were those carried out by *Abich* and *Zimmermann*. The liquid under investigation was enclosed

---

[1] Ed. Vol. II. P. 439.

[2] Phil. Transact. London. Vol. 52. 1762 and Vol. 54. 1764.

[3] *Zimmermann:* Ueber die Elasticität des Wassers. Leipzig 1779.

in a strong cylindrical brass vessel ending in a narrower cylinder, inside which a piston could be pressed down with varying force by means of a lever. The reduction in volume was measured by the motion of the piston. When the compressing forces were compared with the reduction in volume, they were found to increase simultaneously, but not according to any simple law. When *Ørsted* subjected the figures given to a closer investigation, remarkable discrepancies appeared. If thus the volume of the apparatus was calculated from its stated dimensions, a much smaller figure resulted than the one given as the outcome of direct measurement, and in the same way the area of the piston, calculated from the diameter, was different from that given by direct measurement. *Ørsted* solved this inconsistency by assuming that different units must have been employed in the measurement of the lengths and the measurement of the volume, and he succeeded in finding the ratio between these units. By employing it in calculating the reduction in volume for various positions of the piston, he showed that it appeared from the experiments of *Abich* and *Zimmermann* that the reduction in volume roughly speaking was proportional to the compressing forces. This induced *Ørsted* to make a closer experimental investigation of the matter, and he constructed an apparatus which in principle was like *Abich* and *Zimmermann's* but required a smaller application of force, and afforded a more exact determination of the reduction of volume as well as of the compressing forces. He then found the above-mentioned assumption of proportionality between these quantities approximately confirmed. *Ørsted* made experiments not only with water but also with spirits, and found a similar regularity here; the whole investigation is, however, of a preliminary character. By »the compressibility of water« he understood the ratio between the reduction of volume at the pressure of one atmosphere and the volume employed. For water his measurements gave the value 0·00012 at 12° C., a value which is about 3 times as great as that found by *Canton*. *Ørsted* thought *Canton's* figure was too small, the temperature of the water having probably risen slightly during the compression.

Involuntarily we ask how *Ørsted* was led to take up this work which was so remote from his other occupations. A loose sheet among his papers will perhaps afford an explanation. It is a fragment, and in a couple of pages contains some historical remarks on the compression of water. It is evidently part of a greater work, for drawings 59 and 61 are referred to, and at last *Abich* and *Zimmermann's* »highly successful experiments« are mentioned without any addition of critical remarks. At the end of the text some disconnected calculations have been added. It is seen that their object is to find the ratio between the compressing forces employed in the various measurements by *Abich* and *Zimmermann*, probably in order to compare these with the corresponding ratios between the observed contractions. So it would seem as if, through some literary work — perhaps a text-book on which he was working at the time — *Ørsted* was led to a closer investigation of *Abich* and *Zimmermann's* results, thence to a criticism of them, and thence again to taking up the work himself.

After this beginning the matter rested for about four years during which *Ørsted* sought other fields of research. From 1818 to 1819 he was occupied with »Undersøgelser over Bornholms Mineralrige«[1] (Investigations on the Minerals of Bornholm), being one of three members of a committee that carried out a geological survey on which *Ørsted* reported publicly in two papers. Besides he had much lecture work and, as previously described, through preparing a series of such he was led to his important discovery in 1820. The next couple of years he was engaged in electromagnetical and chemical researches, but already in 1822 he again took up his investigations on the compression of water, and from his papers we see that they were in full progress in the summer of 1822 when on the 3rd of August he wrote from Brede, where he was making a temporary stay, about the results to be expected from his experiments. The letter was written to Professor *Hansteen* of Christiania who was staying in Copenhagen and assisted him in the work. That *Hansteen* took a direct and independent part in the experiments is seen partly from a small treatise on his experimental results, written in his own hand, and found among *Ørsted's* papers, and partly from a note signed with his name, which showed that the manipulations demanded some adroitness:

»I had measured and weighed and filled several times with the greatest care. The last time I was fortunate enough to get 4 inches of mercury into the tube and was going to measure the length i. e.

[1] Ed. Vol. III. P. 201.

hold the compasses in one hand, the tube in the other, and at the same time with a finger prevent the admission of the air, I supported the bottle against the hooks of the copper cases; they gave way and — the neck broke; I have fled in despair, so as not to bear the brunt of your displeasure, and dare not show myself before I receive absolution.«

Your repentant

Chr. H.

The first explicit account of the methods and results of the experiments was given in the Proceedings of the Society of Sciences from May 1821 to May 1822[1] which were not, however, printed until the autumn of 1822, it being stated in the minutes of the Society's meetings in 1822 that at the meeting on the 12th of October *Ørsted* submitted the draft for the »Proceedings« and exhibited an apparatus for the compression of water; that it was decided that this should be mentioned in the »Proceedings«; and that *Ørsted* then took his leave in order to go abroad for some time. But already in the September number of the Ann. de Chimie et de Physique we find a short description of the new apparatus and the experiments carried out with it. Its source is not stated, but later on, when in 1823 a more elaborate treatise[2] from the hand of *Ørsted* on the same subject appeared in this periodical, it was stated that the short communication was taken from an English account. He had, then, such a high opinion of his apparatus that already in the course of the summer, while at work with it, he let a description of it be published. He carried the apparatus with him on his long journey abroad, and he mentions in his letters that he showed experiments with it wherever he went.[3] During his stay in Paris he wrote the before-mentioned treatise about the matter, but this contained no more experimental results than the few from the treatise in the »Proceedings«. Among his papers, however, we find a more detailed account[4] of experiments and experimental data partly set down in *Ørsted's* partly in *Hansteen's* hand. We shall now take a closer look at these papers.

In these *Ørsted* makes a valuable contribution towards the

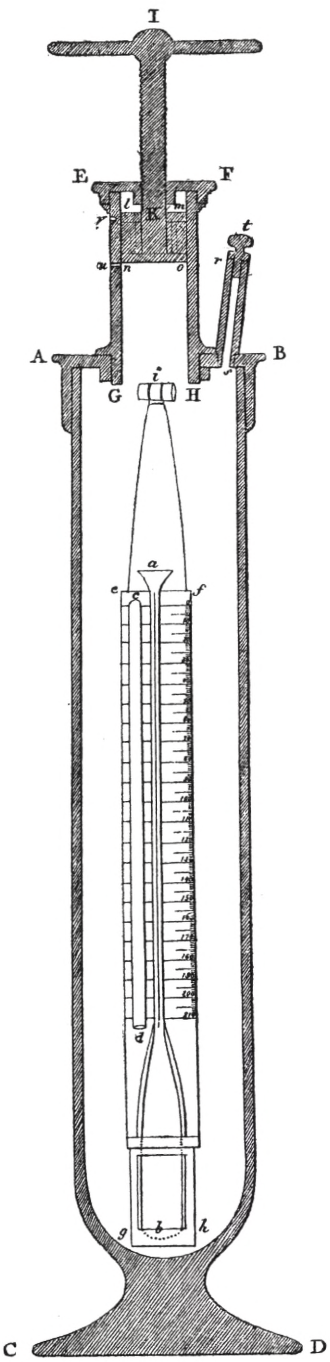
[1] Ed. Vol. II. P. 455, the account was translated and published with a figure in *Schweigger's* Journal. Vol. 36. 1822. Ed. Vol. II. P. 254. [2] Ed. Vol. II. P. 258.

[3] Ed. Vol. II. P. 254, note. [4] Ed. Vol. II. P. 458.

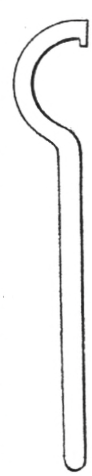
solution of the question concerning the compressibility of fluids, abandoning his previous method and starting on quite a new course. These were the years in which he was successful in so many respects — he discovered electromagnetism, found a new alkaloid in pepper, evolved a fruitful method of producing $Al\ Cl_3$, and now finally constructed a simple and accurate apparatus for measuring the compressibility of fluids.

In the paper[1] in the »Proceedings« for 1821—22 he first criticises his own apparatus of 1817—18, it having become plain to him that he had not measured »the compression of the water alone, but the combined effect of this and the expansion of the vessel. Likewise in such experiments the influence of heat should be kept count of.« His view of *Canton's* experiments had now quite altered, he saw that they were the only ones of those mentioned by him in 1817—18 which had any lasting value, and he speaks with admiration of them, because by *Canton's* method the vessel in which the fluid was compressed received the same pressure from without and within. From this circumstance *Ørsted* concluded that in these experiments the volume of the vessel was not altered by the pressure, and that it might consequently be taken for granted that what was observed was only the alteration in volume of the fluid. Some experiments by *Perkins* carried out in 1820[2] had the same advantage, about which *Ørsted*, by the way, wrote in Ann. de Chim. et de Phys. that he did not know them before he constructed his own apparatus. *Perkins* poured the water to be compressed into a cylindrical metal tube having a closely fitting cylindrical bar for a plug. The apparatus was placed in a very thick-walled wider cylinder containing water which could be subjected to high pressures. The pressure was communicated to the rod which was pressed down in the cylinder, the water in the latter giving way to the pressure. The decrease in volume was measured by the distance the rod had descended, which was indicated by an elastic ring fitting round the rod where it entered the cylinder, and ascending with it when it came up again. The pressure was measured by examining the necessary load on a kind of safety valve closing a tube on the compression cylinder. Neither the measurement of volume nor of pressure could be made with any great exactness, but the principle of compression calls *Ørsted's* to mind.

[1] Ed. Vol. II. P. 455. [2] Philosophical Transactions of the Roy. Soc. of London 1820. P. 324.



In the above-mentioned paper *Ørsted* described his apparatus which was very convenient and admitted of such exact measurements of volume and pressure that his experiments with it are acknowledged as »the first accurate experiments« in this domain.[1] The fluid was enclosed in a piezometer (see figure)[2], an oblong flask with so narrow a neck that the volume corresponding to a length of it equal to a line amounted to 0·000055 of the whole volume of the flask. The fluid was separated from the water in the compression apparatus by a column of mercury which — at any rate in one experiment — was about 4 inches long. The flask was placed in water in a wider glass cylinder, and this water was compressed by a piston. The piezometer thus got the same pressure from within and without, so *Ørsted* took it for granted that its volume was not altered by the pressure, and that the observed alterations in volume of the enclosed fluid must be ascribed entirely to the elasticity of the latter. The pressure was measured by a manometer placed inside the flask in which some air was compressed by the water, the pressure being calculated by *Mariotte's* law.



The piezometer itself served as a thermometer and moreover a very sensitive one, a rise in temperature of one degree causing the water to rise 27‴ in the narrow tube in which it ends. If now in a compression experiment the reading of the volume was the same before and after the

[1] *Wüllner:* Lehrbuch der Experimentalphysik. Vol. I. P. 331. Leipzig 1907.
[2] Reproduced from a paper published later in »Vid. Selsk. Skrift.« Ed. Vol. II. P. 298.

application of the pressure, it was taken for granted that no heating had taken place. This argument is not correct. If heat is evolved during compression, it will disappear during the succeeding expansion, and the fluid will return to the same temperature as before the compression, and therefore occupy the same volume again. This presupposes, though, that heat is neither added nor carried off. It was also seen that such regularity only occurred if the compression and decrease of pressure took a very short time. When this was the case the water would mostly be $^1/_8'''$—$^1/_4'''$ higher in the piezometer after than before the compression, which — if the phenomenon were due to change in temperature — would answer to a change of $^1/_{200}{}^0$—$^1/_{100}{}^0$. The same difference occurring whether the pressure had been increased to 1 or to 5 atmospheres, it was concluded that the rise in temperature was not due to the compression but to a communication of heat from the experimenter.

That this question caused some difficulty is seen from a series of experiments carried out by *Hansteen*, probably in July 1822, since in the before-mentioned letter of August 3rd *Ørsted* requests that the calculations may be forwarded to him; he expresses a conjecture of how water will behave on decrease of pressure at various temperatures, and says that it has perhaps already been shown by experiments how the matter stands.

Hansteen had first compressed and read the position of the column of mercury at a series of increasing pressures, and then let the pressure gradually decrease towards the same value as in the beginning, and had again read the position of the column at a series of decreasing pressures. Here he did not get the same reading at the same pressures in the ascending and descending series; the column was 3''' higher at the conclusion of the experiment than at its beginning, which corresponded to a rise in temperature of about $^1/_9{}^0$. As, for the greatest pressure applied, the mercury had only moved 14''' from the initial position, there was no little uncertainty in the measured figures, which he sought to remove by introducing a correction for the supposed rise in temperature.

When we see the mechanical ingenuity displayed by *Ørsted* in the construction of the apparatus for the compression of water, and in the many little things which make it convenient for use, and also from his notes see how much better *Ørsted's* measurements were than *Hansteen's*, we recall with surprise *Hansteen's* remark in a letter to *Faraday* (Dec. 30. 1857) that *Ørsted* was an indifferent experimenter.[1]

One of *Hansteen's* experimental series was not reliable because the column of mercury did not go back at all when the pressure was discontinued. A hint was thus given that the long string of mercury, by the hindrances to its displacement, rendered the results uncertain, and, as we shall see later on, *Ørsted* found a method by which the string might be omitted when he commenced a fresh series of measurements in this field in 1826—27.

Ørsted did not publish *Hansteen's* results, and neither mentioned them nor the conclusions *Hansteen* drew from them; about his own experimental results he only states that they have shown him that »the compressibilities are in proportion to the compressing powers«, and that the ratio between the reduction of volume and the original volume at the pressure of one atmosphere by 15°—16° C. is 0·0000455. *Canton* got at 64° Fahr. 0·000044
» 34° » 0·000049

In a later experiment with another apparatus *Ørsted* got 0·000044; this figure is not accompanied by any indication of temperature, but must be supposed to correspond to 15° C. *Ørsted* is perfectly right in saying that »the agreement between these experiments and *Canton's* is really extraordinary«.[2]

Among *Ørsted's* papers there are a couple of sheets[3] with experimental data from the 23rd and 24th of September 1822 and the 9th of October of the same year, referring to experiments carried out at 15° and 16 1/2° C. and with an increase of pressure from 1/3 atmosphere to 5 atmospheres. They inform us of the uncertainty of the results which runs to about 4 per cent. The compressibility calculated from the mean of the observations is 0·000047. It is especially the value at 1/2 atmosphere's increase of pressure which deviates; if this is excluded, the discrepancy between the others is not 2 per cent. In a report on his method and experiments in this domain in *Schweigger's* Journal 1822,[4] *Ørsted* states that the latest and best experiments have given the value 0·000047 for the compression of

---

[1] Life and Letters of *Faraday* by *B. Jones* 1870. Part II. P. 389. »Professor *Oersted* was a man of genius, but he was a very unhappy experimenter; he could not manipulate instruments. He must always have an assistant, or one of his auditors who had easy hands, to arrange the experiment; I have often in this way assisted him as his auditor.«

[2] Ed. Vol. II. P. 458. [3] Ed. Vol. II. P. 458. [4] Ed. Vol. II. P. 254.

water, it is evidently the experimental series from September and October he is here referring to. Hence it is strange that in the paper in Annales de Chimie et de Physique some months later he returns to the value 0·000045.

As mentioned above, *Ørsted* was so taken up by his apparatus and the experiments which could be made with it that he carried it with him on a journey and occasionally made demonstrations with it, but otherwise his interest and attention on the journey were directed towards quite a different subject he having, as mentioned, become acquainted in Berlin with *Seebeck's* thermoelectrical experiments. In Paris a great deal of his time was employed in work in this field, work which he continued after his return home, as evidenced in the »Proceedings« for the two following years.

Not until 1824—25 did *Ørsted* return to his experiments on compressibility, but this time gases were the object of his investigations. With Captain *Suensson* he tested the validity of *Mariotte's* law for air for considerable increases of pressure. The first statement of the results appeared in the Proceedings of the Society of Sciences 1824—25,[1] but they were described in detail in a paper in »Det kgl. danske Videnskabernes Selskabs Skrifter« (Publications of the Roy. Dan. Soc. of Sciences), Part II, 1823[2] which was not, however, ready for the press until 1826. Another report of them is found in a letter to *Brewster*, and in 1825 in an explicit treatise in *Schweigger's* Journal.[3]

From the paper in the »Publications« we learn what occasioned the work. As often before a theoretical as well as an incidental practical reason was his incentive. In the preface to the treatise he says: »It seems that . . in all bodies . . . the compression is in proportion to the compressing powers«,[4] and it was in order to explain how this law was found for gases as well as for fluids that the paper was written. The practical cause for taking up the experiments was that in the summer of 1824 he undertook to carry out some investigations on the theory of the air gun with Captain *Suensson* of the Royal Engineers. From this practical purpose he was led to the broader, more comprehensive theoretical aim which also played a part in the experiments of 1822.

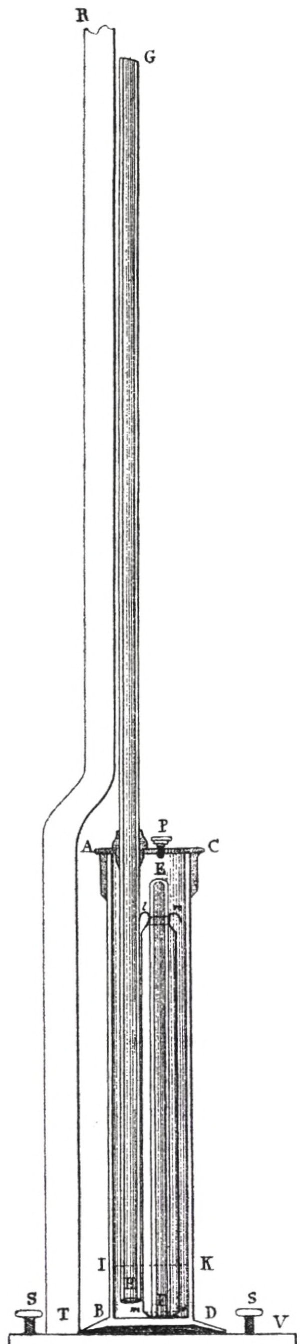
Ørsted's and *Suensson's* experiments on *Mariotte's* law were a

[1] Ed. Vol. II. P. 464. [2] Ed. Vol. II. P. 298. [3] Ed. Vol. II. P. 285.
[4] Ed. Vol. II. P. 298.

great step forward. In the latter half of the 18th century a number of experiments had been made in order to examine the validity of the law for pressures up to 8 atmospheres, and the chief result was that the air was considerably more compressible than it should be according to the law. The deviations were so considerable that *Ørsted* writes: »From these experiments one would even be tempted to believe that the point of condensation at which the air liquefies must be within easy reach and achievable with compressing powers procurable without difficulty«.[1] The experiments had been carried out according to the well-known method of *Mariotte*, and *Ørsted* saw that the main errors in experiments thus carried out originated in the measurement of the volume of the confined air. The volumes employed were too small, so that errors in the reading of the closing mercury column involved great percentage errors in the measurement of the volume, it having been considered necessary to confine the air in very thick-walled and narrow tubes in order that they might be able to resist the great internal pressure. The volumes of air were thus small and the width of the tubes irregular, and a measurement of volume exclusively based on observing the height of the closing mercury must always be very inaccurate. *Ørsted's* efforts were then directed in the first place towards rendering the measurements more exact, and, in the second place, towards extending the domain for which the law had been examined by finding a method of observing the compressed air at considerably higher pressures than hitherto employed. He solved both problems, though by pressures above 8 atmospheres he had to employ an entirely new method which certainly permitted of the use of pressures up to 60—70 atmospheres, but on the other hand was not accurate enough to ascertain slight deviations from the law in the case of atmospheric air. *Ørsted* had thus achieved three important things in this field of research: the improvement of the methods of investigation for pressures up to 8 atmospheres, so that the approximate validity of the law for these pressures was demonstrable, the elaboration of a method to examine the validity of the law at higher pressures showing that the deviations for atmospheric air were at any rate not great; finally, the employment of a method to compress other gases and compare their

---

[1] Ed. Vol. II. P. 299.



compressibility with that of atmospheric air, by which it was proved that *Mariotte's* law held for these, too, when the pressure was far from the liquefaction-pressure at the existent temperature, but not when the pressure approached or reached this value.

As a receiver for the air to be compressed *Ørsted* used a cylindrical tube 6 lines wide and 1 1/2 feet long. It was carefully divided into lines, the capacity of the whole tube was determined by weighing with mercury, and in the same way the capacities of the divisions. The reason why a tube of such great width could be used was that it was placed in water which was compressed at the same pressure as the air in the tube, so that the latter was subjected to the same internal and external pressures. The air in the tube was dried by placing the tube for a long time in a cylinder with calcium chloride. It was then placed, as shown in the figure, in a cylinder filled with water with its mouth beneath the surface of mercury at the bottom of the cylinder. In this was also placed a glass tube open at both ends which passed airtight through the screw-cover of the cylinder. This tube was gradually filled with mercury and thus acted both as compressor and manometer as in the usual experiments according to *Mariotte's* method. It was composed of pieces 7 feet long which were held together by strong iron screws, but it was very difficult to make cementing and screws tight enough, and only once a pressure of 8 atmospheres was successfully sustained.

Some results of the experiments with this apparatus are given in a table[1] of 4 columns, as follows: — 1) the ratios in which the

[1] Ed. Vol. II. P. 304.

volume is reduced, 2) the corresponding ratios in which the pressure is increased, 3) the differences between these ratios which should be equal if *Mariotte's* law held, and 4) »the ratio between these differences and the main quantities«. The last column shows that this ratio in only two out of 17 cases comes up to 0·016 and 0·013, in all other cases it is less than one in a hundred. The table shows that the differences in the 3rd column are positive all but one, i. e. that the volumes are reduced at a somewhat higher rate than demanded by *Mariotte's* law. *Ørsted*, however, calls attention to the fact that this conclusion is not justifiable since the curved surface of the closing mercury makes the reading uncertain. »In all these experiments we have endeavoured to divide, by the eye, the convex part into two parts of equal volume, but the results show that we have attributed too little of this volume to the inclosed air. . . . Without this error the differences would have been smaller, and would have alternated between + and ÷. Apart from this the differences are as small as could be expected in experiments where no vernier could be used. In the last observation, for example, the length of the column of air was 25 lines. Had it been assumed $^1/_{20}$ line longer, the volume would have been $^1/_{500}$ greater, and the deviation would thus disappear«.[1] It is here seen how *Ørsted* argues in quite a different and more modern manner about his experimental results than before, subjecting them to a sober judgment which was previously foreign to him.

From a couple of sheets in his own handwriting[2] found among his papers we learn of the experiments which are the precise supplement to the table published in the treatise in the »Publications«. Here, among other things, we find the correction, before mentioned, for the convexity of the mercury surface and, moreover, we find what volumes should be allowed for when this correction was introduced. It is from here, too, we have the information that the capacities corresponding to the divisions of the tube were determined by weighing the portions of mercury in each.

How excellent these experiments of *Ørsted's* were for the time may be seen by comparison with what was done later and considered fundamental for the investigations on the law of *Mariotte*. Thus it is generally known that *Despretz* in 1827 com-

---

[1] Ed. Vol. II. P. 304. [2] Ed. Vol. II. P. 322.

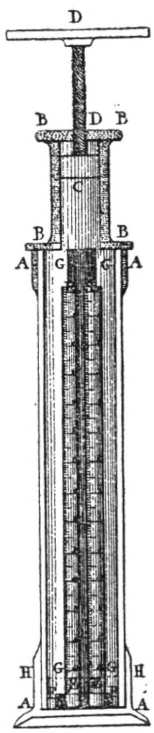
pared the compressibility of various gases and found that it was different for different gases, those which liquefied easily deviating at increasing pressure from the atmospheric air in the direction of greater compressibility, but it is less well known that his method is the same as *Ørsted* employed three years earlier with the same purpose and a similar result, and that he used the apparatus constructed by *Ørsted.*

Dulong's and *Arago's* famous experiments from 1830 on *Mariotte's* law were made with an arrangement similar to *Ørsted's*, but by an ingenious joining of the manometer tube they succeeded in pushing the compression as far as 30 atmospheres. The accuracy actually obtained by them was not much greater than *Ørsted's* within the same limits, the deviations found by them tend the same way — in the direction of greater compressibility, but as the deviations are small, they take it for granted that they are due to experimental errors. *Ørsted,* on the other hand, shows, before he draws this conclusion, that a slight and justifiable correction will give the deviations alternate signs and hence do away with the regular »course« of the errors which might denote a real deviation from the law. *Ørsted's* experiments ought to come in for a share of the many eulogies bestowed in text-books on the accuracy of *Dulong & Arago's* experiments.

Further, as mentioned above, he used his compression apparatus to compare the compressibility of various gases.[1] As shown in the figure, he placed two graduated glass tubes, closed at the top, with their open ends in a basin of mercury side by side in the apparatus. One was filled with atmospheric air, the other with the gas to be compared with it. If now increasing pressures were applied, the mercury would rise in the glass tubes as the gases were compressed, and if the variations in volume occur in the same proportion, the law will hold in the same way for both, as the pressures to which they are subjected are the same. Of the experiments performed only one explicit series[2] with SO_2 and air has been given in the treatise. It shows that the volumes were reduced in the same ratio until the pressure, calculated from the reduction in volume of the atmospheric air, had reached 2·3 atmospheres. After this SO_2 was compressed more than the air, and at 3·2689 atmospheres condensed sulphurous acid became visible in the tube. The

[1] Ed. Vol. II. P. 307. [2] l. c. P. 309.

experiment was performed at a temperature of $21^1/_2{}^0$ C. In his papers several other experiments are mentioned[1] and it is stated both there and in the treatise that the experiments with cyanogen and ammonia have given analogous results.



A remark in the notes seems to show that *Ørsted* has seen liquefaction by expansion, for it is stated that upon a decrease of compression »a white smoke« appeared in the sulphurous acid tube,[2] but it gradually disappeared. *Ørsted* has further examined the validity of *Mariotte's* law for SO_2 by the method described on p. CXXXVI, and he obtained results[3] agreeing with those mentioned above. The table for these experiments is written on the sheet but has not been published. It is seen that the volume decreased in almost the same proportion as the pressure increased until about 2·5 atmospheres, after this SO_2 was compressed faster, and by a pressure of 3·36 atmospheres the liquid sulphurous acid was visible; the temperature was $19^1/_2$ to $20^1/_2$ C. In the paper in *Schweigger's* Journal for 1825[4] a more detailed account was given of the observations on cyanogen, and it was stated that the liquefaction of this gas at a temperature of 23^0 began with a pressure of 3·5 atmospheres. *Ørsted* emphasises that he made these experiments in order to ascertain whether *Mariotte's* law held for all gases, and hence it will be seen, as mentioned above, that *Despretz's* well-known experiments from 1827 in their aim, their method, and their results were a direct repetition of *Ørsted's*. *Despretz*, however, subjected some more gases to examination and pushed the compression as far as 15 atmospheres. *Ørsted* himself speaks with great modesty of the significance of his experiments. At the close of the paper in *Schweigger's* Journal he says[5] that his experiments have served to show the validity of *Mariotte's* law for gases, and in so far have only served to confirm the opinion held by the most distinguished scholars of the day, but as there are still scientists holding the opposite opinion, his publication of the method and results of his experiments may not perhaps be without importance.

[1] Ed. Vol II. P. 322. [2] l. c. P. 324. [3] l. c. P. 325.
[4] Ed. Vol. II. P. 285. [5] Ed. Vol. II. P. 296.

We shall next give an account of how *Ørsted* carried out experiments on the validity of *Mariotte's* law for atmospheric air up to a pressure of 60 atmospheres — the first set of experiments on the validity of the law which employed such high pressures.[1] The method was quite original. The two experimenters had been given the loan of some culasses from air guns, the experiments being first prompted by the wish to ascertain the theory of the air gun. The culasse of the air gun most frequently employed[2] would hold 0·891 gms of air at atmospheric pressure. Air was now pumped into it and by a number of pumpings the air was so greatly condensed that upon weighing the culasse was found to contain up to 101·2 gms. From this weighing the pressure in the culasse was calculated on the assumption that *Mariotte's* law was valid. In this calculation the expansion of the culasse by the great internal pressure was taken into account, its capacity being controlled by weighing in water. The capacity of the culasse in this experiment was 685·3 cm^3, and the expansion 10·2 cm^3 at a pressure of about 60 atmospheres.

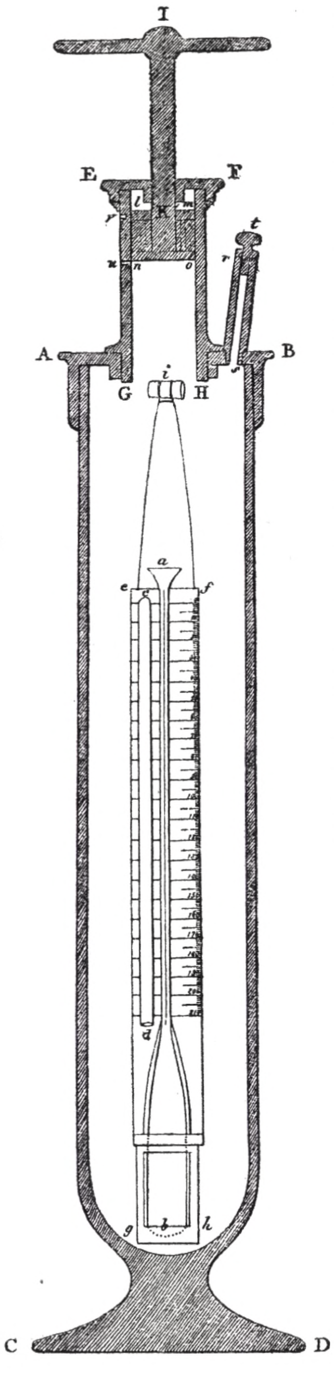
Next, the pressure of the enclosed air was measured directly by finding, by means of a lever, how great a pressure was necessary to open the valve, and the measured and the computed pressures were compared. When the pressure of the air had thus been found — in the above-mentioned experiment the calculated final pressure was 110·5 atmospheres — the valve was opened, some air escaped, the culasse was again weighed, and the pressure again computed and measured directly. The measurements in the first sets of experiments were so uncertain that sometimes the figure obtained was a great deal larger, sometimes smaller than that computed.[3] It was supposed that the irregularities were due to the valve being covered with leather. It was then replaced by a steel valve ground into the aperture, and in the first series of experiments with this the results were certainly more regular,[4] but it could not resist a pressure above 11 atmospheres. At last a steel valve was obtained which could sustain a pressure of 66 atmospheres and which gave regular results. These are given in a table in the treatises.[5] The agreement between the measured and computed quantities was examined in the following way: the calculated pressure was given in atmospheres, the measured pressure in grammes, and

[1] Ed. Vol. II. P. 304. [2] Ed. Vol. II. P. 305. [3] Ed. Vol. II. P. 292, note.
[4] l. c. P. 292. [5] Ed. Vol. II. P. 293 & 307.

in the last column of the table the ratio between the measured and the calculated pressures was given, i. e. it was found how many grammes of the measured pressure corresponded to 1 atmosphere. This ratio should of course be constant if *Mariotte's* law were right. If we neglect the experiment with 1 gm of air in the culasse, which is far removed from what the other 25 measurements give for this ratio, only some few of the values for this differ about 2 per cent from the mean value 1027, the majority showing much smaller deviations. From this *Ørsted* concluded that *Mariotte's* law was valid also for these high pressures.

The remainder of the treatise in the Publications of the Society of Sciences gives an account of the experiments on the compressibility of water[1] which were carried out mainly in 1822—23. *Ørsted* explicitly describes his apparatus and explains how it may best be utilised; he criticises its less fortunate qualities and states how he thinks to remedy its defects when fresh experiments are to be performed with it. In the first place he intended to alter the method of measuring the pressure. For this he had hitherto had a rather short glass tube, containing air, with the mouth downwards and fastened to the same frame which carried the piezometer (cd in the figure). He now intended to separate the tube from the latter, placing it in the same manner as the tubes with air and SO_2 in the compression experiments for the comparison of these two gases, thus immersing its mouth in mercury at the bottom of the compression cylinder. By this arrangement he would be able to use a longer tube. He also intended to let the capillary of the piezometer be supported by a ground glass stopper to be placed in the neck of the reservoir, as in many cases, f. inst. when boiling out the air of the reservoir for filling it with fluid, it was a great disadvantage to have to manipulate so large a flask — from 50 to 60 cm^3 — with such a long and thin neck. With regard to the accuracy presumably obtainable in these experiments he writes: »There are still many small defects incident to this instrument which may be done away with by future work, so that I do not deem it impossible that in the experiments hitherto carried out there might be an error of some few millionths; I ought, however, to say that the experiments I have performed, after having attained to some proficiency in the handling of the instrument, even with different flasks gave very

[1] Ed. Vol. II. P. 310.



nearly the same mean value, namely 45 millionths for a pressure equal to that of the atmosphere when it carries 28 inches of mercury at 15^0 (centigrade) of heat. A deviation of 2 or 3 millionths above or below this mean have been rare . . . I have repeated these experiments many hundreds of times, first in order to convince myself and next to exhibit them to scholars and other friends of science, here as well as in foreign countries. I therefore communicate no series of the figures obtained in the experiments.« So *Ørsted* follows his habit of being very sparing in the statement of his experimental results. In a letter[1] from him to *Hansteen* of Sept. 28th 1826 it is seen that the latter taxed him with letting his tables on the compression of air contain the results of calculations based on measurements but not a sufficient number of these, and *Ørsted* admits that »the thorough-going reader might miss this«, but his usual practice is to publish only a small part of his experimental results, a circumstance which has contributed much to obscure the importance of his researches.

The treatise closes with some »General Remarks on Compressions« from which we see that in his opinion his experiments had shown the general law »that the compressions are proportional to the compressing forces«. He has proved it for air and for water and a few other fluids; it must also hold for solid bodies, since in his compression experiments with water in 1817—18 he found the same

[1] *Harding's* Collection of *Ørsted's* letters.

law valid although he then measured the sum of the alterations in volume of the water and the solid reservoir. As in the new experiments the compressibility of the water proved proportional to the pressure, the compressibility of the substance of the solid reservoir must also follow that law.

Finally, he hints that he has more extensive plans for experiments on compressibility. He made the same statement to *Hansteen* in a letter of the 25th of May 1826[1] when the communications here summarised were to be printed in the Publications of the Society of Sciences: »On going through them once more I have come to make an elaborate plan for such experiments, the expense of which will be defrayed by the Society of Sciences«. The plan was evidently in his mind the whole of the previous winter, for at a meeting of the Society of Sciences on the 16th of Dec. 1825 he read a paper on experiments on the compression of fluids and gases which it would be desirable to carry out; at a meeting in April 1826 the question of how to employ the funds of the Society was discussed, and the Secretary spoke on how the money might be put to important use in experimental investigations. He mentioned as an example that an apparatus was needed for the investigation of *Mariotte's* law from which important and no doubt pregnant results might be expected. Finally, on the 12th of May he laid before the Society a detailed plan for experiments on the compressibility of bodies. The plan itself is not in print, but is found in a Journal of Experiments which had been commenced in May 1826 immediately after the above-mentioned meeting. The beginning of it runs as follows: —

»At the meeting of the Royal Danish Society of Sciences on the 12th of May 1826 Professor *Ørsted* submitted the following proposal for carrying out fresh experiments on the compression of bodies:

As in all hitherto described experiments on the effect of great compressing forces on water, great uncertainty still remains, and even those by *Perkins* cannot be excepted from this judgment, I propose that the Society at its own charge causes experiments to be carried out on the compression of water which go at least as far as a pressure of 100 atmospheres.

---

[1] *Harding's* Collection of *Ørsted's* letters.

That furthermore it causes experiments to be made

1) On the compression of mercury.
2) On the compression of the ethers.
3) On the compression of spirits of wine at various degrees of dilution.
4) As far as possible also on the compressibility of condensed sulphurous acid.
5) On the compression of ammonia and other fluids containing a condensed gas.
6) On the compression of salt solutions, as to whether it is right that the salts may be disengaged by the compression.
7) As to whether heat is evolved by the compression of fluids.
8) As to whether the compressibility varies with the temperature of the fluids.
9) As to whether compression would produce compounds which would not otherwise take place.
10) How much a vessel in which the compression takes place is expanded by the compressing power when no pressure from without counterbalances it.
11) The compression of solids contained in fluids.

On the compression of gaseous bodies the following experiments are proposed: —

1) The definite degree of compression by which gases at a certain temperature liquefy.
2) The tension of a gas which is in contact with its parent fluid.
3) High degrees of artificial cold produced by the parent fluids of the gases passing into gaseous state.
4) The improvement of the air gun in consequence of these experiments.
5) Chemical compounds in consequence of the compression of gases.«

In consequence of this proposal the Society determined to cause these experiments to be carried out and to pay the necessary expenses which were not, however, to exceed 500 Rdr. It was further arranged that as an assistant in these experiments Professor *Ørsted* might engage »Hofmecanicus« Mr. *Foch* and as a remuneration for the time he gave cause 20 Rbd. to be paid to him monthly.

The plan seems exceedingly comprehensive and bears some traces of *Ørsted's* early love of the large programmes, still, it immediately takes on a less imposing aspect when we learn that the Society's expenditure for the realisation of such great purposes was not to exceed 500 Rdr., out of which moreover a salary of 20 Rd. monthly for an assistant was to be paid.

Ørsted at once set eagerly to work to improve his compression apparatus, that it might give more exact measurements, become more convenient for use, and push the pressure further. He reports on these things in the Proceedings of the Society for 1826—27,[1] but says that they cannot be explained without drawings, and that an explicit account of them will appear in the Publications of the Society. This never took place, but a manuscript[2] among his papers gives detailed information, and such is also found in a letter in Reports of the British Association for 1833.[3]

The measurement of the volume during the compressions was improved by omitting the long string of mercury in the capillary and only letting the liquid in the piezometer be separated from the compression-water by a space of air, the mouth of the flask being covered by a kind of diving bell.

The measuring of the pressure was considerably improved by putting into practice the before-mentioned plan of using as a manometer a longer and wider tube with air, the mouth of which was closed by mercury at the bottom of the compression cylinder. By using the larger tube the advantage was obtained of smaller errors in reading the volume of the enclosed air, and consequently smaller percentage errors in the calculation of the pressure, the volumes of air employed being larger than before, and finally recourse could now be had to large pressures as these could now be measured without too great uncertainty.

Furthermore the apparatus was furnished with accessory contrivances for the more convenient drawing and filling of water.

Directly after the meeting on the 12th of May *Ørsted* caused the above-mentioned journal to be begun, and he opened it with the programme of the work now to commence. The journal was chiefly written by the assistant Mr. *Foch*, it simply described from day to day what work had been done, when apparatus had been ordered,

[1] Ed. Vol. II. P. 472. [2] Ed. Vol. II. P. 325. [3] Ed. Vol. II. P. 402.

when they had been delivered, what accidents happened, what deficiencies were found and how they were remedied, what expenses had been paid etc. Furthermore the observations from the individual experiments and the preparatory measurements for these are set down. From this it is seen that much time has been given to the weighing of piezometers and tubes for pressure measurement. It was chiefly the compressibility of liquids which was investigated. The plans of the programme for the examination of gases were temporarily laid aside. Out of the liquids employed water was subjected to most measurements, and these were made at different temperatures. If we look at the direct measurements of the compressibility of water set down, we receive no impression of any closer agreement between the repetitions than in the experiments previously carried out.

The compressibility of mercury was also found through several series of carefully made measurements. — In a letter to *Brewster* dated Dec. 30th 1826 *Ørsted* states the results which in his opinion might with certainty be derived from the experiments he had hitherto carried out.[1] He begins with the information that during the past summer he had carried out a great number of experiments and that he was now calculating corrections for variations in the pressure and temperature of the atmosphere. As soon as these calculations should be completed a paper on the experiments would be finished, and *Brewster* would receive a translation of it. This paper, as mentioned above, was never written. Hence it is of all the more interest that in the letter to *Brewster* as well as in the communication[2] in the Proceedings of the Society of Sciences 1826—27 he ends by recording the results which must be supposed to be only slightly influenced by the corrections wanting. These, taken in the order in which they are found in the letter to *Brewster*, are as follows: —

1) For a pressure of up to 70 atmospheres compressibility of water is proportional to the pressure, and for an increase of pressure of one atmosphere may be put at 45 millionths.
2) Heat is not liberated by the compression.
3) The compressibility of mercury is slightly above one millionth of its volume by an increase of pressure of one atmosphere.

[1] Ed. Vol. II. P. 335. [2] Ed. Vol. II. P. 472.

4) The compressibility of ether is nearly thrice, that of alcohol nearly twice, that of sulphuret of carbon only one and a third that of water.[1]
5) The compressibility of water containing salts, alkalies, or acids, is less than that of pure water.

In the paper in the »Proceedings« which is of later date than the letter, the following has been added: —

6) The compressibility of water is not the same at all temperatures, but is greatest at the lowest temperatures.
7) The compressibility of glass has been found to be less than that of mercury.

The fourth item has here been redrafted. By the translation from Danish to English an error must have crept in which has been repeated in a German rendering.[2]

That the here cited reading is the right one will be seen from *Ørsted's* figures for the compression coefficients for the said fluids. These are[3]

water $45 \cdot 10^{\div 6}$
1) ether $140 \cdot 10^{\div 6}$ = abt. 3 times that of water.
2) alcohol $97 \cdot 10^{\div 6}$ = abt. 2 times that of water.
3) CS_2 $53 \cdot 10^{\div 6}$ = abt. $1\frac{1}{3}$ that of water.

We get a notion of how carefully the experiments were carried out through the fragment of a manuscript[4] found among his papers. Its wording and beginning allow of the conclusion that it contains an account, given at a meeting of the Society of Sciences, of the experiments carried out during the summer of 1826. Since now it appears from the minutes of the Society that *Ørsted* brought this before a meeting on the 7th of January 1827, the manuscript without doubt contains what he wrote on that occasion. It has interest partly by showing the form that *Ørsted* gave to such an address to the Society of Sciences, partly by giving the detailed description of the improvement of the compression apparatus which was never published, and partly by describing the interesting and difficult experiments on the compressibility of mercury.

In order to judge of the value and accuracy of *Ørsted's* experiments we shall compare the above-mentioned 7 points with the results of much later times, these seven being in the main his

[1] Ed. Vol. II. P. 336 note. [2] Ed. Vol. II. P. 336 note.
[3] *H. C. Ørsted:* Naturlærens mekaniske Del. Cpn. 1844. [4] Ed. Vol. II. P. 325.

final results in this domain. As we shall see later on, a criticism of his method appeared in 1827 which he set himself to refute; then his work was stopped for some years, was taken up again at certain intervals with various assistants, but was never carried through to the end. —

The chief results agree with those of a later day.

That the compressibility of water is proportional to the pressure, was confirmed by the measurements of *Cailletet*[1] in 1872, when a method similar to *Ørsted's* was employed, but at pressures up to 700 atmospheres. It was only when *Amagat*[2] continued the experiments to far greater pressures, and with more accurate measurements of these, that the compressibility was shown to diminish with the pressure.

In regard to the numerical value of the compressibility for water *Cailletet* found by *Ørsted's* method at 8^0, 45 millionths of the volume by an increase of pressure of 1 atmosphere, whereas *Ørsted* states this value to hold for 10^0.

Ørsted later on changed his former opinion that the compression evolved no heat.

The determination of the compressibility of mercury is exceedingly good. In experiments which are carefully reported in the before-mentioned manuscript[3] of Jan. 7th 1827 0·00000122 is given as the mean of three series of experiments. By the same method *Regnault* got 0·000001145 and *Amagat* 0·0000015. But though the measurements were thus good, the result remained valueless as it ought to have been corrected by a quantity double the measured value in order to give the real compressibility of mercury. The necessity for this correction was first pointed out a year after by French scientists, and right down to our own day discussions and measurements have turned upon this quantity.

The results in point 4 with reference to the compressibility of ether and alcohol have the proper sequence.

It is correct — at any rate found later by *Grassi*[4] — that the compression of salt solutions is smaller than that of water.

That the compressibility of water is reduced with rising temperature is correct for the temperatures at which *Ørsted's* measure-

[1] Comptes rendus. Vol. 75. P. 77. Paris 1872.

[2] Comptes rendus. Vol. 103. P. 429. Paris 1886. Vol. 104. P. 1159. 1887. Vol. 105. P. 1120. 1887.

[3] Ed. Vol. II. P. 329.

[4] Ann. de chim. et de phys. Vol. 31. (3 ser.) Paris 1851. P. 437.

ments were made, and this fact was first found by him[1] at a more extensive series of temperatures, though *Grassi* has been mentioned as the first person to observe this. The reason is of course that *Ørsted,* as in other cases, did not publish the observations by which the fact was borne out.

Point 7. That the compressibility of glass is smaller than that of mercury is a correct observation.

While *Ørsted* was still occupied with his experiments on compressibility, a paper by *Colladon* and *Sturm* on the compressibility of fluids[2] appeared in 1827 in answer to a prize question proposed by the French Academy. These scientists used *Ørsted's* method in their experiments, only omitting the string of mercury for confining the fluid in the piezometer, which as we know had also long ago been given up by *Ørsted.*

Whereas *Ørsted* supposed that the volume of the piezometer was not altered by the compression, the pressure being equal from within and from without, *Colladon* and *Sturm* contended that the internal volume of the piezometer must vary in the same way as if it were massive glass. If we call the volume of the fluid to be compressed V, the increase in pressure P, the observed reduction in volume $\triangle V$, the compressibility coefficient of the fluid β, that of the glass C_k, we should have: —

$$\triangle V = \beta . V . P \div C_k . V . P$$

$$\frac{\triangle V}{V} = P\,(\beta - C_k), \ \frac{\triangle V}{V} \text{ for } P = 1 \text{ atmosph. is called } \omega$$

$$\beta = \omega + C_k.$$

Ørsted had observed and indicated the value ω; in order to obtain the real compressibility coefficient the correction C_k ought to be added. The question was now how to find C_k. *Colladon* and *Sturm* calculated it from observations on the stretching of glass rods. The stretching coefficient for a glass rod being called C it was supposed that

$$C_k = 3\ C.$$

C being found to be 0·0000011 the correction would be

$$C_k = 0{\cdot}0000033,$$

which was thus to be added to the observed value of ω.

---

[1] *Canton* had found the same for temp. 15° and 4°.

[2] Ann. de chim. et de phys. 36. P. 113. 1827.

Ørsted replied to this, partly in *Poggendorff's* Annalen in a letter[1] to the editor, partly in the Proceedings of the Society of Sciences[2] for 1827—28. Already on the 25th of April 1828 he reported to the Society »on some fresh experiments by which he refutes the opinion recently advanced by *Colladon* and *Sturm* that the vessels in which water is compressed by his method are reduced in cubic capacity.« First he maintained against *Colladon* and *Sturm* that long ago he had given up the obstruction with mercury in the piezometer, and next he turned to the value of the applied correction C_k. He contended that it must be wrong to calculate the reduction in volume of a solid body by pressure, or the increase in volume by stretching, in the simple way that *Colladon* and *Sturm* had employed, putting it equal to three times the alteration in length, as by stretching and compression of a rod its cross section will alter, from which follows that the alteration in volume is not so simply dependent on the alteration in length. He stated his intention of deciding the matter experimentally by trying to find C_k directly for glass. In order to do so he filled a piezometer with pieces of glass, filled up with water and placed the whole in the compression cylinder, he then observed the collective reduction in volume of the water and the glass in the piezometer and from this could calculate that of the glass. His results showed him that C_k is smaller than it should be according to the calculation of *Colladon* and *Sturm*, but they were not sufficiently certain nor in sufficient agreement to afford any numerical value. In the meanwhile he found another way of showing that *Colladon* and *Sturm's* argument must be wrong. He compressed water in a leaden flask, the mouth of which was furnished with a glass tube; for lead it was known that $C = 0{\cdot}00002048$. According to the calculation of *Colladon* and *Sturm*, C_k should then equal $0{\cdot}00006144$. Since now the compressibility of water, according to all measurements taken, is less than this quantity, the water should rise in the neck of the leaden flask by the compression if the theory were correct, whereas, on the contrary, it sank a little more than if the flask were of glass, so that the compressibility is two millionths more by this determination than in the glass piezometers. By this *Ørsted* thought he had proved that *Colladon* and *Sturm's* correction was wrong.

[1] Ed. Vol. II. P. 348. [2] Ed. Vol. II. P. 476.

He now let this matter rest for some years. It seems remarkable that during these years he should entirely give up work which he had begun with such energy and eagerness, it would appear as if *Colladon* and *Sturm's* paper had cooled his interest. Certainly he was much occupied in 1829—30 with the foundation of the Polytechnical School, still he carried on other experimental researches, indeed, on the 23rd of January 1829 it is recorded in the minutes of the Society of Sciences that »the Secretary proposed that some of the 500 Rbd. which the Society has granted for experiments on compression should be employed for experiments on producing great magnetical effects by galvanism.«[1] In 1830—31 he constructed an apparatus for the measuring of depths of the sea[2] for which he used an altered form of his piezometer.

In 1832—33 he again returned to the researches on compression, and a report on their results is found in the Proceedings of the Society of Sciences[3] for that year, and in »Reports of the British Association«[4] for 1833; this latter paper contains the sole published complete description of his apparatus in the shape it had after 1826. In this treatise he again takes up the question of *Colladon* and *Sturm's* correction and gives an account of a number of experiments with glass pieces in a large piezometer employed in the way described above; the experiments were extended to the examination of other solids than glass. According to these experiments C_k for solids — and especially for glass — would be so small that it was of the order of magnitude of the experimental errors of the measurements.

For the rest the object of the treatises is to find an explanation of the irregularities in the numerous determinations of the compressibility of water, against which even the latest improvements in his apparatus could not guard him in spite of all the practice of the experimenter. A journal of experiments from these years shows that a lot of measurements were made.

Ørsted then got the idea that the irregularities were due to the circumstance that heat was evolved by the compression, so that water, for each increase of pressure of one atmosphere, rises $^1/_{40}{}^0$ C. in temperature. This would then, by temperatures above 3·075, by which *Ørsted* assumed the density to be a maximum, apparently

[1] Ed. Vol. II. P. 478. [2] Ed. Vol. II. P. 482. [3] Ed. Vol. II. P. 485.
[4] Ed. Vol. II. P. 402.

diminish the compressibility, and by temperatures below 3·075 increase it, and, the expansion coefficient of water being different at different temperatures, the apparent compressibility would vary irregularly with the temperature. *Ørsted* then put forward the hypothesis that the compressibility of water was in reality the same by all temperatures, namely 46 millionths, which had been found by 3·075, and he then tried to calculate what the observation ought to give at a series of other temperatures by adding to or subtracting from this value $^1/_{40}$ of the expansion coefficient by the temperature in question, and also by correcting for the expansion of the piezometer by the same assumed heating. In this way he succeeded in establishing agreement between hypothesis and experience by a series of temperatures.

Ørsted's idea, that heat was generated by the compression, touches on a problem of far-reaching importance which scientists in 1832 had no means of solving otherwise than by direct measurement, a method which he tried to use some years later. The principle of the conservation of energy had to be found, and the second law of thermo-dynamics to be recognised, before it could be predicted that such an evolution of heat must take place, and before its numerical value could be calculated. The rise in temperature caused by an isentropic compression may be found by means of a formula first advanced by *W. Thomson* in 1858; it may be written:

$$\left(\frac{dT}{dp}\right)_{Q = \text{constant}} = \frac{T.V_0.\alpha_t}{C_p}$$

where T is the absolute temperature by which the compression takes place, V_0 the initial volume of a unit of weight of the substance compressed, C_p the specific heat mechanically measured, α_t the expansion coefficient and dp the increase in pressure. From this it will be seen that dT can be found for an increase in pressure of one atmosphere when the expansion coefficient, the specific heat, and the density of the substance at the experimental temperature are known. If we carry through the calculation for water at 25^0 we get,[1]

[1] Ann. d. Physik und Chemie. Neue Folge. Bd. 20. 1883. P. 882 et seq., in a treatise by *I. Drecker:* Ueber die innere Ausdehnungsarbeit von Flüssigkeitsgemischen im Vergleich zu derjenigen ihrer Bestandtheile.

$dT = 0{\cdot}001849^0$; $\alpha_t \cdot dT = 0{\cdot}00025 \,.\, 0{\cdot}001849 = 0{\cdot}4622 \,.\, 10^{\div 6}$ then gives the correction to be added to the compressibility coefficient found at 25^0; it is only about 1 per cent of its value and is thus without significance in comparison with the errors in *Ørsted's* experiments. For other fluids the matter wears another aspect; the corresponding percentage magnitude of the correction is for[1]

CS_2	abt. 50 per cent.
Ether	- 30 per cent.
Alcohol	- 18 per cent.

Ørsted's compression method is markedly isentropic, the experiments being carried out in as short a space of time as possible in order to avoid the transition of heat from the experimenter and other surroundings. Hence it is obvious that the lacking correction for the generation of heat plays so great a part with other fluids than water that the results for these must turn out much less exact. As might be expected, the figures are too low. Thus *Ørsted* found for

Alcohol	$93.10^{\div 6}$
CS_2	$53.10^{\div 6}$

whereas *Drecker,* at a slightly higher temperature found for

Alcohol	$113{\cdot}8.10^{\div 6}$
CS_2	$97{\cdot}5.10^{\div 6}$.

If the error were to be avoided the experiments must be performed in such a manner that the change in volume caused by the compression was not read until after so long a time had passed that the rise in temperature had disappeared. This method was used e. g. by *Amagat,* and also by *Drecker* to whose paper we referred above. After 1839 *Ørsted* tried whether it could be shown by direct measurement that a change in temperature took place during the compression. In this work he was assisted by a man who became one of the pioneers in the research through which the principle of the conservation of energy came to be recognised — cand. polyt. *Colding.*

From an experimental register it is seen that in the summer of 1837, from May to some way into October, experiments have again been carried out on the compressibility of water, and values have been found at somewhat varying temperatures, but the work was not finished. So in the winter of 1839 *Ørsted* applied to the Society

[1] Ann. d. Phys. u. Chem. Vol. 45, 1892. P. 560. *W. C. Röntgen*: Ueber den Einflusz der Compressionswärme auf die Bestimmungen der Compressibilität von Flüssigkeiten.

of Sciences for 150 Rbd. for completing his researches on the compressibility of water. He writes: »It is my wish now to put the last touch to this work in order to give a comprehensive treatise on compressibility, for which I have most of the material at hand in a very explicit diary of the experiments. But for the completion some more experiments are necessary as well as a great number of calculations which, though easy, are extensive and wearying. I fear that for yet a very long time I shall find myself prevented from completing the work if I do not let somebody else make these calculations. For this I intended to solicit the help of the Society«.[1] In *Ørsted's* papers we can see that *Colding* had began the calculations and also, that the experiments had been taken up again and brought to a certain degree of completion by *Colding*. Their aim was a direct demonstration of the generation of heat by compression by means of a thermo-couple, one junction of which was fused into the bottom of the compression cylinder. *Colding* wrote a small treatise on the result which is found among *Ørsted's* papers, but he did not get to any decisive result and so *Ørsted* has probably not been willing to publish it. The only mention made of this work is in the Proceedings of the Society of Sciences[2] for 1845 in a communication of one page. This is the last from the hand of *Ørsted* on the compression researches, it states that a series of direct measurements of variations in temperature during the compression of water have shown that when the pressure increases with one atmosphere the temperature of the water rises $\frac{1}{49{\cdot}2} = 0{\cdot}02^{0}$ C. He regards the result as being beyond doubt, still he will try to make it certain by a few more series of experiments when the season gets colder. About these no more is ever heard and, as shown above, the numerical result of the measurement is too high.

It is evident that it was the omission of the correction for the reduction in volume of the piezometer during the compression which obscured the significance of *Ørsted's* figures, and caused him to lose interest in completing the experiments because he could not quite definitely make up his mind upon this matter.

[1] Minutes of the meetings of the Society of Sciences 15th Febr. 1839.

[2] Ed. Vol II. P. 527 (with a supplement from *Ørsted's* papers p. 528).

He does not actually write anywhere that the correction is unjustifiable, but he contests the manner in which it is calculated and maintains that not theory but direct observation must decide how large it is to be, and his own attempts at a direct determination led to no decisive result. It is of some interest to observe how difficult the whole question is. *Ørsted*, as we see, has rightly contended against *Colladon* and *Sturm* that the assumption $C_k = 3\,C$ is incorrect. If μ represent the ratio between the change in a unit of length perpendicular to the stretching direction of a rod and in this direction, we obtain the now generally known relation

$$C_k = 3\,C\,(1-2\mu).$$

It was shown already in 1827 by *Cagniard de la Tour*, and later by others, that $C_k > 0$ for most substances, from which it follows that $\mu < {}^1/_2$. *Poisson* thought he could prove that μ must be the same for all substances and equal to ${}^1/_4$. *Wertheim* maintained that $\mu = {}^1/_3$; *Stokes* and many others thought that μ was not the same for all substances and finally, in 1889, *Amagat* measured C_k for various glasses and other substances by compressing rods of them in water just as *Ørsted* had tried to do. *Amagat* found that the value of μ was dependent on the substance. If we get $\mu \gtreqless {}^1/_3$ we get $C_k \lesseqgtr C$. According to *Amagat's* measurements the value of C_k for lead should be slightly different from the value for glass, hence the apparent compression of water should be about the same in a glass flask as in a leaden flask, as *Ørsted* had found it. *Regnault* reckoned with a correction of $C_k = 0{\cdot}000002374$ when using a glass piezometer, *Amagat* found a slightly greater value for seven glass piezometers; *Ørsted* was thus right in asserting that the correction was of the same order of magnitude as the errors in his determination of the compressibility of water.

Thus we see that *Ørsted* had reason to look with distrust upon *Colladon* and *Sturm's* correction; also that the problem thus given rise to was of an extremely difficult nature; and that *Ørsted*, in order to solve it, suggested a method which later, in *Amagat's* experiments, determined the size of the correction. That he did not employ any correction at all diminished the significance of the figures which his experiments had given him, but his method has been fundamental for all later work in this domain and the accuracy of his direct measurements has justified the confidence placed in his method.

The years from 1815 to 1829 proved, scientifically, the most fruitful in *Ørsted's* life. Not only did his electromagnetic discovery mark an epoch, but, as we have seen, his other researches during this period gave results of universal importance. At the same time he rose in esteem among his countrymen. His work in the service of the community was not diminished and his interest in this work gradually increased; thus, in order to promote general education he founded, in 1824, Selskabet for Naturlærens Udbredelse (Society for the Propagation of Sciences), whose organisation was entirely based on great personal activity on the part of its founder who retained the leadership till his death. In 1829 the Polytechnical School was founded on *Ørsted's* initiative, and the great task of its organisation as well as its continued management fell to him who was the principal from its foundation and for the rest of his life. His work as Secretary to the Society of Sciences also increased, partly because the general sphere of action of the Society was enlarged, and partly because it turned to new activities under the very influence of its secretary. Among these may be mentioned Artesian borings, a magnetical observatory, and a meteorological institute, all claiming his thoughts, often his work, and in both cases his time.[1]

The result was that *Ørsted,* during the last 20 years of his life, found only little and disconnected time for scientific work, and was thus diverted from such systematic experimental research as requires a connected time for its completion, whereas his philosophical and esthetic interests, which he had never entirely deserted, became more predominant, their cultivation being more easily combined with his other work. These interests united with his zeal for popular education, found encouragement and appreciation in his intellectual and estheticising circle and led to a fairly extensive production.

Yet he continued to occupy himself with experimental work, but more with such as comes within the scope of the inventor and the teacher than within that of the scientist.

---

[1] Ed. Vol. III. P. IX. *Kirstine Meyer: H. C. Ørsted's* Arbejdsliv i det danske Samfund (*H. C. Ørsted's* Varied Activities in the Danish Community).

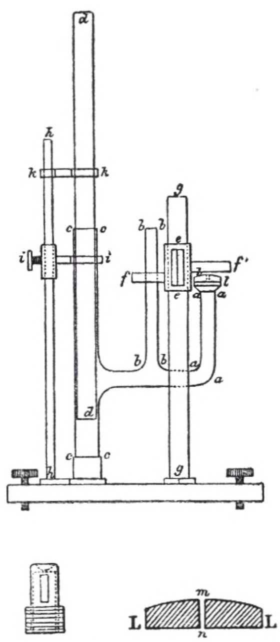
From reports in the Proceedings of the Society of Science and from notes to his monthly lectures we see that he kept abreast, theoretically and practically, of new departures in his science, as requisite in a teacher of the young, and that he tested new experiments and made independent use of them, often suggesting alterations and improvements in the apparatus employed. Further we find him interested in physico-technical experiments as was only natural in the principal of a school for applied science. Thus in 1831 he constructed an apparatus for measuring depths of the sea,[1] a variation on his piezometer, and in 1842 we find him working at the problem of electroplating.[2] He studied *Gauss's* methods of magnetic investigation in 1834—35,[3] established a magnetical observatory at the Polytechnical School and carried out measurements there. In his papers we find proposals for the construction of an automatic metal thermometer for registering atmospheric temperatures, and for new hygrometers. The principle of the latter is that two sheets of metal, riveted to form a hoop, will be able to change the position of an index when one is cooled by water evaporating from its surface while the other, being dry, and isolated from the first, retains the temperature of the atmosphere. He served geology by registering the temperature at various depths in the bores of Artesian wells.[4] He invented an instrument »designed merely for trade« to measure the thickness of glass in silvered mirrors.[5] The apparatus is merely sketched in the »Proceedings« (1844). The idea is simple and neat, though the principle undoubtedly shows to more advantage on paper than in reality.

Reports on a couple of instruments he had designed were made by *Ørsted* at the Meetings of Scandinavian Scientists of which the first took place at Gothenburg in 1839. He was an eager participator in these meetings, and his universal reputation as a scientist and his whole position in the community and in the social world, combined with his eloquence, contributed to render him a conspicuous figure there.

At Gothenburg he exhibited an apparatus for examining the capillary action[6] in metal tubes whose opacity prevented the usual method of reading the difference in height between the fluid in a capillary and in a wider tube. The main feature of the method was

[1] Ed. Vol. II. P. 482. [2] Ed. Vol. II. P. 501. [3] Ed. Vol. II. P. 488.
[4] Ed. Vol. II. P. 505. [5] Ed. Vol. II. P. 523. [6] Ed. Vol. II. P. 413 & 497.

the use of three fairly wide intercommunicating glass tubes one of which carried a ring, ground level, and was closed at the top by a metal plate fitting on to the ring and having a very narrow channel in the middle serving as a capillary. The tubes were filled with liquid, and by a piston in one of them the liquid was made to rise partly into the capillary and partly into the third tube which served to measure the height needed to press the liquid out of the upper mouth of the capillary, or the depth to which it must be made to fall by the aid of the piston in order to detach the fluid from the lower edge of the capillary. The apparatus has doubtless many weak points, it was never used by others and very little by *Ørsted* himself.



At the Meeting of Scientists in Copenhagen in 1840 he exhibited a »new electrometer«,[1] a modification of *Coulomb's* torsion balance and undoubtedly a very sensitive electroscope, but not actually an electrometer, the interdependency of the deflection and the charge of the adjustable rod and of the fixed charged conductor being very complicated; nor has this apparatus obtained any practical significance. Electrical experiments occupied him a good deal from 1840 to the time of his death as may be perceived both from his printed works and his papers — in these latter we find among various other things one or two proposals for new electric multipliers. One of these is interesting, it runs as follows:

»A metal wire bent as in a multiplier and able to revolve easily round two points is placed opposite the poles of a strong magnet in such a way that it will be deflected as soon as it is traversed by electr.« This, then, is the *Deprez-d'Arsonval* galvanometer already foreshadowed here.

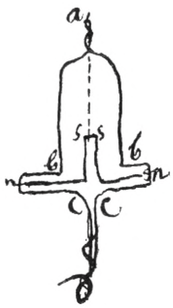
Another proposal is found on the same sheet of paper:

»*Febr. the 13th 1845*«

»Might not an excellent multiplier be made in the following way [Fig. P. CLIX]. *n s s n* is a magnetic needle in which *s s* have

[1] Ed. Vol. II. P. 411 & 499.

south-seeking, *n* and *n* north-seeking magnetismus. It is suspended by means of silkworm fibre. *a b c d* a conductor made of two copper wires. As the needle may easily be magnetised so as to have practically no directive force, the conductor must exert great influence on it. It must inter alia be excellent for very slight thermo-electric effects.« Whether this proposal was ever carried into effect we do not know.



Once more we find *Ørsted* occupied with the improvement of the galvanical cell. *Grove* had constructed his wellknown constant galvanical cell, the precursor of *Bunsen's,* with a platinum rod in concentrated nitric acid for the positive pole. *Ørsted* substituted a platinum-coated china cylinder for the platinum rod;[1] by this substitution the cell was made cheaper and thus more suited for general use.

In 1847—49 he made his last, extensive experimental investigations on the subject of diamagnetism. In 1845–46 *Faraday* had published his first works on this hitherto unknown phenomenon, and at a meeting of the Society of Sciences on the 23rd of April 1847 *Ørsted* demonstrated these experiments as well as *Faraday's* experiments on the rotation of the plane of polarised light in a magnetic field. The collection of physical instruments at *Ørsted's* disposal contained a very large electromagnet, so a sufficiently powerful field for exhibiting these sensational phenomena could be obtained. In the course of the summer of 1847 *Ørsted* examined the power of the electromagnet under various circumstances by registering the variations in its carrying capacity according to the number and combination of the cells providing the magnetising current, or the shape and size of the anchor.[2]

Finally, on the 30th of June 1848 he reported on a more extensive series of investigations[3] on diamagnetic phenomena before the Society of Sciences, and these experiments were continued and supplemented[4] by him in the following year. His results were first published in a French memoir printed in Copenhagen and then in the Annales de Chimie et de Physique from which they were translated into German.

For these experiments the before-mentioned large electromagnet was used, with pole plates which in some cases were cylinders, in

[1] Ed. Vol. II. P. 500. [2] Ed. Vol. II. P. 552. [3] Ed. Vol. II. P. 419 & 568. [4] Ed. Vol. II. P. 574.

others flat pieces of iron of 0·09 m. width and 0·026 m. thickness, hence in both cases suited to produce non-homogeneous fields. The observations that most attracted his interest were the following: A small diamagnetic needle, which adjusted itself »equatorially« between the pole planes in relation to their connecting line, tended, when placed above their edges, to take a direction parallel to the latter, and thus formed a right angle with the position it took up between the pole planes, whereas a slightly magnetic body, when between the poles, tended to place itself in the direction of their connecting line, but when above the edges of the pole planes, at right angles to this line, i. e. took up positions at right angles to those of the diamagnetic bar in the same circumstances. A small iron bar, on the other hand, retained its direction along the connecting line, even when raised above the edges of the pole planes.

In consequence of these observations *Ørsted* divides all bodies magnetically into 3 groups: the repellably diamagnetic, the attractably diamagnetic, and those that behave like iron. This division answers to that introduced in modern times: the diamagnetic, the paramagnetic and the ferromagnetic; in a non-homogeneous field the diamagnetic particles will seek the weakest points of the field, the paramagnetic particles the strongest. These points are, however, independent of the direction of the lines of force, and there is nothing to prevent the construction of magnetic fields in which paramagnetic bars are at right angles to the lines of force while the diamagnetic are parallel. Such fields, then, were used by *Ørsted*, and his observations are explained by the rule given above. *Faraday* had previously seen the same thing for diamagnetic bodies and explained the apparently strange phenomena by the irregular character of the field in connection with »the ruling principle that each particle tends to go by the nearest course from strong to weaker points of magnetic force.«[1] But the difference between the positions of paramagnetic and diamagnetic bars in a non-homogeneous field was first mentioned by *Ørsted*, though his observations on this point have passed unnoticed; they show, however, that he observed something new and essential also in this last, more extensive, experimental research.

In one or two other fields he commenced investigations men-

[1] *M. Faraday:* Experimental Researches in Electricity. (Dec. 1845). Vol. III. P. 42. London 1855.

tioned partly in the Proceedings of the Society of Sciences partly in short communications made in foreign periodicals with a view to inducing others to take up the same work. One was the investigation of the changes of mercury in airtight glass bulbs;[1] such changes were observed by him to take place in some glass bulbs and not in others. He began to examine the influence of various glasses and the possibility of demonstrating a change in weight for each observed alteration. This work was never finished.

In 1848 *Ørsted* occupied himself to some extent with experiments in radiant heat according to *Melloni's* method, altering and improving the apparatus in various ways. The results of these researches were mentioned in the Proceedings of the Society of Sciences[2] and they are characterised by a certain completeness; radiations from the heated surfaces of liquids and from heated currents of air were measured by comparing them with the radiant heat from blackened iron of the same temperature. Only few and uncertain numerical results are given.

Thus it seems as if *Ørsted* took up experimental work with renewed interest in 1848—49 in spite of his advanced age. When Denmark was involved in war in 1848 his inventive faculties were brought into play, and we see the same as we have witnessed in 1914—18 that war induces a desire for new methods of destruction and new means of communication. In a letter[3] to Lieutenant-Colonel *Fibiger* dated March 30, 1848, *Ørsted* first alludes to the fact that about 30 years earlier he experimented on the galvanical firing of mines and then goes on to say:

»I now beg to draw fresh attention to this matter, not nearly so much on account of the actual firing of the mines, of which perhaps no use will be made at all in the impending war, as on account of a similar use, an application on a small scale of which I spoke with some experts already some time ago, namely, of burying in a road to be taken by an attacking enemy, under a comparatively thin layer of earth, small reservoirs filled with gunpowder and earth or fragments of stones, which could be fired by a communicating wire on a given signal, and that in a shorter time than one second after the signal. A few pounds of powder would, in spreading destruction among an advancing enemy, effect further confusion.«

From the two following letters it appears that he occupied him-

[1] Ed. Vol. II. P. 525. [2] Ed. Vol. II. P. 565. [3] *H. C. Ørsted's* papers. B. U. H.

self with the communication of sound signals — »telephoning« — through water. The first letter is addressed to Count *Rosen* and is written in April 1850.

»As agreed yesterday I cannot neglect to inform you of the result of the telephonic experiments I caused to be made yesterday. They were entirely satisfactory. The larger of the two bells, which does not, however, weigh more than a hundred pounds, was heard with perfect distinctness even at the greatest distance to which the experiments were carried. That distance was certainly only half a [Danish] mile, but at that distance the sound was almost as loud as close by. Before long the experiment shall be made at greater distances.«

Three days after he wrote to the postmaster-general, Count *Danneskjold Samsøe*, applying for 100 Rdl. in addition to the 100 Rdl. already granted for telephonic experiments. Then comes the following passage: —

»On this subject I have made some experiments. The first took place in one of the water reservoirs of the town under the ice. The sound was heard under the water at various distances, at last as far as 2400 feet. Later I have had experiments made at sea with a larger bell, the weight of which is, however, below 100 pounds. The sound of this bell was heard under the water at various distances which time and circumstances did not permit us to carry further than to half a [Danish] mile, but at that distance the sound produced by the bell was still heard so perfectly and with so little loss of strength that it was highly desirable to push the experiments to greater distances. When now two days ago we wanted to carry out these more extensive experiments and for this purpose had obtained the loan of a small steamer from the Minister of Naval Affairs, in order to carry out the bell to all the distances desired, the big receiver procured for the experiments was accidentally damaged. This has happened once before and necessitated considerable repairs, the new repairs now again required will further increase expenses. . . . . The prospect of a successful result is so far from being diminished that on the contrary it has been increased by the experiments. Should this undertaking be successful, we shall obtain in telephony a means of communication through water which will be much less expensive than the electromagnetic telegraph.« Of the further result of the experiment no communication was made.

While *Ørsted's* experimental work and production had thus in the latter years of his life a casual and desultory character, he was at the same time a consistent and fertile philosophical author. He obtained a wide circle of readers both in his own and other countries and his philosophical production had thus to a certain extent the effect of diverting the general attention from his works on physical science. He brought the facts and laws of science to bear upon philosophical questions and thus associated his philosophy with science. He assigned to these works the mission of contributing to show »in a clearer light the significance of the sciences for general education.« In his preface to the poem »Luftskibet«[1] (The Airship) (1836) he laid great stress on this purpose: »I am thoroughly convinced that the sciences ought to constitute an essential part of general education, and I should like to contribute to this result as far as I am able . . . . We are all agreed that the sciences should be cultivated. Their immeasurable usefulness recommend them to the general public. Those who have more insight also acknowledge the great effect the sciences must have both on the development of the power of thinking and the power of conceiving and imagining, but as yet no serious attempt has been made to connect them with those thoughts and feelings which move mankind most, and which in my belief might be both expanded and purified by the sciences. . . . . Amongst other things the sciences would seem able to a great extent to act as a guide to us in the investigation of the nature of the beautiful«. He embodied this thought in a series of papers on »Det Skjønnes Naturlære« (The Natural Philosophy of the Beautiful). He had occupied himself with this question at the time of his researches on acoustical figures, and as early as 1808 he wrote a dialogue »Over den Fornøjelse Tonerne frembringe« (On the Pleasure Produced by Music) containing most of the ideas which were advanced and more exactly formulated by him in his later years. In the winter of 1845 he lectured on the »natural philosophy of the beautiful« before the Society for the Propagation of the Sciences, but already in the previous years he had occupied himself with the subject. He published two dialogues on this sub-

[1] Saml. og efterl. Skrifter of *H. C. Ørsted*. Vol. 4. P. 99. Kbhvn. 1851.

ject in 1839[1]; lectured on it at the Meeting of Scandinavian Scientists at Stockholm in 1842; he wrote papers in the Proceedings of the Society of Sciences[2] in 1842 and 43 on the phenomena in optics on which he had based his conclusions; he made brief mention of the results in his Mechanical Physics (1844)[3]; he incorporated these results in a lecture given at the Meeting of Scientists at Kiel in 1846;[4] and supplemented them in his last communication to the Society of Sciences in 1850 — in short, the subject occupied his mind for many years.

I shall now endeavour to give in brief the line of thought evidenced in that part of the philosophical production of his later years, which he himself classed as science, but which has rather had the effect of rendering conspicuous the philosopher *H. C. Ørsted* at the cost of the scientist of that name. The same train of reasoning forms the continuous thread running through his popular philosophical writings.

The phenomena producing impressions of beauty in us reach our consciousness through sight or hearing; we must examine whether these things giving the feeling of beauty have any features in common. The result is: »we receive impressions of beauty from that which is in harmony with reason«. But what are we to understand by »that which is in harmony with reason?« Examples taken from the domains of mathematics and the sciences give us the answer: that which is in harmony with reason is what is governed by simple mathematical laws. Thus, through sight we receive impressions of beauty from forms outlined by simple mathematical curves and planes, especially from such in which symmetry reigns, and through the ear we receive impressions of beauty from tones, which can produce the sonorous figures of the forms mentioned above. The feeling of beauty produced by colours and various degrees of light may likewise be referred to phenomena following simple mathematical laws, hence »attuned to reason« or modelled on »natural thoughts«. This does not, however, mean that the person who receives an impression of beauty from some object is conscious of this fundamental »harmony with reason«. »Beauty pleases us as the impression of an idea of which we are not,

[1] Saml. og efterl. Skrifter. Vol. 3. P. 67 seq. Kbhvn. 1851. [2] Ed. Vol. II. P. 506 & 509.
[3] Naturlærens mekaniske Del. Kbhvn. 1844. P. 347 seq. [4] Saml. og efterl. Skrifter. Vol. I. »Aanden i Naturen«. I. P. 125 seq. Kbhvn. 1851. Ed. Vol. II. P. 545.

however, at the same time conscious.«[1] »The idea is the intuitively perceived unity of thoughts;.... it seems to us as if all the thoughts we had found in it now float across our mind in the shape of memories and form a complete impression.«[2]

The question is now raised »how can the reason latent in all objects have the powerful effect on us which is produced by beauty?«[3] This is »answered by the fact that our whole sensual nature is built according to the same fundamental laws as our intellectual nature.« Man is in possession of an »inner sense« »so framed according to the laws of reason in the rest of the world, that it derives satisfaction from whatever bears the stamp of reason, though the enjoyment does not necessarily imply any consciousness of this reason.«[4] There are, however, rather narrow limits to the forms able to affect the »inner sense«; »our inner sense is unable to comprehend the character of other than the simplest thoughts; in so far as the inner sense especially conceives intuitively, figures expressing the simple thoughts, symmetry even of very complicated forms, shades of light and proportions of colour, sound motion (rhythm), the simple harmonies and movements in music will be its chief objects.«[5]

We must then again ask if we can find any cause for this harmony between the simple laws valid for the phenomena and the inner sense. To this *Ørsted* replies: »The laws of nature in the bodily world are laws of reason, the revelation of one reasonable will; if thus we figure to ourselves the whole bodily world as the continual work of eternal reason, we cannot abide by the consideration of this, but are carried on to perceive in our thinking too the same laws of the universe. In other words, spirit and nature are one, viewed under two different aspects. Thus we cease to wonder at their harmony.«[6] Here we see *Ørsted's* idea from his early years, of a unity in the forces of nature, applied, in the last

[1] Om Grunden til den Fornøjelse Tonerne frembringe. *H. C. Ø.* Saml. og efterl. Skrifler. Vol. 3. P. 85. Kbhvn. 1851.

[2] l. c. P. 85.

[3] Om Symmetrien og de ved den frembragte Skønhedsindtryk. Saml. og efterl. Skrifter. Vol. 3. P. XIV.

[4] Hele Tilværelsen eet Fornuftrige, speech at the Meeting of Naturalists at Kiel, 1846. Saml. og efterl. Skr. Vol. I. P. 148. The Soul in Nature by *Hans Chr. Ørsted* translated from the German by *Leonora* and *Joanna B. Horner.* London 1852. P. 110.

[5] Ed. Vol. II. P. 584.

[6] To Capitler af det Skjønnes Naturlære. Saml. og efterl. Skrifter. Vol. 3. P. 169.

days of his old age, to the idea of the religious unity of spirit and nature. His last unfinished work, »The Way from Nature to God«, dealt with this subject. Full of plans for the continuation of these works he died on the 9th of March 1851.

H. C. Ørsted's scientific life had been a happy one, happy above all because it opened up quite new fields to science, happy, too, because of the enthusiasm and belief in its worth and significance with which it had been lived from the first days of youth to late old age.

Ørsted was prolific in scientific ideas. His receptive mind assimilated the theories, ways of reasoning and experimental methods of his time and gave birth to new scientific results. In his youth he tried to find general laws for various phenomena by deduction. In the chief work of his manhood he built up the result he had dimly foreseen, inductivily, through experiments. In the second great work of his manhood, on the compressibility of liquids, he employed the same method and threw out suggestions for future enquiry. In the philosophical works of his old age he tried to apply scientific working methods in domains associated with his assiduous work in the service of his countrymen. This work, which engrossed the greater part of his time and thought during his whole life, will be described in a subsequent paper.